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STRESSES IN BEAMS.

By Prof. DeVOLSON WOOD.

Contributed to Van Nostrand's Engineering Magazine.

Mr. D. H. Clark, in his Manual of Rules, Tables and Data, makes a statement in regard to the "Theory of Flexure," which, in our opinion, ought not to go unnoticed. He is an authority of such high character that his statements on such problems are liable to be accepted without being questioned; and hence it is the more important that any error should be corrected. Assuming that many of our readers have not the work conveniently at hand, we make the following lengthy extract from pp. 504, 505, from the work above referred to, that the reader may the more conveniently get that author's argument and statement as presnted by himself:

"These, the normal stresses or resistances due to the absolute horizontal compression and extension of the beam are supplemented by diagonal resistances by which each of them is augmented 75 per cent.

"To elucidate the origin of diagonal resistance, it may be observed that the upper and lower portions of a beam—above and below the neutral axis—may be considered as two individual members of a frame united at their surface of contact—the neutral one. Let cBd, Fig. 1, be a triangular frame fixed at ca and loaded at B. The pieces cc'' and dd'' of the upper and lower members are respectively extended and compressed, when the load is applied to the length

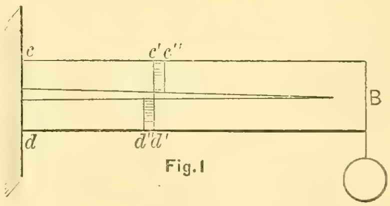


Fig. 1

cc'' and $dd.''$ If the members of the frame are placed parallel to each other in close contact, as in Fig 2, extension

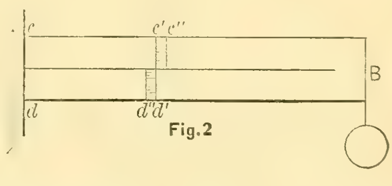


Fig. 2

and compression take place as before. Let now the two members be united in the line nop, Fig. 3, and so consolidated as to form a semi-beam; the extension and the compression partially neutralize each other: at the neutral line nop, they are absolutely neutralized and the amounts of extension and of compression are represented by the triangles $c'c''\ o$

and $d'd''$ o. The structure is thus, in a certain sense, crippled, and the extension

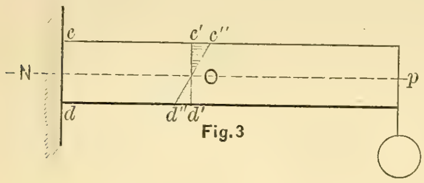


Fig. 3

and compression, instead of being rectilineal, are curvilineal, and the semi-beam is deflected, as in Fig. 4. The counteraction here pointed out is necessarily ex-

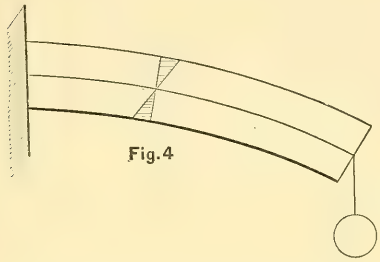


Fig. 4

erted diagonally, at an angle of 45° with the neutral line; as in the line $c'o'$, Fig. 5, with the transverse section $c'd'$.

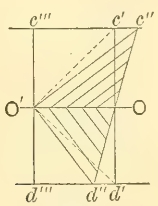


Fig. 5

"In the intervention of the diagonal stress in deflected rectangular beams, according to the analysis in the text, is found the solution of the mystery of the 'resistance of flecture,' which has been so denominated, and has been experimentally demonstrated by Mr. W H. Barlow. The contrast between the action of the diagonal bracing of lattice girders, and that of the solid web of web girders, throws a flood of light on the recondite strains in webs and in solid beams."

The assertion that the "diagonal resistances' augments the absolute horizontal compressions and extensions 75 per cent., is entirely gratuitous. No argument is presented to support it. Every student of the flexure of beams knows wherein the difficulty lies. The computed value of the stress on the most remote elements of a beam at the instant of rupture, does not agree either with the tensile or compressive strength. If the tensile strength T of cast iron be 15,000 pounds, per square inch, and the crushing strength C be 100,000 pounds, it will be found that the modulus of rupture R for beams will be some intermediate value, say, about 35,000 pounds per square inch. To account for this discrepancy, Barlow proposed his "resistance of flexure" and made experiments upon beams of various forms, and with different materials, and many of the results of which seemed to substantiate his theory. But certain elements of his theory are in direct conflict with well-established principles of statics, and it is certainly unsafe to hold to any theory, however plausible it may appear by itself, which violates other known laws. We will indicate these points.

He *assumed* that the shearing stress was uniformly distributed over the cross-section; but for homogeneous materials, the theory shows that it is greatest at the neutral axis and zero at the upper and lower elements, as shown in the writer's *Resistance of Materials*, article 179, where also the specific law for rectangular beams is deduced. In the light of this fact it cannot be claimed that the shearing stress will be uniform over the cross-section.

Again, it is well known in statics that a longitudinal shear is accompanied by an equal transverse shear at any point; just as one couple necessitates an opposite couple of equivalent moment to produce equilibrium. In case, then, there is no transverse shear there will be no longitudinal shear. If a beam be supported at its ends and loaded with two equal weights equidistant from the supports, there will be no transverse shear between the loaded points, for the algebraic sum of the loads on the middle portion will be zero. But Barlow *assumed* that shear exists wherever there is bending, and hence, in this case, is in direct violation of established principles. We are not

aware that any experiments have been made under these conditions for the purpose of testing theory.

Again, he *assumed* that the ultimate "resistance of flexure" varied directly as the ultimate stress on the remote elements. According to his experiments, it nearly equals t (the tenacity) for cast iron, and about $\frac{1}{2}t$ for wrought iron. Also that this resistance was due to the difference in the elongation of contiguous fibers. But these principles appear to be in conflict since the value of t is absolute, and a deep beam will not be bent so much at the point of rupture as a more shallow one, and hence the relative elongations will be less in the former than in the latter case.

The remark of Mr. Clark, in the last sentence above quoted, does not remove the difficulty. Even admitting that the combined transverse shear and longitudinal pull produces an oblique stress, it does not follow that the latter will pass in a right line from the surface to the neutral axis, as illustrated in Fig. 5; and even if it did, we fail to find how it could reinforce the tensile resistance 75 per cent. Every computer of a truss bridge assumes that when the stress on the lower chord exceeds its tensile strength, the bridge will fail. He does not presume to add anything to the strength of the chords on account of the diagonal bracing. Again, admitting that the longitudinal pull and transverse shear produces a diagonal resultant, it follows that the direction of the resultant will be nearly parallel to the surface of the beam at the outer elements, since the transverse shear is very small in that vicinity, and the direct pull the greatest, and will be the diagonal of a square only on the neutral axis, where the only forces are equal rectangular shears.

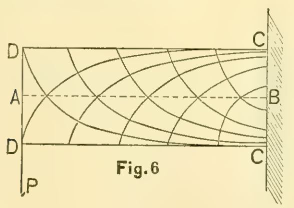


Fig.6

The direction of the resultant stresses will be tangent to the lines in Fig. 6 at any point.

But a closer examination of Barlow's theory fails to indicate how the diagonal stresses are produced in accordance with it. Barlow seemed to consider that his "Resistance to Flexure" was caused by a *longitudinal* force, or shear, acting between contiguous elements unequally elongated, and if this view be correct, it is not easy to see how a diagonal resultant can be found, much less the grounds for the assertion that "the counteraction here pointed out is necessarily exerted diagonally at an angle of 45° with the neutral line."

But it still remains that the computed stress on the remote elements of a beam at rupture exceeds the tenacity of the material. The reason why the strength of a beam exceeds its computed strength when T, the modulus of tenacity, is used in place of the tabulated value of R in the formula for the strength of a beam, is due to the fact that the hypotheses upon which the analysis is founded are not realized in the experiment. The laws for elastic resistance are extended to that of ultimate strength, although it is well known that the laws fail when the elastic limit is passed. The outer fibers in the latter condition become relatively weakened, so that the effective depth—or more strictly, the depth which would represent the strength according to the assumed laws, is less than the measured depth of the beam. Thus, if T be the modulus of tenacity, its value for a rectangular beam supported at its ends, and broken by a load P at the middle, should be found by the formula

$$T=\frac{3\ Pb}{2bd^2},$$

in which, if a value for d less than that of the measured depth be used, the resulting value will exceed T, as is actually found in practice. It is possible that this way of stating the case is original.

There is another reason for the discrepancy. It is assumed that the section of rupture is a plane—a condition never realized. The section of rupture being irregular, more elements must be separated than if it were plane, a condition which might give an apparent increase of strength.

It thus appears that the cause of the discrepancy between computed and observed values may be fully accounted for without involving hypotheses in conflict with other known principles.

SCIENCE AND ENGINEERING.

From "Nature."

In the address delivered by Mr. Westmacott, President of the Institution of Mechanical Engineers, to the English and Belgian engineers assembled at Liége last August, there occurred the following passage: "Engineering brings all other sciences into play; chemical or physical discoveries, such as those of Faraday, would be of little practical use if engineers were not ready with mechanical appliances to carry them out and make them commercially successful in the way best suited to each."

We have no objection to make to these words, spoken at such a time, and before such an assembly. It would, of course, be easy to take the converse view, and observe that engineering would have made little progress in modern times but for the splendid resources which the discoveries of pure science have placed at her disposal, and which she has only had to adopt and utilize for her own purposes. But there is no need to quarrel over two opposite modes of stating the same fact. There *is* need, on the other hand, that the fact itself should be fairly recognized and accepted, namely, that science may be looked upon as at once the handmaid and the guide of art, art as at once the pupil and the supporter of science. In the present article we propose to give a few illustrations which will bring out and emphasise this truth.

We could scarcely find a better instance than is furnished to our hand in the sentence we have chosen for a text. No man ever worked with a more single-hearted devotion to pure science—with a more absolute disregard of money or fame, as compared with knowledge—than Michael Faraday. Yet future ages will perhaps judge that no stronger impulse was ever given to the progress of industrial art, or to the advancement of the material interests of mankind, than the impulse which sprang from his discoveries in electricity and magnetism. Of these discoveries we are only now beginning to reap the benefit. But we have merely to consider the position which the dynamo-electric machine already occupies in the industrial world, and the far higher position which, as almost all admit, it is destined to occupy in the future, in order to see how much we owe to Faraday's establishment of the connection between magnetism and electricity. That is one side of the question—the debt which art owes to science. But let us look at the other side also. Does science owe nothing to art? Will any one say that we should know as much as we do concerning the theory of the dynamo-electric motor, and the laws of electro-magnetic action generally, if that motor had never risen (or fallen, as you choose to put it) to be something besides the instrument of a laboratory or the toy of a lecture-room. Only a short time since the illustrious French physicist, M. Tresca, was enumerating the various sources of loss in the transmission of power by electricity along a fixed wire, as elucidated in the careful and elaborate experiments inaugurated by M. Marcel Deprez, and subsequently continued by himself. These losses—the electrical no less than the mechanical losses—are being thoroughly and minutely examined in the hope of reducing them to the lowest limit; and this examination cannot fail to throw much light on the exact distribution of the energy imparted to a dynamo machine, and the laws by which this distribution is governed. But would this examination ever have taken place—would the costly experiments which render it feasible ever have been performed—if the dynamo machine was still under the undisputed control of pure science, and had not become subject to the sway of the capitalist and the engineer?

Of course the electric telegraph affords an earlier and perhaps as good an illustration of the same fact. The discovery that electricity would pass along a wire and actuate a needle at the other end was at first a purely scientific one, and it was only gradually that its importance, from an industrial point of view, came to be recognized. Here, again, art

owes to pure science the creation of a complete and important branch of engineering, whose works are spread like a net over the whole face of the globe. On the other hand, our knowledge of electricity, and specially of the electrochemical processes which go on in the working of batteries, has been enormously improved in consequence of the use of such batteries for the purposes of telegraphy.

Let us turn to another example in a different branch of science. Whichever of our modern discoveries we may consider to be the most startling and important, there can, I think, be no doubt that the most beautiful is that of the spectroscope. It has enabled us to do that which, but a few years before its introduction, was taken for the very type of the impossible, viz., to study the chemical composition of the stars; and it is giving us clearer and clearer insight every day into the condition of the great luminary which forms the center of our system. Still, however beautiful and interesting such results may be, it might well be thought that they could never have any practical application, and that the spectroscope, at least, would remain an instrument of science, but of science alone. This, however, is not the case. Some thirty years since Mr. Bessemer conceived the idea that the injurious constituents of raw iron, such as silicon, sulphur, &c., might be got rid of by simple oxydation. The mass of crude metal was heated to a very high temperature; atmospheric air was forced through it at considerable pressure, and the oxygen, uniting with these metalloids, carried them off in the form of acid gases. The very act of union generated a vast quantity of heat, which itself assisted the continuance of the process; and the gas, therefore, passed off in a highly luminous condition. But the important point was to know where to stop; to seize the exact moment when all, or practically all, hurtful ingredients had been removed, and before the oxygen had turned from them to attack the iron itself. How was this point to be ascertained? It was soon suggested that each of these gases in its incandescent state would show its own peculiar spectrum; and that if the flame rushing out of the throat of the converter were viewed through a spectroscope, the moment when any substance, such as sulphur, had disappeared would be known by the disappearance of the corresponding lines in the spectrum. The anticipation, it is needless to say, was verified; and the spectroscope, though now superseded, had for a time its place among the regular appliances necessary for the carrying on of the Bessemer process.

This process itself, with all the momentous consequences, mechanical, commercial and economical, which it has entailed, might be brought forward as a witness on our side; for it was almost completetely worked out in the laboratory before being submitted to actual practice. In this respect it stands in marked contrast to the earlier processes for the making of iron and steel, which were developed, it is difficult to say how, in the forge or furnace itself, and amid the smoke and din of practical work. At the same time the experiments of Bessemer were for the most part carried out with a distinct eye to their future application in practice, and the value for our present purpose is therefore not so great. The same, we believe, may be said with regard to the great rival of the Bessemer converter, viz., the Siemens open hearth; although this forms in itself a beautiful application of the scientific doctrine that steel stands midway as regards its proportion of carbon, between wrought iron and pig iron, and ought, therefore to be obtainable by a judicious mixture of the two. The basic process is the latest development, in this direction, of science as applied to metallurgy. Here, by simply giving a different chemical constitution to the clay lining of the converter, it is found possible to eliminate phosphorus—an element which has successfully withstood the attack of the Bessemer system. Now, to quote the words of a German eulogizer of the new method, phosphorus has been turned from an enemy into a friend, and the richer a given ore is in that substance, the more readily and cheaply does it seem likely to be converted into steel.

These latter examples have been taken from the art of metallurgy; and it may of course be said that, considering the intimate relation between that art and the science of chemistry, there can be no wonder if the former is largely depend-

ent for its progress upon the latter. I will therefore turn to what may appear the most concrete, practical and unscientific of all arts—that, namely, of the mechanical engineer; and we shall find that even here examples will not fail us of the boons which pure science has conferred upon the art of construction, nor even, perhaps, of the reciprocal advantages which she has derived from the connection.

The address of Mr. Westmacott, from which I have already taken my text, supplies in itself more than one instance of the kind we seek—instances emphasized by papers read at the meeting where the address was spoken. Let us take, first, the manufacture of sugar from beet-root. This manufacture was forced into prominence in the early years of this century, when the continental blockade maintained by England against Napoleon prevented all importation of sugar from America; and it has now attained very large dimensions, as all frequenters of the Continent must be aware. The process, as exhaustively described by a Belgian engineer, M. Mélin, offers several instances of the application of chemical and physical science to practical purposes. Thus, the first operation in making sugar from beet-root is to separate the juice from the flesh, the former being as much as 95 per cent. of the whole weight. Formerly this was accomplished by rasping the roots into a pulp, and then pressing the pulp in powerful hydraulic presses; in other words, by purely mechanical means. This process is now to a large extent superseded by what is called the diffusion process, depending on the well-known physical phenomena of *endosmosis* and *exosmosis*. The beet-root is cut up into small slices, called "cossettes," and these are placed in vessels filled with water. The result is, that a current of endosmosis takes place from the water towards the juice in the cells, and a current of exosmosis from the juice towards the water. These currents go on cell by cell, and continue until a state of equilibrium is attained. The richer the water and the poorer the juice, the sooner does this equilibrium take place. Consequently the vessels are arranged in a series, forming what is called a diffusion battery; the pure water is admitted to the first vessel, in which the slices have already been nearly exhausted, and subtracts from them what juice there is left. It then passes as a thin juice to the next vessel, in which the slices are richer, and the process begins again. In the last vessel the water which has already done its work in all the previous vessels, comes into contact with fresh slices, and begins the operation upon them. The same process has been applied at the other end of the manufacture of sugar. After the juice has been purified, and all the crystallizable sugar has been separated from it by boiling, there is left a mass of molasses containing so much of the salts of potassium and sodium that no further crystallization of the yet remaining sugar is possible. The object of the process called osmosis is to carry off those salts. The apparatus used, or osmogene, consists of a series of trays filled olternately with molasses and water, the bottoms being formed of parchment paper. A current passes through this paper in each direction, part of the water entering the molasses, and part of the salts, together with a certain quantity of sugar, entering the water. The result of thus freeing the molasses from the salts is that a large part of the remaining sugar can now be extracted by crystallization.

Another instance in point comes from a paper dealing with the question of the construction of long tunnels. In England this has been chiefly discussed of late in connection with the Channel Tunnel, where, however, the conditions are comparatively simple. It is of still greater importance abroad. Two tunnels have already been pierced through the Alps; a third is nearly completed, and a fourth, the Simplon Tunnel, which will be the longest of any, is at this moment the subject of a most active study on the part of French engineers. In America, especially in connection with the deep mines of the Western States, the problem is also of the highest importance. But the driving of such tunnels would be financially if not physically impossible, but for the resources which science has placed in our hands, first, by the preparation of new explosives, and, secondly, by methods of dealing with the very high temperatures which have to be encountered. As regards the first, the history of explosives is scarcely anything else

than a record of the application of chemical principles to practical purposes—a record which, in great part has yet to be written, and on which we cannot here dwell. It is certain, however, that but for the invention of nitroglycerine, a purely chemical compound, and its development in various forms, more or less safe and convenient, these long tunnels would never have been constructed. As regards the second point, the question of temperature is really the most formidable with which the tunnel engineer has to contend. In the St. Gothard Tunnel, just before the meeting of the two headings, in February, 1880, the temperature rose as high as 93° Fahr. This, combined with the foulness of the air, produced an immense diminution in the work done per person and per horse employed whilst several men were actually killed by the dynamite gases, and others suffered from a disease which was traced to a hitherto unknown species of internal worm. If the Simplon Tunnel should be constructed yet higher temperatures may probably have to be dealt with. Although science can hardly be said to have completely mastered these difficulties, much has been done in that direction. A great deal of mechanical work has, of course, to be carried on at the face or far end of such a heading, and there are various means by which it might be done. But by far the most satisfactory solution, in most cases at least, is obtained by taking advantage of the properties of compressed air. Air can be compressed at the end of the tunnel either by steam-engines, or, still better, by turbines where water power is available. This compressed air may easily be led in pipes to the face of the heading, and used there to drive the small engines which work the rock-drilling machines, &c. The efficiency of such machines is doubtless low, chiefly owing to the physical fact that the air is heated by compression, and that much of this heat is lost whilst it traverses the long line of pipes leading to the scene of action. But here we have a great advantage from the point of view of ventilation ; for, as the air gained heat while being compressed, so it loses heat while expanding; and the result is that a current of cold and fresh air is continually issuing from the machines at the face of the heading, just where it is most wanted. In consequence, in the St. Gothard, as just alluded to, the hottest parts were always some little distance behind the face of the heading. Although in this case as much as 120,000 cubic meters of air (taken at atmospheric pressure) were daily poured into the heading, yet the ventilation was very insufficient. Moreover, the high pressure which is used for working the machines is not the best adapted for ventilation; and in the Arlberg tunnel separate ventilating pipes are employed, containing air compressed to about one atmosphere, which is delivered in much larger quantities, although not at so low a temperature. In connection with this question of ventilation a long series of observations have been taken at the St. Gothard, both during and since the construction; these have revealed the important physical fact (itself of high practical importance) that the barometer never stands at the same level on the two sides of a great mountain chain; and so have made valuable contributions to the science of meteorology.

Another most important use of the same scientific fact, namely, the properties of compressed air, is found in the sinking of foundations below water. When the piers of a bridge, or other structure, had to be placed in a deep stream, the old method was to drive a double row of piles round the place and fill them in with clay, forming what is called a cofferdam. The water was pumped out from the interior, and the foundation laid in the open. This is always a very expensive process, and in rapid streams is scarcely practicable. In recent times large bottomless cases, called caissons, have been used, with tubes attached to the roof, by which air can be forced into or out of the interior. These caissons are brought to the site of the proposed pier, and are there sunk. Where the bottom is loose, sandy earth, the Vacuum process, as it is termed, is often employed; that is, the air is pumped out from the interior, and the superincumbent pressure then causes the caisson to sink and the earth to rise within it. But it is more usual to employ what is called the Plenum process, in which air under high pressure is pumped into the caisson and expels the water, as in a diving bell. Workmen

then descend, entering through an air lock, and excavate the ground at the bottom of the caisson, which sinks gradually as the excavation continues. Under this system a length of some two miles of quay wall is being constructed at Antwerp, far out in the channel of the river Scheldt. Here the caissons are laid end to end with each other, along the whole curve of the wall, and the masonry is built on the top of them within a floating cofferdam of very ingenious construction.

There are few mechanical principles more widely known than that of so-called centrifugal force; an action which, though still a puzzle to students, has long been thoroughly understood. It is, however, comparatively recently that it has been applied in practice. One of the earliest examples was, perhaps, the ordinary governor, due to the genius of Watt. Every boy knows that if he takes a weight hanging from a string and twirls it round, the weight will rise higher and revolve in a larger circle as he increases the speed. Watt saw that if he attached such an apparatus to his steam engine, the balls or weights would tend to rise higher whenever the engine began to run faster, that this action might be made partly to draw over the valve which admitted the steam, and that in this way the supply of steam would be lessened, and the speed would fall. Few ideas in science have received so wide and so successful an application as this; but of late years another property of centrifugal force has been brought into play. The effect of this so-called force is that any body revolving in a circle has a continual tendency to fly off at a tangent; the amount of this tendency depending jointly on the mass of the body and on the velocity of the rotation. It is the former of these conditions which is now being taken advantage of. For if we have a number of particles all revolving with the same velocity, but of different specific gravities, and if we allow them to follow their tendency of moving off at a tangent, it is evident that the heaviest particles having the greatest mass will move with the greatest energy. The result is, that if we take a mass of such particles and confine them within a circular casing, we shall find that, having rotated this casing with a high velocity and for a sufficient time, the heaviest particles will have settled at the outside and the lightest at the inside, whilst between the two there will be a gradation from the one to the other. Here, then, we have the means of separating two substances, solid or liquid, which are intimately mixed up together, but which are of different specific gravities. This physical principle has been taken advantage of in a somewhat homely but very important process, viz., the separation of cream from milk. In this arrangement the milk is charged into a vessel something of the shape and size of a Gloucester cheese, which stands on a vertical spindle, and is made to rotate with a velocity as high as 7,000 revolutions per minute. At this enormous speed the milk, which is the heavier, flies to the outside, while the cream remains behind and stands up as a thin layer on the inside of the rotating cylinder of fluid. So completely does this immense speed produce in the liquid the characteristics of a solid, that if the rotating shell of cream be touched by a knife it emits a harsh, grating sound, and gives the sensation experienced in attempting to cut a stone. The separation is almost immediately complete, but the difficult point was to draw off the two liquids separately and continuously without stopping the machine. This has been simply accomplished by taking advantage of another principle of hydromechanics. A small pipe opening just inside the shell of the cylinder is brought back to near the center, where it rises through a sort of neck and opens into an exterior casing. The pressure due to the velocity causes the skim-milk to rise in this pipe and flow continuously out at the inner end. The cream is at the same time drawn off by a similar orifice made in the same neck and leading into a different chamber.

Centrifugal action is not the only way in which particles of different specific gravity can be separated from each other by motion only. If a rapid "jigging" or up-and-down motion be given to a mixture of such particles, the tendency of the lighter to fly further under the action of the impulse causes them gradually to rise to the upper surface; this surface being free in the present case, and the result being therefore the reverse of what happens in the rotating

chamber. If such a mixture be examined after this up-and-down motion has gone on for a considerable period, it will be found that the particles are arranged pretty accurately in layers, the lightest being at the top and the heaviest at the bottom. This principle has long been taken advantage of in such cases as the separation of lead ores from the matrix in which they are embedded. The rock, in these cases, is crushed into small fragments, and placed on a frame having a rapid up and-down motion, when the heavy lead ore gradually collects at the bottom and the lighter stone on the top. To separate the two the machine must be stopped and cleared by hand. In the case of coal-washing, where the object is to separate fine coal from the particles of stone mixed with it, this process would be very costly, and indeed impossible, because a current of water is sweeping through the whole mass. In the case of the Coppée coal-washer, the desired end is achieved in a different and very simple manner. The well-known mineral feldspar has a scientific gravity intermediate between that of the coal and the shale, or stone, with which it is found intermixed. If, then, a quantity of feldspar in small fragments is thrown into the mixture, and the whole then submited to the jigging process, the result will be that the stone will collect on the top and the coal at the bottom, with a layer of feldspar separating the two. A current of water sweeps through the whole, and is drawn off partly at the top, carrying with it the stone, and partly at the bottom, carrying with it the fine coal.

The above are instances where science has come to the aid of engineering. Here is one in which the obligation is reversed. The rapid stopping of railway trains, when necessary, by means of brakes, is a problem which has long occupied the attention of many engineers; and the mechanical solutions offered have been correspondingly numerous. Some of these depend on the action of steam, some of a vacuum, some of compressed air, some of pressure water; others again ingeniously utilize the momentum of the wheels themselves. But for a long time no effort was made by any of these inventors thoroughly to master the theoretical conditions of the problem before them. At last, one of the most ingenious and successful among them, Mr. George Westinghouse, resolved to make experiments on the subject, and was fortunate enough to associate with himself Captain Douglas Galton. Their experiments, carried on with rare energy and perseverance, and at great expense, not only brought into the clearest light the physical conditions of the question (conditions which were shown to be in strict accordance with theory), but also disclosed the interesting scientific fact that the friction between solid bodies at high velocities is not constant, as the experiments of Morin had been supposed to imply, but diminishes rapidly as the speed increases —a fact which other observations serve to confirm.

The old scientific principle known as the hydrostatic paradox, according to which a pressure applied at any point of an inclosed mass of liquid is transmitted unaltered to every other point, has been singularly fruitful in practical applications. Mr. Bramah was perhaps the first to recognize its value and importance. He applied it to the well-known Bramah press, and in various other directions, some of which were less successful. One of these was a hydraulic lift, which Mr. Bramah proposed to construct by means of several cylinders sliding within each other after the manner of the tubes of a telescope. His specification of this invention sufficiently expresses his opinion of its value, for it concludes as follows: "This patent does not only differ in its nature and in its boundless extent of claims to novelty, but also in its claims to merit and superior utility compared with any other patent ever brought before or sanctioned by the legislative authority of any nation." The telescope lift has not come into practical use; but lifts worked on the hydraulic principle are becoming more and more common every day. The same principle has been applied by the genius of Sir William Armstrong, and others, to the working of cranes and other machines for the lifting of weights, &c.; and, under the form of the accumulator, with its distributing pipes and hydraulic engines, it provides a store of power always ready for application at any required point in a large system, yet costing practically nothing when not actu-

ally at work. This system of high-pressure mains worked from a central accumulator has been for some years in existence at Hull, as a means of supplying power commercially for all the purposes needed in a large town, and it is at this moment being carried out on a wider scale in the East End of London.

Taking advantage of this system, and combining with it another scientific principle of wide applicability, Mr. J. H. Greathead has brought out an instrument called the "injector hydrant," which seems likely to play an important part in the extinguishing of fires. This second principle is that of the lateral induction of fluids, and may be thus expressed in the words of the late William Froude: "Any surface which, in passing through a fluid, experiences resistance, must, in so doing, impress on the particles which resist it a force in the line of motion equal to the resistance." If, then, these particles are themselves part of a fluid, it will result that they will follow the direction of the moving fluid and be partly carried along with it. As applied in the injector hydrant, a small quantity of water derived from the high-pressure mains is made to pass from one pipe into another, coming in contact at the same time with a reservoir of water at ordinary pressure. The result is, that the water from the reservoir is drawn into the second pipe through a trumpet-shaped nozzle, and may be made to issue as a stream to a considerable height. Thus, the small quantity of pressure-water, which, if used by itself, would perhaps rise to a height of 500 feet, is made to carry with it a much larger quantity to a much smaller height, say that of an ordinary house.

The above are only a few of the many instances which might be given to prove the general truth of the fact with which we started, namely, the close and reciprocal connection between physical science and mechanical engineering, taking both in their widest sense. It may possibly be worth while to return again to the subject, as other illustrations arise. Two such have appeared, even at the moment of writing, and though their practical success is not yet assured, it may be worth while to cite them. The first is an application of the old principle of the siphon to the purifying of sewage. Into a tank containing the sewage dip a siphon pipe some thirty feet high, of which the shorter leg is many times larger than the longer. When this is started, the water rises slowly and steadily in the shorter column, and before it reaches the top has left behind it all, or almost all, of the solid particles which it previously held in suspension. These fall slowly back through the column, and collect at the bottom of the tank, to be cleared out when needful. The effluent water is not, of course, chemically pure, but sufficiently so to be turned into any ordinary stream. The second invention rests on a curious fact in chemistry, namely, that caustic soda or potash will absorb steam, forming a compound which has a much higher temperature than the steam absorbed. If, therefore, exhaust steam be discharged into the bottom of a vessel containing caustic alkali, not only will it become condensed, but this condensation will raise the temperature of the mass so high that it may be employed in the generation of fresh steam. It is needless to observe how important will be the bearing of this invention upon the working of steam engines for many purposes, if only it can be established as a practical success. And if it is so established, there can be no doubt that the experience thus acquired will reveal new and valuable facts with regard to the conditions of chemical combination and absorption, in the elements thus brought together.

An experiment on the loss of pressure of steam in a long steam pipe was lately made in the Gould and Curry mine, California, by Mr. R. G. Carlyle, engineer. The pipes used were four-inch gas pipes connected with flanges and placed in a long trough made of twelve-inch by two-inch planks, and thus eight inches square inside. The space between pipe and wood was filled up with wood. Although the pipe was one thousand three hundred and forty-one feet in length there was practically no difference between the pressure at the boiler and at the engine, and this was proved by several gauges and mercury columns. Mr. Carlyle concludes that there is no limit to the distance to which steam may be carried.

PRESSURE ATTAINABLE BY THE USE OF THE "DROP PRESS."

By ROBERT H. THURSTON, HOBOKEN, N. J.

The writer has recently taken occasion to determine the magnitude of the pressures attainable and not infrequently utilized in the use of the "drop press," now so extensively employed in the process of "drop forging," and in the manufacture of small parts of sewing machines, firearms and light machinery.

The opportunity was offered to make this determination in the course of an investigation of the efficiency of drop presses lately made by the Mechanical Laboratory of the Stevens Institute of Technology. It was found that the most efficient presses experimented with had an "efficiency," as the term is technically used, of 90 per cent.—*i. e.*, the work done by the drop was 90 per cent. of that which was due to the weight falling through the measured height. The table given below is based upon the assumption that this efficiency can be reached, and exhibits the mean pressure attained when the piece attacked is crushed to the amount of $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$ inch respectively. The maximum pressures must exceed those given. The mean pressures are calculated by determining the amount of energy of the falling drop at the instant before stopping—*i. e.*, of the quantity of work done upon it by gravity and stored in it, and dividing that measure in foot-pounds by the distance through which the crushing of the "work" takes place. These figures are seen to be simply enormous, and the power of this form of press is evidently limited only by the rigidity of its parts and their strength.

The figures given for the pressures reached when the compression is $\frac{1}{16}$ inch can only be obtained when the anvil is so set and of such material that the yielding there occurring cannot absorb more than the allowed 10 per cent. of the total work of the falling mass. The same remark applies to the table generally, but the loss may always be expected to fall within the assumed figure for the smaller weights and lesser heights fallen through. If the machine is well built and the anvil and foundation are of ample size and rigidity for good work, it is not improbable that the higher figures can be readily obtained; also, if proper precautions are taken in the setting of the press.

The *intensity* of pressure attainable is evidently determined by the area of the surface exposed to the action of the drop, and this in turn determines the distance through which crushing may occur. The figures given in the table are total pressures, and the mean intensity of pressure corresponding to these amounts is to be obtained by dividing the total pressure as shown in the table by the area of section of the crushed piece, or by the mean area opposed to the crushing action during the operation. The proper comparison is that of the energy of the falling weight with the "resilience," elastic or total, or both, of the mass on the anvil or in the dies.

The limit to the resistance of any mass on the anvil is found at the pressure at which the metal will "flow" continuously. This pressure varies with not only the kind of metal, but with every variation in the chemical composition, the physical structure or the form and method of support of the piece. For general use the value of this "modulus" may be taken at about the value of the shearing resistance of the material. For soft wrought iron, for example, it may be taken at about 50,000 pounds per square inch (3,515 kg. per sq. cm.) for moderately hard iron at a figure 20 per cent. higher, and for pure copper at about one-half the latter figure. There is, however, a great difference in the behavior of the two metals under pressure. The former has a distinct elastic limit in its original state, which becomes "exalted," as was shown by the writer some ten years ago, when the piece is distorted, and becomes approximately equal to the maximum force, producing change of form, remaining permanently altered. The metal thus transformed does not yield subse-

PRESSURES OF THE DROP PRESS.—(EFFICIENCY 90 PER CENT.)

Fall of drop in feet and inches.	Compression in inches.	Weights of Drop.								
		Pounds. 50.	Pounds. 100.	Pounds. 200.	Pounds. 400.	Pounds. 600.	Pounds. 800.	Pounds. 1,000.	Pounds. 1,500.	Pounds. 2,000.
Ft. Ins.	Ins.									
0 3	$\frac{1}{16}$	2,160	4,320	8,640	17,281	25,921	34,562	43,202	64,803	86,405
	$\frac{1}{8}$	1,080	2,160	4,320	8,640	12,960	17,281	21,601	32,401	43,202
	$\frac{1}{4}$	540	1,080	2,160	4,320	6,480	8,640	10,800	16,200	21,601
0 6	$\frac{1}{16}$	4,320	8,640	17,281	34,562	51,843	69,124	86,405	129,607	172,811
	$\frac{1}{8}$	2,160	4,320	8,640	17,281	25,921	34,562	43,202	64,803	86,405
	$\frac{1}{4}$	1,080	2,160	4,320	8,640	12,960	17,281	21,601	32,402	43,202
0 9	$\frac{1}{16}$	6,480	12,960	25,921	51,843	77,764	103,686	129,608	194,412	259,216
	$\frac{1}{8}$	3,240	6,480	12,960	25,921	38,882	51,843	64,804	87,206	129,608
	$\frac{1}{4}$	1,620	3,240	6,480	12,960	19,441	25,921	32,402	43,603	64,804
1 0	$\frac{1}{16}$	8,640	17,281	34,562	69,124	103,687	138,249	172,811	259,216	345,622
	$\frac{1}{8}$	4,320	8,640	17,281	34,562	51,843	69,124	86,405	129,608	172,811
	$\frac{1}{4}$	2,160	4,320	8,640	17,281	25,922	34,562	43,202	64,804	86,405
1 6	$\frac{1}{16}$	12,960	25,921	51,843	103,686	155,529	207,373	259,216	388,824	518,432
	$\frac{1}{8}$	6,480	12,960	25,921	51,843	77,764	103,686	139,608	194,412	259,216
	$\frac{1}{4}$	3,240	6,480	12,960	25,921	38,882	51,843	64,804	87,206	129,608
2 0	$\frac{1}{16}$	17,281	34,562	69,124	138,249	207,373	276,498	345,622	518,433	691,244
	$\frac{1}{8}$	8,640	17,281	34,562	69,124	103,686	138,249	172,811	259,216	345,622
	$\frac{1}{4}$	4,320	8,640	17,281	34,562	51,843	69,124	86,405	129,608	172,811
2 6	$\frac{1}{16}$	21,601	43,202	86,405	172,811	259,216	345,622	432,028	648,042	864,056
	$\frac{1}{8}$	10,800	21,601	43,202	86,405	129,608	172,811	216,014	324,021	432,028
	$\frac{1}{4}$	5,400	10,800	21,601	43,202	64,804	86,405	180,007	162,010	216,014
3 0	$\frac{1}{16}$	....	51,843	103,686	207,373	311,059	414,747	518,433	777,649	1,036,866
	$\frac{1}{8}$	....	25,921	51,843	103,686	155,529	207,373	259,216	388,824	518,433
	$\frac{1}{4}$	.....	12,960	25,921	51,843	77,764	103,686	129,608	194,412	259,216
3 6	$\frac{1}{16}$	....	....	120,967	241,935	362,902	483,871	604,838	916,257	1,209,677
	$\frac{1}{8}$	....	....	60,483	120,967	181,951	241,935	302,419	458,128	604,838
	$\frac{1}{4}$	....	....	30,241	60,483	90,975	120,967	151,209	229,064	302,419
4 0	$\frac{1}{16}$	....	....	....	276,498	414,747	552,996	691,244	1,036,866	1,382,488
	$\frac{1}{8}$	....	....	....	138,249	207,373	276,498	345,622	518,433	691,244
	$\frac{1}{4}$	....	....	....	69,124	103,686	138,249	172,811	259,216	345,622
5 0	$\frac{1}{16}$	....	....	....	....	518,434	691,245	864,056	1,296,084	1,728,112
	$\frac{1}{8}$	....	....	....	....	259,217	345,622	432,028	648,042	864,056
	$\frac{1}{4}$	....	....	....	....	129,609	172,811	216,014	324,021	432,028

quently to any less pressure. It will not flow under any pressure less than that which is required to produce distortion immediately upon its application. Copper, however, has no true and measurable elastic limit in its original condition as found in the market, and it does flow under the continued action of forces far less than those required to produce rapid and continuous distortion by steady pressure. A load which produces no visible effect when first applied will after a time be found to have caused a very decided, and often a very extensive, alteration of the form of the mass. This is also a now well-known property of some kinds of brass and of many other metals belonging to what the writer has called the tin class, to distinguish them from the metals of the iron and steel class, which do not exhibit this treacherous behavior. This difference is of some

importance, not only as indicating the best method of working them, but also as showing that the first of these two classes is a safer class to deal with where the metal is to be used in the carrying of heavy and unintermitted stress than is the second class.

Another important distinction between these two classes is, as indicated by the results of investigations made by the writer, that the "iron class," which includes all the irons and all of the steels, offers more resistance as the rupturing action is slower, while the "tin class," which includes nearly all the other metals, and very nearly all of the alloys that the writer has ever tested, yields the more readily the more slowly the distortion goes on. The second class is thus subject to that singular kind of change of form under heavy, continuous stress which is illustrated in the movement of all viscous solids—ice, for example, as seen in its flow in the glacier. This, it seems probable, may often occur under pressures far within those which are required to cause change of form in the testing machine in the ordinary methods of test. Iron has been found by Vicat, and later by the writer, to exhibit something of such a phenomenon, but only when the pressures are considerably above one-half those usually found for the moduli of rupture, and this action is only seen in serious degree when the iron has been annealed and thus softened. Common merchant iron, so far as the writer is aware, does not show any tendency to such slow and imperceptible yielding under moderate loads.

The bearing of these facts upon the value of the drop press as a means of working iron and other metals into shape is obvious. Change of form can only begin when the elastic limit of the material is passed, and flow can only progress steadily and uninterruptedly when the pressure applied is in excess of the resistance of the metal to flow. The soft metals which belong to the "tin class" are best attacked by processes which cause a comparatively slow motion of their particles in changing form; iron and steel, on the contrary, being less resistant at high than at low velocities of flow, are best worked by methods which produce rapid distortion. Professor Kick, of Prague, has shown very plainly that this difference in the amount of work demanded by the soft metals under the two kinds of treatment may amount to a very important quantity. He finds that the distortion of bodies by the action of the hydraulic press and by the action of a hammer dealing a succession of blows to produce the same change of form consume power in the ratio, in some cases, of one to ten. It is thus evident that the hammer or the drop is to be used for those special cases in which the pressure desired cannot be reached by ordinary methods, and that it is best adapted to the working of iron and steel. The hydraulic press and automatic machinery are to be preferred where they can be conveniently and cheaply used. For much of the work that is now done in our smaller kinds of manufacturing, the drop has been shown by experience to be the only machine which will give the required enormous pressures and do the work rapidly and cheaply.

The maximum area of surface exposed to pressure which will be allowable for any given amount of compression can be determined approximately by dividing the total mean pressure due to the action of the drop, with the given fall, and the proposed compression by the maximum resistance of the material. The maximum area which will permit any action upon the surface is to be ascertained by dividing the same maximum pressure due the fall of the drop by the elastic limit of the metal in compression.

The total work absorbed, or the resilience of the mass, up to the elastic limit is to be measured by multiplying the elastic resistance by one-half the percentage of compression which marks the elastic limit; the result measures the resilience in inch-pounds when the unit of measure is the inch, and in centimeter-kilograms when the units are metric. The total work done in any permanent change of shape is proportional to the volume affected and to the maximum resistance of the material to such deformation.

What figures shall be adopted for the resistance to be calculated upon in the production of flow in metals subjected to the action of the drop press is a question which the writer is unable to answer definitely. It would seem probable that

the effect of the blow may be, in the case of cold metal, somewhat similar to that of cold rolling, and, this being the case, the initial resistance to flow must be taken as at least 70,000 pounds per square inch 4,921 kg. per sq. cm.), and the resilience during flow at as high as 70,000 inch-pounds per cubic inch (4,921 kg. m. per cubic centimeter) for good common wrought iron. It may be safe to take the figure for hot iron, as usually worked, at less than one-half this amount. For copper the writer would, in the absence of exact data, take the work of deformation to be two-thirds that of iron for pieces of small section, and would expect a great increase of resistance with either metal when the surface acted upon by the drop becomes large in proportion to its thickness. Probably no very reliable figures can yet be given. Whatever the resistance may be, the drop will be very certain to overcome it, and the variation in its amount will simply determine how many blows must be struck to obtain a given amount of change of form.

HYDRAULIC ARCHITECTURE.

From "The Builder."

No portion of the duty of either the architect or the engineer is so difficult as that which relates to hydraulic constructions. There are many reasons for this, such as the great cost and trouble of obtaining foundations below water, the different behavior of various kinds of cement, and of other materials, in the air and in the water, the variation in the stability due to weight, from the buoyant action of the water, and other causes. But probably the chief difficulty lies in the absence of satisfactory theory. For all kinds of terrestrial structures, from the simplest form of cottage to the loftiest vault, burdened with impossible pendants, or soaring between flying buttresses and pinnacles, the definite mathematical law can be detected and expressed. It is true that the draughtsman rather leans on practice and experience than on geometric or algebraic analysis. But if the latter be wanted, in the case of any new and unprecedented construction, it is attainable; and within the last few years, as those columns of our own which are devoted to reviews bear ample witness, so much thought has been given to the subject of the graphic solution of structural problems that there is hardly a question which can be put to the architect of which he cannot work out the answer on the drawing board.

The case is far otherwise when water has to be taken into account. Instances of the disastrous failure of costly works for hydraulic purposes are not so rare as might be desired. Thus in May, 1847, the Government of India directed that the works on the Ganges Canal, designed by Sir Proby Cautley, should be vigorously carried out. Water was admitted into the canal in April, 1854, but during the next few years defects in the work came gradually to light, the chief of which was the "excessive declivity in the bed of the main channel, which caused a velocity of current greater than the sandy soil was calculated to withstand without erosion" (Buckley's "Irrigation Works of India," p. 101); yet the gradient was only 18 inches per mile. In 1863 Sir Arthur Cotton estimated the alterations and additions to this canal at £2,725,000; and proposed to reduce the inclination of the bed of the canal by from 3 inches to 6 inches per mile. But there is no doubt that Sir Proby Cautley had followed the formula of Dubuat, which at the time was, and indeed still is, an accepted authority for the setting-out of canals.

But we need not go to India for an illustration of the imperfect acquaintance with hydraulic law which is as yet characteristic of the state of science. As recently as 1864 the Mersey Board so far completed the works of the Low Water Basin at Birkenhead that water was run for the first time through the sluicing conduits. "But from some cause," says Mr. Ellacott, in his description of the

basin (Min. Proc. Inst. C. E., vol. xxviii., p. 525), "which was never clearly accounted for, such a violent shock was communicated to a portion of the masonry forming the back of the landing-stage recess, that the wall was instantly forced over 5 inches out of the perpendicular. Except for the purpose of sluicing under the landing-stage, no further attempt was made to run the water through the culvert. No trial was ever attempted of the corresponding culvert on the north side of the basin." Again, "on January 25, during the fourth trial of the sluices, the gates of the north channel were torn from their fastenings, and swept into the chamber. On the 21st of July the gates of the south sluicing chamber were carried away in a similar manner." After the 15th of November, 1864. when the last trial of the sluice was made, it was found that the sheet piling in front of the apron was bare on the face to a depth of 4 feet, that a hole 9 feet deep below the finished bottom of the basin had been excavated by the water, so that the piling had parted from the masonry of the apron. On the 23d of November the water was pumped out of the chamber, when it was found that a large part of the floor had been torn up, and the concrete and piles laid bare in several places. It became clear that the action of the sluices was attended not only with much inconvenience and hindrance to business, but also with a considerable amount of danger. In fact, so unmanageable were these sluices, with a head of only 14 feet 3 inches of water, which gave to the issuing current a velocity of twenty miles per hour, that the low-water basin, on which £470,252 had been spent, had to be abandoned and converted into a wet dock.

It is true that Mr. Rendel, the original designer of the sluices in question, was unfortunately no longer surviving to carry out his plans. But the law of the flow of water from an orifice under a definite head is so well known that it is not to be thought possible that the velocity of the current exceeded that anticipated. What was wanting was experience as to the inability of even the most carefully-constructed masonry to resist the effects of such a flow of water. From this point of view the failure of the Birkenhead sluices is highly instructive.

Scarcely less significant were the casualties that occurred in 1855, during the construction of the Victoria Docks, designed and executed by Mr. Bidder. These docks are entered from the Thames through a lock 80 feet wide at the bottom, 326 feet 6 inches long from gate to gate, and with a depth of 10 feet on the cill at low water. The walls of the dock are of concrete, faced with piling, and 20 feet thick at the bottom. The portion of the chamber containing the gates is of brickwork, and the bottom between the walls is lined with 7 feet 6 inches of concrete, or with 6 feet of brickwork. The top of this continuous invert is 25 feet 6 inches below Trinity high-water mark. On Sunday, 17th June, 1855, great progress had been made on the works; the upper and lower gates had been lifted into their places, the caisson was nearly completed, the bottom of the large dock of 74 acres had been puddled; and the removal of the river bank, and dredging at the entrance, was nearly all that remained to be done. There had been no symptom of weakness, nor any premonition of what was about to take place, except that on the previous day some joints in the coping on the south side were observed to be a little open, but to so slight an extent that the circumstance was not reported. The next day, however, in the afternoon, the portion of the north side between the upper and lower gates began to give way, moving forward bodily into the lock, pushing up the thick puddle towards the center, bending and breaking the tie bars behind, dragging the tie piles forward, and, in some instances, breaking them off. A few hours afterwards the south side failed in the same way, but the brick walls and platforms remained intact.

This large and sudden failure was accounted for (Proc. Inst. C. E., vol. xviii., p. 463) on the ground that the continuous pumping which had been carried on for two years, night and day, in order to get in the foundations of the lock walls, had drained the country over a considerable area. The water in a well at a distance of $2\frac{1}{4}$ miles from the docks had been lowered during this pumping, and rose when it ceased. At the time of the accident the pumping had been discontinued for some weeks, and 3 feet of

water had been allowed to collect in the dock, to test the puddle. It was excluded from the lock chamber, and it was thought that the hydraulic pressure accumulated at the back of the wall, and forced them bodily inwards. Fractures of the pivot casting, and abrasion and splitting of the roller-path, also took place.

The Avonmouth Dock was designed by Mr. Brunlees; commenced in August, 1868, and opened for traffic in February, 1877. It has an area of 16 acres, being 1,400 feet long and 500 feet wide, and is entered from the Severn by a lock, which has a clear length of 454 feet between the inner and outer gates, and is 70 feet in width. The range of tide for which the gates have to provide is 43 feet 10 inches; the width of the dock wall at the base, 2 feet above the dock floor, is 16 feet, and the width at the top is 10 feet 6 inches. When about 247 lineal yards of this section of wall had been built up to the level of the coping, and the backing was well advanced, a failure of about 90 yards occurred. The wall broke through from top to bottom in the middle of this distance, slid forward 12 feet, and sank 4 feet 6 inches. Where not affected by this slip, the wall showed no tendency to slide out of place, but an inclination to overturn by coming forward at the top. It was built with a batter of 3 feet 4 inches from floor to coping, but when the forward movement ceased, the batter measured only 1 foot 4 inches. The part of the wall that slipped forward maintained its original batter.

When the east wall was finished and backed up to nearly its full height, and within a few days from the date fixed for letting water into the dock, a subsidence of about 140 yards of the most recently completed portion took place. The movement was of a similar kind to that which had occurred on the opposite side. At the apex or breach, the wall slipped forward 15 feet 6 inches, and sank 7 feet 6 inches. The dock floor in front was thrown up 10 feet in height for a distance of about 60 feet. The breach in the wall was much larger than in the other case, and extended through the foundations. This portion of the wall was founded at an average depth of 9 feet below the bottom of the dock, and the whole of the foundation moved forward with the slip. Both accidents occurred on a Sunday night, when no men were at work, and two hours before the occurrence of the latter failures nothing unusual was to be seen in the wall.

In the discussion which took place on the Avonmouth Dock at the Institution of Civil Engineers on December 12, 1878, Mr. Walmsley Stanley remarked that before designing the walls of a dock, the exact nature of the strata on the line, and for some distance on each side of the walls, should have been ascertained. He regarded the foundation for the docks at Avonmouth as exceptionally good. They rest on a bed of fine grey sand underlying the clay at an almost uniform level throughout its length, and at a depth of 6 feet below low water of equinoctial spring tides. The trial borings were only carried 10 feet deeper, and the pile-driving gave no signs that the sand had been passed through at a depth of 25 feet. Springs were frequent in this sand. A layer of 6 inches of clay had been sufficient to keep these down, but when this was removed they burst out in various places, and in some had to be provided with permanent outlets. The question is very pertinent. "Were these failures through any new elements of danger, or new combination of elements, or did they arise from want of proper examination before the design and execution of the work?" Whatever reply be given to this question the special study that is requisite for works of the kind is abundantly proved by these serious failures.

In the year 1864 it was found necessary to extend the dock accommodation at Belfast. The foundation for the wall of the new Abercorn basin was firm sand, so that it was not considered necessary to do more than drive in front of the wall a close row of sheet piles, 12 feet long and 6 inches thick. At the Dufferin and Spencer docks the substrata were soft and unreliable, and 15 feet bearing piles of round larch were introduced. Battens and sleepers were laid on the heads of these piles, and covered with 18 inches of concrete, on which the masonry, 16 feet in width at the bottom, was built. These walls showed weakness from the first. As the backing proceeded, the symptoms became more seri-

ous. Two years after the erection of the entrance to the Spencer tidal dock, a length of 70 yards of the wall fell bodily forward into the entrance. It fell outwards at the top, pivoting on its toe, and broke the bearing piles across about their middle, 6 feet or 7 feet below the bottom of the wall. The sleepers, concrete, and tops of the piles adhered to the masonry, which had to be removed by divers. The nature of the backing used, which seems to have exerted on the back of the wall a semi-fluid pressure perhaps double of that due to hydraulic head, together with the soft foundation, are enough to account for the Belfast failures. Still the same question recurs: Was not the nature of these materials known beforehand? and should they not have been more skillfully dealt with?

Closely combined with the important question of the security of the foundation of hydraulic works is that of the true principles on which their dimensions should be determined. While adequate size is demanded by the exigencies of traffic, any undue or unused excess of dimensions is at the same time a cause of increased cost and of increased risk. In hydraulic works the ruling dimensions must be determined in consistence with those of the craft which it is intended to accommodate. As to this, the present course of shipbuilding is, to some extent, in favor of restricted rather than of increased width, although in those dimensions which provide for the length of craft there is advance rather than decline. The gates of the Canada Dock at Liverpool, 100 feet wide, were constructed for paddle-box steamers. So great a width is not needful for a screw-ship. It is plain that the extreme dimensions in length, breadth, and width, plus a certain allowance for clearance, of the craft which it is designed to accommodate, should be taken as the ruling dimensions for hydraulic works. For locks, a foot or two of clearance in each direction may suffice; for docks, basins and canals, 25 per cent. of width and depth will hardly be too much to allow for clearance. Room in excess of this will involve waste. The present proportions used by shipbuilders give from eight to ten beams to the length, and locks that are not in this proportion either for one or for a pair of vessels involve great waste, both in construction and in supply of water. Thus the 454 feet lock at Avonmouth could accommodate the largest vessel that is now likely to pass it, if it were 45 feet, or at the utmost, 50 feet wide. A dock entering from the tideway can hardly be expected to admit two vessels at a time, convenient as this arrangement is on a canal; but if two vessels, say of 38 feet beam, were docked at the same time at Avonmouth, there would be a useless length of 50 feet or 60 feet in the lock.

The want of intelligent foresight in the adoption of dimensions that shall be adequate, and not more than adequate, for the work that a structure is intended to facilitate, is remarkably evinced in the dimensions of our canal locks. Thus the line of canal communication between the water of the Thames and that of the Trent is made by two different lines. A boat arriving at Northampton by the Grand Junction Canal has to use the Grand Union Canal in order to go on to Leicester. But while the locks on the Grand Junction are 87 feet 6 inches long, and 15 feet wide, those on the Grand Union are only 78 feet long, and 7 feet 2 inches wide. As to the width, it may be urged that the narrow locks will admit a single boat, while the wide lock will admit either a barge or a pair of boats of the same width as will pass, singly, through the narrow lock. But as to length there is a positive discrepancy. A boat built for the Grand Junction lock will not pass through the Grand Union lock. A boat built for the latter, if it pass on the former canal, loses 10 or 15 per cent. in capacity, and involves a corresponding unnecessary loss of water at each lockage.

In no case is this absence of proper engineering consideration more conspicuous than in that of the Suez Canal. After an expenditure of £20,000,000 on a water way of eighty-eight geographical miles in length, extreme difficulty is felt in passing a traffic of 7,000,000 tons of shipping through the canal in a year. In 1882, 3,198 ships, of an average tonnage of 2,150 tons each, made the transit of the canal. This is not quite nine vessels per day, and yet it has been decided to expend a million sterling on the inefficient palliatives of intermediate basins. "For all steam ships, or vessels towed,

varying between 280 feet and 300 feet in length," reported the Hydrographer to the Navy, in February, 1870, "with 35 feet beam, and a draught of 20 feet, it will, with the improvements and appliances earlier described, be a convenient highway....For the transit of vessels larger than those described, the canal is not so well adapted." That the Indian transports, of "about 400 feet long, with a draught of 22 feet of water, and beam of nearly 50 feet, can pass through the canal is undeniable; but no practical seaman need be told that in steering through what may be called a continuous dock ninety miles in length, less than 100 feet wide, and with nothing showing above water to mark the center of it, frequent grounding and consequent delay may be anticipated, though every possible care and precaution be taken. It is to be considered, also, that the midship section of one of these vessels bears about an average proportion of 1 to 4 to the deep water of the canal."

And yet the cross section of the Suez Canal, according to the final determination, contains 3,862 square feet beneath the water line. The midship section of H. M. S. Warrior is 1,219 square feet. This is a much closer fit than the proportion named by the Hydrographer to the Navy. The Warrior made one of the fastest passages on record through the canal, viz., in 12 hours 50 minutes. But her speed at sea was 14.35 knots per hour. But of the entire cross section of the canal, not more, in many parts, than 66 per cent. is in any way available for navigation. The only function performed by at least one third of the area is that of allowing the water displaced by the passage of a vessel to return to the wake of the same, or that of the protection of the banks from wash (as to the latter, however, revetting with stone is being partially carried out). The passage of vessels is restricted to a speed of five knots, and no two vessels are allowed to cross in the canal unless one of them be moored in a lay-by.

Of these "gares," or passing places, there are fourteen. It will thus be seen that thirty vessels are the utmost that can at one time be in course of transit through the canal, viz., fifteen under steam and fifteen in the lay-byes or terminal basin. At five miles an hour (and allowing only twelve hours in the day) it would thus take nearly two days to go through 100 miles of canal. But, as for half the time of transit, the vessels are in the lay-byes, the speed is reduced to 2½ knots, and the time of passage is extended to four days. Hence the just complaints of the shipping trade.

By the use of a scientific cross section, of equal area to that actually excavated, but with revetted sides, the capacity of the Suez Canal would have been increased in a proportion which it may seem fabulous to state. In the first place two 2,000-ton vessels would have been able to pass one another in any part of the canal, the vessel which had the wind against her slacking or stopping for the time of passing. This would at once raise the capacity of the canal from 30 to 200 vessels—as a line of ships might follow one another, in each direction, with perfect ease a mile apart. In the second place the time consumed in passing would have been reduced by one-half; as no time would be lost in lay-byes. In the third place the cost of propulsion would have been reduced, as the hydraulic mean depth of the scientifically designed canal (the area of water divided by the wetted perimeter) which is taken by hydraulicians as a measure of resistance, is double that of the actual cross-section. And in the fourth place, as a curved bottom would be substituted for the present flat bottom, vessels of 4 feet more draught than could pass the French canal could pass one constructed with the true hydraulic cross-section.

Important as these considerations are to the shareholders who have sanctioned the expenditure of another million for the construction of intermediate basins, which will little, if at all, increase the actual capacity of the canal for traffic, as above shown, we have but little hope that any remonstrance expressed in the English language will be likely to reach them. But if traffic is impeded by the contempt shown by politicians for hydraulic rules, it is to be hoped that this will not be altogether the case in England. Not a week now passes without signs of the revival of the public interest in canals. What has been done in England by canals is now being investigated; and the return of a certain portion of traffic

to these cheap and silent high roads is now anticipated by every writer who touches on the subject.

Since the days of Brindley and of Telford great advance has been made in mechanical and in hydraulic knowledge. It is sufficient to compare the experiments of Mr. Froude, as they are described in volume xviii. of the Transactions of the Institution of Naval Architects, with those of Mr. Palmer and Sir John MacNeill, as described in volume i. of the Transactions of the Institution of Civil Engineers, to see how much has been done to determine the true dimensions of vessels, and how little has been done to determine the true dimensions of canals.

According to the formulæ now accepted, the resistance to the passage of a vessel through the Suez Canal is just double that which would be encountered if the cross section, when of the same area, was of a better shape. We do not say that this is the case—we only say that it is so according to the accepted formulæ. But with millions in the course of expenditure on the waterways of Europe, is it not lamentable that we should have to look in vain for the inauguration of those experiments, for the guidance of the hydraulic architect and engineer, of which every member of the profession who has turned his attention to the subject at once acknowledges the importance and deplores the absence?

RESISTANCE ON RAILWAY CURVES AS AN ELEMENT OF DANGER.

By JOHN MACKENZIE, Assoc. M. Inst. C. E.

From Proceedings of the Institution of Civil Engineers.

The friction caused by the unequal and indirect action of the wheels of a locomotive engine or carriage on curved rails is a source of additional tractional resistance, and also of a considerable amount of danger, from the tendency of the wheels to run off the rails; and it is entirely with regard to this danger that the subject is treated in this paper.

On looking over the Board of Trade Returns of Railway Accidents for several years, it will be found, in most of the cases in which engines left the rails without a tolerably obvious cause, that the engines were six-wheeled with parallel axles, and in some cases were running at low speeds; indeed in one case the speed was so low that the centrifugal force was more than balanced by the cant of the rails, so that some other agency than centrifugal force was evidently at work.

The tendency which a wheel has to mount the rail on a curve is evidently caused by the adhesion or friction between the rail and the flange of the wheel, and this adhesion will bear some proportion to the side pressure with which the flange is forced against the rail. In the case of the wheel most likely to mount the rail, namely the outer leading wheel, this side pressure, at low speeds, is principally caused by the resistance which the treads of the wheels oppose to the sliding motion which takes place in running round a curve.

Taking the case of an engine with six cylindrical wheels on three parallel axles, all coupled, the middle or driving axle being midway between the other two: when such an engine is running on a curve, and the clearance is the least possible, so that the flanges of the wheels just fill the space between the rails, then the driving axle is radial, the wheels on the leading axle tend to run outwards, and those on the trailing axle to run inwards.

The flange of the outer leading wheel is pressed hard against the rail. The flange of the inner driving wheel is parallel to the rail, and has of itself no tendency to press against it, but is kept close to it by the trailing wheels: the outer trailing wheel, although the flange is kept close to the rail by the outer leading and inner driving wheels, has a tendency to leave the rail; and the inner trailing wheel, notwithstanding the tendency to run against the rail, has the flange

kept clear of it by the outer leading and inner driving wheels.

When the coning of the wheels exactly suits the curve, the leading wheels, and in this case the trailing wheels, do not slide along but only across the rails; but the sliding of the driving wheels is increased if the wheels are coned, unless indeed they are coned in the opposite direction to the others.

When the clearance between the rails and the flanges of the wheels is increased to just a sufficient extent to allow the inner trailing-wheel flange to run against the rail then the driving axle is not radial, but has the inner end slightly in advance of a radial line, and the trailing axle has the inner end slightly behind a radial line, and the sliding motions of the wheels take place round a point P, situated between the driving and the trailing wheels. In this case the sliding of the trailing, as well as of the driving, wheels is increased if the treads of the wheels are conical.

When the clearance is increased, or the radius of the curve increased, to a still further extent sufficient to allow the inner trailing wheel to run with the flange just clear of the rail, then the position which the engine takes up on the rails depends somewhat upon the comparative loads on the wheels. The driving wheels, in following the leading wheels have a tendency to assume a position with their axis radial; but the trailing wheels have the same tendency, and it is impossible that both axles can be radial. If one pair of wheels is much more heavily loaded than the other, the more heavily loaded pair will run with the axis radial, but the tendency of the trailing wheels to run forward is increased by the tendency of all the inner wheels to outrun the outer ones, so probably the trailing axle is generally radial, and the sliding motions of the wheels take place round a point situated where the inner wheel touches the rail.

In order to cause these sliding motions, the outer leading-wheel flange exerts against the rail a pressure sufficient to overcome the adhesion or friction of the treads of the wheels; this pressure being exerted directly on the leading wheels, and transmitted to the other wheels through the medium of the engine framing acting as a lever with its fulcrum at the point of contact.

The resistance which a wheel opposes to sliding across the rail probably does not differ much from its adhesion for traction; and the pressure exerted by the leading-wheel flange to overcome this resistance or adhesion will, for each wheel, be approximately equal to the adhesion of that wheel multiplied by its distance from the wheel which acts as fulcrum, and divided by the distance of the leading axle from the axle on which is the wheel acting as fulcrum. These pressures being taken for each wheel, the sum of them will be the total side pressure on the outer leading-wheel flange.

When the flange of the outer leading wheel is on the point of mounting the rail, the tread, being relieved of pressure and adhesion, need not be taken into account in estimating the pressure on the flange.

The distances which the several wheels slide vary with the sharpness of the curve, but the adhesion remains constant; so that although the energy expended in causing the wheels to slide while the engine runs a certain distance along the rails increases with the sharpness of the curve, the pressure in the line of the axle required to effect this sliding is nearly constant, whatever may be the radius of the curve. The pressure exerted by the flange on the rail is, however, slightly greater than that in the line of the axle, as that line is not at right angles to the rails, and this pressure increases as the secant of the angle at which the wheel stands on the rails. As this angle can never in practice exceed 2° 30′, and the secant of 2° 30′ does not exceed the radius by one-thousandth part of its length, the additional pressure arising from increasing sharpness of curve may be neglected.

On a curve the point of contact between the outer leading-wheel flange and the rail is in advance of a perpendicular from the wheel's axis, so that the motion of the wheel at that point is downward, imparting a downward pressure to the rail, and an upward pressure to the wheel. Thus, when the adhesion between the flange and the rail is greater than the weight upon the wheel, the flange would rise and mount the rail even if the surfaces at the point of contact were ver-

tical; and when they are inclined (as they always are), the tendency of the wheel to rise is augmented.

Owing to the downward motion of the flange at the point of contact, the side pressure required to cause the flange to mount the rail is, not the pressure which, when the wheel is at rest, would force it over the rail in opposition to friction as well as to gravitation, but the very much smaller pressure which, when the wheel is at rest and the tread slightly raised above the rail, would cause friction sufficient to prevent its falling into its place again.

With a wheel and rail whose surfaces at the point of contact do not appreciably vary from the vertical, the wheel would be caused to mount the rail by a side pressure bearing the same proportion to the load on the wheel which the oad bears to the adhesion.

When the surfaces at the point of contact are inclined (as they always are), the pressure required to cause the wheel to mount the rail diminishes rapidly as the angle of inclination increases; as with an angle flatter than that down which the one surface would just slide on the other, the flange, if raised, would not slide back into its place even if there were no side pressure.

When the outer leading wheel begins to rise, the increasing deflection of the spring opposes a greater resistance to its rising further. This tendency to rise, while the conical part of the flange touches the rail, is nearly constant so long as the proportion of adhesion to pressure is constant; but if this proportion raises the wheel to such an extent that the rounded edge of the flange begins to come into contact with the rounded corner of the rail, the tendency to rise increases more rapidly than the resistance of the spring, and the wheel, which has then passed its maximum of stability, mounts the rail.

The higher tension of the spring adds to the load on the outer leading wheel, and at the same time raises the center of gravity of the engine, and alters the distribution of the remaining weight on the other wheels, this redistribution varying with different arrangements and proportions of springs.

It has not been proved experimentally whether the friction of the flange against the rounded corner of the rail is the same, in proportion to the pressure, as is that of the tread against the crown of the rail. The surface of a cylindrical tread may run on a straight rail without any sliding motion whatever; but the highly inclined surface of the flange can never run against the rounded corner of the rail without some portion of even the very small surfaces in contact sliding. It is therefore possible that the flange-friction partakes of the nature of the friction of a skidded wheel, or of a wheel against a brake-block which has been found to diminish at high velocities; and this possibility is almost rendered a probability by the circumstance that, of the cases reported of engines leaving the rails, comparatively few occurred at high speeds. This is the most doubtful part of the question; or rather it is the only really doubtful point; the rest is a mere mechanical problem, but an extremely complicated one. In the absence of information on this point, it will be assumed that, at moderate velocities, the flange friction bears the same proportion to the pressure as does the adhesion of the tread, and that the two vary together when the state of the rails varies. This being so, with a doubled coefficient of friction the side pressure on the leading-wheel flange, required to make the other wheels slide, is doubled: at the same time the proportion which the flange's own adhesion bears to that side pressure is also doubled, so that the adhesion of the flange is fourfold. In other words, the tendency to mount the rail increases as the square of the fraction representing the coefficient of adhesion; or rather it would increase in this ratio if the surfaces were vertical, but when they are inclined the ratio is less rapid.

When the leading flange is lubricated, its friction is nearly constant, so that the tendency to rise increases only in the simple ratio of the adhesion, instead of in the duplicate ratio.

It is evident that if there were no friction between the rails and the treads and flanges of the wheels, the outer leading wheel, when running slowly on a curve, would have no tendency to mount the rail; and it is equally evident that if the friction were infinitely, or very, great, the engine would run straight forward over the rails. Between these extremes there

must be a certain proportion of friction which will just balance the weight on the wheel, and any addition to this friction would cause the wheel to rise over the rail. This differs in different engines; and if for any engine the proportion of friction which will cause it to leave the line is one that may occur in certain conditions of the rails, then that engine is dangerous in the extreme; and any engine is so to some extent, which has not, in this as in other respects, a considerable margin of safety.

Captain Douglas Galton, Assoc. Inst. C. E., in giving the results of his experiments on the adhesion between wheels and rails, states that, "On dry rails it was found that the coefficient of adhesion of the wheels was generally over 0.20. In some cases it rose to 0.25 or even higher. On wet or greasy rails, without sand, it fell as low as 0.15 in one experiment, but averaged about 0.18. With the use of sand on wet rails it was above 0.20 at all times; and when the sand was applied at the moment of starting, so that the wind of the rotating wheels did not tend to blow it away, it rose up to 0.35, and even above 0.40."

Thus an engine could not be considered safe if, when the adhesion rose to 40 per cent. of the weight, the side pressure on the flange of the leading wheel were sufficient to prevent the flange, when raised, from sliding down again on the side of the rail.

To apply the foregoing to the case of an engine which actually left the rails, the following example may be taken from the Board of Trade Returns as to railway accidents, where the circumstances are thus reported: "The branch leaves the main line on a curve to the right, with a radius of only 640 feet. . . . There is a cant of $2\frac{1}{4}$ inches. The engine is a four-wheel-coupled tender engine, with a wheel base of 14 feet 4 inches. The weights on the three pairs of wheels are—

	Tons.	cwt.
"On leading wheels..........	7	8
" driving "	10	13
" trailing "	7	12
Total..........	25	13

"The engine itself is in good order, and there is no reason to attribute the accident to any fault in the rolling stock. The speed was, no doubt, as much as 15 miles an hour, the authorized speed, but . . . there is no reason for supposing that this speed was exceeded.

"The road did not gauge very evenly from the facing points southward, being in some places as much as $\frac{9}{16}$ inch slack, and in others exact to gauge."

The Inspector goes on to say: "I am of opinion that this accident was due to the want of a proper check-rail at this curve of under 10 chains radius, and to the somewhat slack state of the permanent way."

Assuming that each wheel had an independent spring which (not to overestimate the danger) may be assumed to have been stiff enough to deflect only 2 inches with its usual load; and that the driving axle, with its wheels, weighed $1\frac{1}{2}$ tons, and the leading and trailing axles, with their wheels, 1 ton each; and that the outer leading wheel rose $\frac{1}{2}$ inch before the rounded edge of the flange came into contact with the rail,—then this additional deflection of its spring probably increased the load on this wheel to about 4 tons 1 cwt., and caused the remaining weight to be distributed on the other wheels somewhat as follows, namely:

	Tons.	cwt.
On the inner leading wheel.....	3	10
" outer driving "	5	0
" inner " "	5	7
" outer trailing "	3	15
" inner " "	4	0

Assuming that the inner driving-wheel flange was pressed against the rail, and also, as a preliminary, that the adhesion was $\frac{1}{5}$ of the weight, the pressure exerted by the outer leading-wheel flange in causing the treads of the outer wheels to slide on the rails (its own tread being lifted clear of the rail), was probably:

For the outer leading wheel — Ton.
$=0.0(4.05\times 7.16=29=N)$ $=0.0000$

" the inner leading wheel
$=0.2(3.5\times 71.6=25.060)\div 7.16=0.7000$

" the outer driving wheel
$=0.2(5.0\times 4.916=24.580)\div 7.16=0.6866$

" the inner driving wheel
$=0.2(5.35\times 0.00=0.000)\div 7.16=0.0000$

" the outer trailing wheel
$=0.2(3.75\times 8\ 61=32.287)\div 7.16=0.9018$

" the inner trailing wheel
$=0.2(4.00\times 7.16=28.640)\div 7.16=0.8000$

$M=110.567$ foot-tons.
$15\ 44=110.567\div 7.16$
$0.2\times 15.44=$ tons 3.0884

or say a little over 3 tons.

Next, assuming that the flange formed an angle of 75° with a horizontal plane, or 15° from the vertical, then, with an adhesion of $\frac{1}{5}$, the load of 4 tons 1 cwt. on the outer leading wheel would prevent its rising unless forced against the rail by a side pressure equal to:

$$\tfrac{1}{5}=0{\cdot}2=\text{cotangent } 78°\ 41'\text{; then}$$

$$\text{Tangent } (78°41'-15°\ 0')\times 4.05 \text{ tons} =2.02\times 4.05=8.18 \text{ tons or, say,}$$

$8\frac{1}{4}$ tons, or nearly three times the side pressure which would cause the other wheels to slide; so that with this proportion of adhesion, there would have been a considerable margin of safety unless other forces were at work.

It is uncertain from the report whether or not the steam was shut off when the engine left the rails; but in order to leave the case as simple as possible, it will be assumed that it was so, and that the engine was exerting no tractive force.

Taking the speed at 15 miles an hour, or 22 feet per second, the centrifugal force was:

$$\frac{\overset{\text{Tons.}}{25.65}\times 22^2}{32\times 640 \text{ feet}}=\overset{\text{Ton.}}{0.606}.$$

or only about 12 cwt., which was considerably more than counteracted by the cant of $2\frac{1}{4}$ inches; the inward force due to which was

$$\frac{2\frac{1}{4}\text{ inches}\times 25{\cdot}66 \text{ tons}}{59 \text{ inches}}=0.97 \text{ tons.}$$

or slightly exceeding 19 cwt., being about 7 cwt. more than the centrifugal force. As indicated by the loads on the wheels, the center of gravity was about $\frac{1}{2}$ inch behind the center of the driving axle; so, assuming the inner driving-wheel flange to have been pressed against the rail, the excess of cant would have thrown on the outer leading-wheel flange a side pressure of

$$86 \text{ inches} : \tfrac{1}{2} \text{ inch} :: 7 \text{ cwt.} : 0.04.,$$

or about 5 lbs., which need not be taken into consideration. To this small extent, however, the engine would have been less likely to have left the rails had it been running at the speed at which the cant would balance the centrifugal force. There is no probability that the engine was running at the very high speed at which the centrifugal force, after counteracting the cant, would have been sufficient to cause the flange to adhere: the accident cannot be accounted for under the conditions mentioned.

Assuming, however, that the adhesion, instead of $\frac{1}{5}$, or 20 per cent., had been 37 per cent. of the weight: then in order to cause the other wheels to slide, the outer leading-wheel flange would have required to exert a side pressure equal to

$$0.37\times 15.44=5.7 \text{ tons.}$$

or about 5 tons 14 cwt.; and this side pressure is just equal to that which, with 37 per cent. of adhesion, would cause the flange to adhere and rise over the rail, namely,

$$0.37 \text{ being cotangent of } 69°\ 42',$$

$$\text{Tangent } (69°\ 42'-15°)\times 4.05 \text{ tons} =1.41\times 4.05=5.7 \text{ tons, 5 tons 14 cwt.}$$

Thus the accident might have been caused by the rails being in such a condition that the adhesion was 37 per cent. of the weight, assuming that it happened at a place where the gauge was sufficiently tight to allow the flange of the inner driving wheel to come into contact with the rail before that of the inner trailing wheel. This might have been the case, as the curvature of the rail in the length of the wheel base amounted to about $\frac{1}{2}$ inch.

The tractive force, the centrifugal force, and the cant have been left out of consideration, as their effect in this case was so small as hardly to influence the result.

It may have been noticed that the coefficient of the friction which would just cause the flange to adhere and mount the rail was assumed to be 37 per cent. in order more easily to explain the process of calculation, as the formula for ascertaining this coefficient is somewhat long, though quite elementary, being

$$\frac{N}{\left(\tan a\times\frac{M+N}{2\cos a}\right)\times\sqrt{\left(\frac{M+N}{2\cos. a}\times\frac{M-N}{2}\right)\times\left(\frac{M+N}{2\cos a}-\frac{M+N}{2}\right)}}=\text{coefficient,}$$

where N=product of the weight on the outer leading wheel multiplied by its distance from the wheel acting as fulcrum.

M=sum of the similar products of all the other wheels; and

a=angle formed by the working face of the flange and the plane of the wheel.

In the case in question—

N=29 foot-lbs.

M=110.567 foot-lbs.

$a=15°$; and

$$\frac{29}{(\tan 15° \times 69.78) + \sqrt{\left(\frac{69.78}{\cos 15°}+40.78\right)\times\left(\frac{69.78}{\cos 15°}-40.78\right)}}=0.37.$$

The foregoing case was selected for an example as being comparatively free from disturbing elements, and not on account of the percentage of friction causing the engine to leave the rails being exceptionally low. In another accident reported on, adhesion would have been less than 36 per cent. if the engine had been tight to gauge and about 46 per cent, if slack. In this last case the Inspector considered that the accident was due partly to some defect in the permanent way, and partly to the load on the wheel which left the rail being less than on the other wheels, the load on the leading wheels being 8 tons 12 cwt., on the driving wheels 12 tons 13 cwt., and on the trailing wheels 10 tons.

The following tables are calculated, as far as possible, exactly in a similar manner to the case given as an example, and are merely intended to indicate the com-

INFLUENCE OF DISTRIBUTION OF LOAD ON THE COEFFICIENT OF DERAILING ADHESION.

Normal Load on			Tight to Gauge, with Inner Driving-wheel Flange touching Rail.			Slack to Gauge, with Inner Trailing-wheel Flange touching Rail; or with Flangeless Driving Wheels.		
Leading Axle.	Driving Axle.	Trailing Axle.	Coefficient of Derailing Adhesion.	Coefficient of Derailing Adhesion with Flanges, lubricated.	Additional Tractional Resistance due to curve of 5 chains radius, in lbs. per ton, with adhesion =one-fifth of load.	Coefficient of Derailing Adhesion.	Coefficient of Derailing Adhesion, with Flanges lubricated.	Additional Tractional Resistance due to curve of 5 chains radius, in lbs. per ton, with adhesion =one-fifth of load.
Tons.	Tons.	Tons.			Lbs.			Lbs.
1	28	1	0 12	0.10	4.48	With these loads the adhesion of the driving wheels prevents the trailing-wheel flange from touching the rail.		
2	26	2	0.18	0.19	4.96			
3	24	3	0.24	0.28	5.47			
4	22	4	0.28	0 35	5.98			
5	20	5	0.31	0.41	6.50			
6	18	6	0.33	0.47	7.05			
7	16	7	0.35	0.51	7.58	0.40	0.63	9.56
8	14	8	0.36	0 55	8.11	0.44	0.75	9.85
9	12	9	0.38	0.58	8.64	0.48	0.87	10.11
10	10	10	0.39	0.61	9 20	0.52	1.00	10.50
11	8	11	0.40	0.64	9 74	0.55	1.14	10.84
12	6	12	0.41	0.66	10.28	0.59	1.28	11.23
13	4	13	0.41	0.67	10 82	0.63	1.43	11.35
14	2	14	0.42	0.70	11.35	0.66	1.61	11.79
15	0	15		= four wheels.		0.70	1.79	11.99
8	14	8	0 36	0.55	8.11	0.44	0.75	9.85
8	11	11	0.34	0.48	9.00	0.46	0.82	9.92
8	8	14	0.32	0.43	9.90	0.49	0.90	9.89

Influence of the Width of Gauge on the Derailing Adhesion.

Width of Gauge.	Coefficient of Derailing Adhesion.	
	Tight to Gauge, with Inner Driving-wheel Flange touching the Rail.	Slack to Gauge, with Inner Trailing-wheel Flange touching Rail, or with flangeless Driving Wheels.
Feet. Inches.		
2 0	0 42	0.55
3 0	0.41	0.54
4 8½	0.39	0.52
7 0	0 36	0.49
35 0	0.20	0.29
75 0	..	0.20

Influence of the Length of Wheel-base on Derailing Adhesion on a Curve of 5 Chains (330 feet) Radius.

Length of Wheel-base.	Coefficient of Derailing Adhesion.	
	Tight to Gauge, with Inner Driving-wheel Flange touching the Rail.	Slack to Gauge, with Inner Trailing-wheel Flange touching Rail, or with flangeless Driving Wheels.
Feet.		
1	..	0.20
2	0.20	0.29
5	0.29	0 41
10	0.36	0.49
12	0.38	0.50
15	0.39	0.52
20	0.40	0.53
25	0.41	0.52
30	0.42	0.52
35	0.42	0 50
40	0.41	0.48
50	0 40	..
200	0.20	0.20

parative influences which variations in the proportions of an engine have in increasing or diminishing its tendency to leave the rails. These influences are measured by the coefficient of the friction which will just cause the flange of the outer leading wheel to adhere and mount the rail, referred to in the tables as the "coefficient of derailing adhesion."

Influence of the Number of Wheels on the Derailing Adhesion.

Number of Wheels.	Coefficient of Derailing Adhesion.	
	Tight to Gauge, with the Flange of the Inner Wheel in the middle, or nearest the middle, of the length touching the Rail.	Slack to Gauge.
4	0.70	0.70
6	0.39	0.52
8	0.39	0.39
10	0.29	0.35
18	0.20	0.24

It will be noticed that the danger is greatest in engines which have an axle in the middle of the length of their wheel-base.

Influence of the Angle of Flange on the Derailing Adhesion

Complement of the Angle between the working face of the Flange and a horizontal Plane.	Coefficient of Derailing Adhesion.	
	Tight to Gauge, with Inner Driving-wheel Flange touching the Rail.	Slack to Gauge, with Inner Trailing-wheel Flange touching Rail, or with flangeless Driving Wheels.
° ′		
0 0	0.53	0.68
5 0	0.48	0.62
10 0	0.43	0.56
15 0	0.39	0.51
16 30	0.37	0.50
20 0	0.35	0.46
22 30	0.33	0.44
25 0	0.31	0.42
30 0	0.28	0.38
35 0	0.25	0.34
40 0	0.22	0.30
45 0	0.19	0 27
50 0	..	0.23
55 0	..	0.20

The engine is supposed in all cases to weigh 30 tons. Where not otherwise stated, and not inconsistent with the other particulars, this weight is considered, when the wheels are level, to be equally distributed on the treads of six

coupled wheels 4 feet in diameter; the leading axle with its wheels weighing 1 ton, the driving axle and wheels $1\frac{1}{2}$ ton, and the trailing axle and wheels 1 ton, the deflection of each of the springs with its ordinary load being 2 inches. The load stated in the tables is this normal load, and not the load as altered by the rising of the outer leading wheel; but the alteration caused by this rising is allowed for as before.

Similarly the wheel base is taken as 15 feet, the gauge 4 feet $8\frac{1}{2}$ inches, the angle between the face of the flange and the plane of the wheel 15°, and the treads of the wheels are assumed to be cylindrical.

In most cases the circumstances differ from the above in one particular only.

When described as "tight to gauge," the curve and clearance are supposed to be such that the inner driving-wheel flange touches the rail. Where described as "slack to gauge," the curve is supposed to be easy enough, or the clearance and end play of the bearings to be sufficiently great, to allow the inner trailing-wheel flange to touch the rail; or the driving wheels are supposed to be flangeless, which would have the same effect.

As it is particularly desired to avoid exaggerating the danger, the circumstances as above stated are perhaps rather more favorable than would usually exist; for instance, the springs would generally be deflected more than 2 inches, and the angle of most flanges probably exceeds 15°. The centrifugal force also would generally increase the tendency to mount the rail, and in some cases this tendency would be increased by high winds and by oscillations.

In most cases the proportions are carried, in the tables, to their extreme limit in one direction, being that at which the derailing adhesion becomes less than one-fifth of the load, or 0.20. As might have been expected, this limit is generally far beyond what occurs in practice.

Restricting the subject strictly to six-wheeled engines with parallel axles, and referring to such engines only, the following inferences may be drawn from the foregoing remarks:

1. That the danger of leaving the rails increases when the adhesion increases.

2. That the greater the portion of the weight of the engine resting on the leading wheels, the less is the danger of leaving the rails at low speeds.

3. That when tight to gauge the danger is increased to the greatest extent by augmenting the load on the trailing wheels, at low speeds.

4. That when slack to gauge, or the driving wheels flangeless, or the radius of the curve large, the greatest danger is brought about by increasing the load on the driving wheels.

5. That the danger increases when the radius of the curve decreases; but at low speeds, the danger increases very slowly, until the curvature becomes such that the inner driving-wheel flange is brought into contact with the rail, when the danger increases suddenly; and the danger is aggravated by tightness of gauge.

6. That this sudden increase of danger is obviated by making the driving wheels flangeless; and that flanges on the driving wheels are worse than useless.

7. That the danger increases when the gauge is widened in proportion to the length of the wheel-base.

8. The danger of leaving the rails diminishes when the length of wheel-base increases in proportion to the width of gauge; but when the length of wheel-base increases in proportion to the radius of the curve, the danger increases; so that there is a certain length of wheel-base giving a minimum of danger: but within the limits of practice, the longer the wheel-base, the less the danger of leaving the rails. Nevertheless the tractional resistance always increases with the wheel-base.

9. The danger is increased by increasing the number of wheels; but the tractional resistance diminishes when the number of wheels is increased.

10. That the danger increases when the angle formed by the working side of the flange and the plane of the wheel increases beyond that necessary to keep the rounded edge of the flange clear of the rail on the sharpest curve it has to run on.

11. That the danger is increased by enlarging the diameter of the leading wheels.

12. That the danger is increased by diminishing the projection of the flange.

13. That the danger is increased by making the springs of the leading wheels more flexible.

14. That the danger is considerably diminished by lubricating the flanges of the leading wheels; and increased by applying sand to these wheels.

15. That, when the center of gravity is behind the driving axle, the danger is increased by an excess of super-elevation of the outer rail beyond that required to counteract the centrifugal force.

16. That the danger may be increased by shutting off steam and reducing speed while running on a curve.

17. That, although one engine or carriage may run round a curve with less tractional resistance than another, it is not therefore necessarily in less danger of leaving the rails. In many cases it is quite the reverse.

Lastly. That, assuming an adhesion of 40 per cent. of the weight to be possible, some engines of not unusual proportions have, as regards leaving the rails, a very narrow margin of safety: so narrow that, in any other matter connected with the running of a train, a similar margin of safety would be considered insufficient.

Appendix I.

The paper was suggested by an unintentional experiment, in which the springs of the driving wheels of a six-wheeled engine were tightened to increase the available adhesion. This was overdone, and when the engine was pushed slowly along to remove it to another line of rails, the leading-wheel flange, on coming to a curve, mounted the rail. The springs were then slackened back to their old bearings, and the engine ran safely afterwards, as it had done before. The rails were dry, and owing to the nature of the ground they were in places sprinkled with sand, so that probably the adhesion was great in proportion to the weight.

This incident seemed not only to point to the recognized necessity of having a considerable portion of the weight of a six-wheeled engine resting on the leading wheels, or rather the outer one of them, to guide the other wheels around a curve, but also to suggest that it would be desirable to ascertain the limit of safety in this respect, as it appeared possible that, in practice, this limit is sometimes exceeded, and may account for many hitherto unexplained accidents.

The most satisfactory way of ascertaining this limit of safety would be by direct experiment; but an exhaustive series of such experiments would be an undertaking which would require the resources of a railway company.

In the absence of such experiments, and to afford an opportunity for preliminary discussion, it may therefore be advisable to attempt to estimate this limit of safety by inferences from experiments made for other purposes, and from the experience of accidents recorded in the Board of Trade Returns.

The subject is not devoid of public interest, as, according to the Board of Trade Returns of Railway Accidents in 1880, the deaths of passengers resulting from "passenger trains, or parts of passenger trains, leaving the rails," were more numerous than those from any other class of accident enumerated in the summary at p. 3.

Appendix II.

A single narrow wheel rolling freely on a level surface at low speed runs in a straight line; but it is turned out of that line so as to run in a curve by an extremely small force; and, when the axis is kept in the direction of a radius to any curve, the wheel traverses that curve with very little resistance. Two wheels of the same size, keyed fast on the ends of the same axle, roll, upon a flat surface, in a straight line at right angles to their axis; and they cannot be turned out of that line without causing one wheel to slide backwards or forwards. When only the outer one of two such wheels is guided around a curve by a rail the inner wheel outruns the outer until the axle becomes parallel with the rail, or at such angle that the wheels cease to revolve. To enable two wheels on the same axle to run around a curve without sliding, it is necessary to make one of them larger than the other, or to make one or both of them loose on the axle.

Two wheels, mounted one in front of the other (as in a bicycle), on parallel axles, also roll in a straight line at right angles to their axes, and any deviation from this straight line can only be accomplished by causing one wheel to slide sideways. If two such wheels on parallel axles are caused to run in a curve by a rail or guide acting on the front wheel, that wheel on first entering the curve

slides sideways in an inward direction toward the center of the curve, but still with a direction giving it a tendency to run outwards. The hind wheel by this motion is turned inwards, until the axis assumes a radial direction, when it follows the front wheel, though not exactly in the same track, being nearer to the center of the circle. This seems to be the only combination of equal and parallel wheels in which, the front wheel being guided in a curve, the other or others take up a definite relative position, whether they are loaded equally or unequally. In all other combinations, the positions of the wheels depend upon their loads and the state of the surfaces as regards friction, owing to the sometimes conflicting tendencies of all the wheels, except the front ones, to run with their axles radial, and of all the inner wheels to outrun the corresponding outer ones.

To allow two wheels, one in front of the other, to travel around a curve without slipping, the axles must be made to converge towards the center of that curve. When three wheels, on the same plane, are mounted on parallel axles, and the front wheel is caused to move in a circle, the middle and hind wheels have each a tendency to run with its axis in a radial direction; but as it is impossible that they can both do this, they take up positions such that their plane forms a tangent to the curve, either at the more heavily loaded of the two wheels, or at a point somewhere between them, nearer to the more heavily loaded wheel. Thus a carriage on four or more equal wheels, fixed on two or more parallel axles, will, on a plane surface, run in a straight line, and can only be turned out of that line by at least two of the wheels being made to slide backwards and forwards, and at least two to slide sideways, one of which is at the same time sliding backwards or forwards; so that at least three wheels (or rather all the wheels but one) must be sliding.

As stated in a former discussion, "A model carriage, having two pairs of wheels fixed on parallel axles, the diameter of the wheels on one side of the carriage being less than those on the other side—this model carriage being placed on a board, and one end of the board lifted until gravity caused the carriage to run down—it was found that the carriage ran straight, and not, as might have been expected, in a curve." This, however, is the case only when the carriage has a comparatively long wheel-base. When the wheel-base is less than the width between the wheels, the carriage travels in a curve. The shorter the wheel-base, the more nearly does the curve seem to approach that proper to the cone formed by a pair of wheels, and the carriage turns in this direction even although the wheel-base is longer on the side next the small wheels than on the other side. The model thus seems to travel in the direction which causes the smallest amount of sliding, whether this sliding takes place in the direction of the planes of the wheels, or of their axes; that is to say, along or across the rails. Therefore, in a carriage or engine with a long wheel-base, it would be a greater advantage to have the axles radiate than the wheels to run loose on the axles; while, with a broad gauge and a very short wheel-base, it would be better to have loose wheels than radiating axles. This is more evident if the proportions of wheel-base and gauge are carried to their extreme limits.

Suppose the wheel-base of a four-wheeled carriage to be the longest possible, namely nearly equal to twice the radius of the curve on which it runs, and the gauge to be very narrow, then, if the axles are parallel, the wheels will be nearly at right angles to the rails, and will not roll, their whole motion being sliding sideways; while, if the axles are radial, the wheels on each, though of the same size, being close together, will roll around the curve without much friction.

Suppose the gauge to be the widest possible, namely nearly equal to the radius of the curve, and the wheel-base extremely short, then the axles will be so nearly radial, that the wheels will roll around the curve with very little sliding in the direction of their axes, or across the rails; but if they are all of one size, to enable them to run without an excessive amount of sliding in the direction of the rails, they would require to be made loose on their axles, since the inner wheels, being close to the center of the curve, have hardly any distance to travel, and so hardly need revolve.

When a carriage or engine with four equals on two parallel axles is running on

a curve at slow speed (or at such speed that the cant of the rails balances the centrifugal force), the carriage not being attached to a train, and there being no play between the flanges of the wheels and the rails, then both axles are parallel to a radius half-way between them, and none of the wheels are parallel with the rails on which they run, the leading wheels standing across the rail in a direction giving them a tendency to run outwards, and the trailing wheels at an equal angle in the opposite direction, with a tendency to run inwards. Suppose, now, the flanges were removed from all the wheels, the carriage would run straight forwards at a tangent to the curve, and the path traced by the rail on the circumference of each wheel would, for a short distance, be a helix or screw, with an obliquity or pitch corresponding with the angle formed by the wheel and rail. If the helix is right hand in the case of the leading wheels, it will be left hand in that of the trailing wheels. The wheels, however, being prevented by their flanges from running straight on, slide across the rails to the extent of the pitch of the helix during each revolution, the leading wheels sliding inwards towards the center of the curve, and the trailing wheels outwards. The flange of the outer leading wheel is thus forced against the rail by a pressure sufficient to cause both the leading wheels to slide inwards across the rails; and the flange of the inner trailing wheel is acted on by a pressure sufficient to cause both trailing wheels to slide outwards across the rails.

Owing to the difference of the lengths of the inner and outer rails, either the wheels on the inner rail, besides sliding across the rail, will also slide backwards, or those on the outer rail forwards. The former is probably the case in ordinary working, when an engine with all wheels coupled is dragging a load, and the latter when running by momentum or down an incline with steam shut off. For the present purpose, however, it is only necessary to consider which will be the case when the flange of the outer leading wheel is in the act of mounting the rail. At that time, the whole of the load on that wheel is borne by the adhesion of its flange, and the tread is relieved of all load and adhesion, so that the wheel is running on a diameter larger than that of the tread, and larger, on almost any curve, to a sufficient extent to allow the inner wheel to revolve at the speed due to its tread, running on the shorter length of the inner rail. If all the wheels are coupled, the inner trailing wheel also revolves at the speed due to its tread, leaving the outer trailing wheel revolving at a speed slower than that due to its tread, running on the greater length of the outer rail. In this case it is probable that this one wheel slides forward on the rail, the whole of the wheels thus revolving at the speed due to their treads, traveling over the length of the inner rail.

The outer leading wheel thus slides forwards, and at the same time inwards across the rail; the inner leading wheel slides only inwards across the rail; the outer trailing wheel slides forwards and outwards across the rail; the inner trailing wheel slides only outwards across the rail. The sliding motions of the wheels are thus similar to those which would take place if the engine, without moving forwards, revolved horizontally about a center situated half way between the two inner wheels; and the distance which any wheel slides along and across the rail while the engine makes a complete revolution, is the same, or nearly the same, as that which it slides while the engine is running around a complete circle of any diameter, however large. Thus the energy expended in overcoming the friction of the treads of the wheels in going around a circle is a constant quantity, whatever may be the size of that circle. From this it follows that, as generally admitted, the additional tractional resistance due to that friction varies inversely as the radius of the curve; but it does not follow that the tendency to leave the rails increases in anything like this ratio.

When, by the force of traction, the inner wheels are caused to slide backwards, the sliding motions of the wheels will take place around a point situated between the two outer instead of the inner wheels.

Again, when the clearance between the flanges of the wheels and the rails is sufficient, the trailing axle runs forwards until it assumes nearly the direction of a radius to the curve. It may be slightly in advance of this position, or slightly

behind it, the precise position depending upon the comparative loads on the wheels. and the proportion of wheel-base to gauge. In this case, supposing as before that the outer wheels slide forwards, the outer leading wheel slides inwards across the rail and at the same time forwards; the inner leading wheel slides only inwards across the rail; the outer trailing wheel slides only forwards; and the inner trailing wheel does not slide at all. The sliding motions of the wheels on the rails are thus nearly the same as those which would take place if the engine revolved about the inner trailing wheel. The flange of the inner trailing wheel is thus relieved of all side pressure, and the only flange pressing against the rail is that of the outer leading wheel, which must thus exert the whole of the side pressure required to cause the sliding of its own wheel, of the inner leading wheel, and of the outer trailing wheel. The leading wheels now run at a greater angle with the rails than they did when the gauge was tight; and the distance which the wheels slide in going around a circle is greater.

A carriage with four, six, or more wheels on parallel axles, does not absolutely require flanges on any except the leading wheels, which being thus guided, the other wheels will follow them, and, if wide enough, will keep on the rails.

Thus, when there is clearance enough, or the curve is easy, the whole of the flange friction comes on the outer leading wheel.

Appendix III.

In the case of a six-wheeled engine running, without exerting tractive force, with the inner driving-wheel flange in contact with the rail, it is even more probable than in the case of a four-wheeled engine, that the outer wheels slide forwards, and that the inner ones do not slide backwards. Assuming the wheels to be nearly equally loaded, the weight upon the driving wheels is not sufficient to enable their adhesion to cause them to form a fulcrum to move the trailing wheels across the rails. Thus, the inner driving-wheel flange is pressed against the rail, causing additional adhesion, to overcome which, in the event of the inner driving wheel sliding backwards, an additional pressure would be exerted by the flange of the outer leading wheel. This, in turn, causes an additional pressure on the inner driving-wheel flange, so that the two wheels act and react upon each other, and the side pressure on the inner driving-wheel flange causes the adhesion of that wheel to be greater than that of the outer one.

Appendix IV.

With a tire having a flat or conical flange sharply joined to a cylindrical or slightly conical tread, or with almost any tire running on a rail considerably rounded on the top, there are two points of contact, one between the top of the rail and the tread vertically under the wheel's axis, and the other between the side of the rail and the flange some distance in advance of the axis when running towards the rail on a curve.

With a tire whose flange is joined to the tread by a curve of considerably greater radius than that of the rounded corner of the rail, or with almost any tire running on a flat-topped rail, the two points of contact merge into one when the wheel is running towards the rail on any ordinary curve, the wheel resting on the rounded corner of the rail at some point determined by the proportion of weight to side pressure, and this point is slightly in advance of a perpendicular from the wheel's axis. In this latter case flange-friction can hardly be said to have a separate existence.

Appendix V.

Had the engine been exerting a considerable amount of tractive force, the case would have been altered, perhaps, somewhat as follows: The tractive force being, as regards the adhesion of the wheels, equivalent to a strain pulling the wheels backwards, the inner driving and trailing wheels would slide backwards, instead of the outer ones forwards. The backward pull would therefore relieve the outer leading wheel of the side pressure required to effect the longitudinal sliding motion, leaving only that required to effect the cross sliding. This pressure would thus be reduced:

For the outer leading wheel $0.0(4.05 \times 7.16 = 29 = N)$	$= 0.0$
For the inner leading wheel $0.2(3.5 \times 7.16 = 25.060) \div 7.16$	$= 0.70$
For the outer driving wheel	$= 0.0$
For the inner driving wheel	$= 0.0$
For the outer trailing wheel $0.2(3.75 \times 7.16)$	$\div 7.16 = 0.75$
For the inner trailing wheel $0.2(4.0 \times 7.16)$	$\div 7.16 = 0.80$
Tons	2.25

Assuming, however, that the drag-link was attached to the engine at a considerable distance behind the driving axle, then the backward pull would give this part of the train a tendency to straighten out, and so increase the side pressure on the outer leading wheel. Assuming the backward pull to be equal to one-fifth of the weight of the engine, or $\frac{25.65}{5} = 5.13$ tons, acting at 11 feet behind the driving axle, where the coupling would form an angle of about 179° with the center line of the engine, it would cause an inward pressure at the coupling of $5.13 \times \sin. 179° = 0.089$ ton, which, supposing the engine to be revolving about either driving wheel, would cause an additional outward pressure on the outer leading-wheel flange of $\frac{0.089 \times 11 \text{ feet}}{7.16 \text{ feet}} = 0.14$ ton, which, added to 2.25 tons brings up the pressure to only 2.39 tons. Thus the side pressure on the outer leading-wheel flange is considerably less when an engine is exerting its tractive force, than when running with steam shut off.

THE SUEZ CANAL—ITS ENGINEERING, COMMERCIAL, AND POLITICAL ASPECTS.

By Lieut.-Gen. F. H. Rundall, C.S.I., R.E., late Inspector-General to the Government of India.

From the "Journal of the Society of Arts."

The Suez Canal has of late occupied, and is still occupying largely, the attention of all Europe—and, perhaps it may not be too much to say, of the whole civilized world. At the same time notwithstanding so much has been written and talked of about it, beyond the fact of its being the great thoroughfare to India for the ships of all the Western nations, but little comparatively regarding it is known to the public generally.

The mercantile world, and those more particularly interested in the Canal, can of course obtain from the Parliamentary Blue-books a mass of important information and statistics not otherwise available, more especially as regards the official negotiations which have passed between the various Governments in reference to the commercial and political aspects of the undertaking. But Blue-books are not attractive reading, even to those who have leisure, while business men seldom have time sufficient to devote to their study. The Society of Arts, therefore, being desirous of procuring a succinct, and at the same time correct and comprehensive, account of the Suez Canal in all its aspects, an endeavor is made, in the following remarks, to meet that desire, by collating, from official and other publications, the most important of the particulars available, and, at the same time, to consider the various bearings of a question which is now attracting such universal attention, and which is generally felt to be of primary importance to England at the present time.

The Suez Canal has to be viewed in three aspects:

1. As an engineering work.
2. As a commercial undertaking.
3. As a political problem.

Before commencing to discuss these three points in detail, I propose to give a short history of the scheme, from the period of its first incubation, to its present stage of development.

The first idea, in modern times, of connecting the Mediterranean with the Red sea seems to have originated with Napoleon I., but before he could give any practical shape to that idea, the Emperor had to evacuate Egypt.

Nothing further was attempted till the year 1842, when Captain James Vetch, of

the Royal Engineers, published a pamphlet, in which he indicated the direction a canal should take across the Isthmus, and its vast importance to this country; but the Government of the day were opposed to the construction of any canal, and Captain Vetch's suggestions fell to the ground. Subsequently, in the year 1846, a Commission of engineers assembled to discuss certain proposals, which, however—beyond exposing the error as to a difference of level of thirty feet between the Mediterranean and Red Sea—came to nothing, and so the subject dropped until 1854, when it was revived by Monsieur de Lesseps putting forward his project for a direct canal between the two seas. His proposals met with considerable opposition in England, strangely enough from commercial authorities, who foresaw the total revolution that would be made in the carrying trade, but who were apparently unable to appreciate or believe in the enormous advantage that would accrue to them from such a revolution. But there was likewise a formidable political opposition, based on certain objections which the Government of the day foresaw would, unless duly safeguarded, be likely to give rise to considerable difficulties in the future. That those objections were not destitute of foundation, every statesman of the present day will readily admit; and the difficulties which are now attendant on current negotiations only too plainly indicate that a scheme which at first apparently concerned only a small body of speculators, has turned out to be a political problem, perplexing more or less the whole continent of Europe; but the difficult task of solving which has, by a general consensus of opinion, devolved, and rightly so, upon England.

This phenomenon is not without its prototype. Our Eastern Empire, as is well known, had its first beginnings in the ventures of a few enterprising merchants, whose marvelous prosperity, in spite of themselves, raised the East India Company to the highest importance as a political factor, stimulated its acquisitive energy until its operations got beyond control, and the Merchant-princes of the East found themselves overturned in the great convulsion which, in 1857, shook the Empire to its foundations, and so nearly involved the whole of the gigantic fabric in ruins. It is, to say the least, a curious coincidence that, ere India had recovered from the throes of that convulsion, an enterprise like the Suez Canal, originated by private speculators, and the interests and prosperity of which are so closely dependent on those of India, should be started into existence, and, after nearly foundering, should, in a brief time, enter on such a career of prosperity, become a link between gigantic interests, and in its turn rise to be an important factor in the political world; and what is stranger still, instead of proving, as was anticipated, a spear for piercing its armor, it has been converted into the first line of defence for England—a veritable covered way for safe-guarding its Eastern possessions! Is the parallel to be pursued any further? Doubtless the projector would exclaim, "*Dii omen avertant*!"

But to return to the history. In 1859, Monsieur de Lesseps' scheme was actually begun, and after successfully combating many difficulties, both physical and political, the Canal, though incomplete, was finally opened from sea to sea ten years afterwards, in the year 1869, with great pomp and ceremony, in the presence of a brilliant Imperial assemblage. From 1869, to the present time, the Canal has been in constant use, without a day's intermission. Its career has not been altogether unchequered; but it has emerged from its difficulties, and has now reached a pitch of prosperity exceeding the most sanguine expectations of its projector.

To pass on to the consideration of the Canal as an engineering work. It is unnecessary, as well as out of place here, to enter into minute details of construction, and therefore only its most general features will be brought under review. There is little room to doubt but that the engineers who were entrusted with the surveys, selected, on the whole, the most favorable line for the Canal, as regards economy of first construction. The entrance from the Mediterranean is between two long piers, or moles, run out from the coast, with the idea partly of creating a species of harbor, in which vessels approaching the Canal might lie in quiet water. The western mole, however, was also intended to serve as a protection against the entrance becoming silted by

the Nile deposit, which is transported during the floods of that river in enormous quantities along the coast for many miles.

It may be open to comment whether, instead of placing the entrance from the Mediterranean at Port Said, it might not have been better made more to the eastward, and further removed from the influence of the silt-laden waters of the Nile. It is said that the difficulties connected with the silt deposit at that entrance have been overcome, but that assertion must be received with caution; for it is quite certain that the coast line between the Damietta mouth and near the Canal has advanced considerably, while the 5-fathom line had receded, as noticed in Sir John Stoke's Report in 1874, 1,200 yards in three years. The diagram on the wall is on too small a scale to show the present position of that line, and the rate at which the intervening space is being filled up, or how far the statements of Monsieur Lemasson, the French engineer, are borne out, as to the effect that a deposit of 7½ million cubic meters per annum was likely to produce. The western mole has, it is believed, now reached a length of nearly 3,000 yards, and to where the 5-fathom line used to lie. Of course, as deeper water is reached, the process of silting up will become slower, but that it cannot be wholly arrested by mere extensions of the western pier, though accompanied by dredging, is incontrovertible. Hence it may be admitted that the further the entrance could have been removed to the eastward, the less inconvenience would have been experienced from the translation of the Nile deposits.

With the exception of some unnecessary curves, especially that near the town of Ismailia, the general alignment of the Canal is satisfactory; but since the traffic began to increase, the mistake of making those curves has become apparent. It must be admitted, however, that the mistake has been aggravated rather by the continually increasing length given to vessels which have been (and are still being) built for the navigation of the Canal.

The total length of the line from Port Said to Suez measures 88 geographical, or 100 land miles, of which 39 miles only are in cuttings through land, the other 61 miles running through a succession of lakes. The northern half of the Canal, as far as Lake Timsah, had to be excavated by manual labor, until sufficient depth could be obtained to admit of dredging operations; but the portion south of Lake Timsah was excavated entirely by dredgers, as water was obtained on a high level from the Freshwater Canal, which had, meanwhile, been constructed; and thus by filling up the line of the Suez Canal to that level, sufficient depth was obtained for floating the dredgers. At El Guisr, Serapeum, and the Chalouf plateaux—in all, about eighteen miles of cutting—rock (more or less hard) was encountered, rendering excavation both difficult and costly, as blasting operations had to be resorted to. Though along the rest of the line the material to be excavated consisted principally of sand, with an occasional admixture of clay, yet the task of not only excavating soil to a depth of 26 feet below water, but of conveying it away, involved some difficulty, which was finally overcome by some very ingenious adjuncts and appliances to the ordinary dredgers. The excavated material could not, as in the case of river or harbor dredging, be simply lifted into hopper barges, and be towed out and discharged into the sea; it had to be deposited on banks on either side, the height and breadth of which were, of course, continually increasing.

According to Lieut.-Colonel (now Sir A.) Clarke's report, written in February, 1870, it was originally intended to construct the Canal with a surface width of 325 feet, and a bottom width of 144 feet; but the latter, at the present time, does not average more than 70 feet. The side slopes, which vary according to the soil passed through, are being now faced with stone, by which means a considerable length of the Canal is protected from the erosion which used to take place from the wash of the steamers.

A minimum depth of 26 feet is maintained throughout the Canal. The bottom width being only half that originally intended is, of course, insufficient to allow of vessels passing one another, and obliges a low speed to be maintained.

Sir John Stokes observes, in a report dated April 20, 1876, that, in his opinion, the narrow width is not altogether a disadvantage, inasmuch as by necessitating the Canal being worked on the block sys-

tem the risk of collision is greatly reduced, and the passage of vessels is, in the long run, performed in a shorter space of time than if vessels were allowed to navigate independently. Unquestionably, the risk of collision is especially to be avoided where large vessels are concerned, for the momentum of masses 2,000 to 4,000 tons in weight, moving even at a low velocity, is exceedingly great, and when brought into collision must be attended by disastrous results. But the narrowness of the section operates disadvantageously in retarding the speed of vessels below the economical unit. When the difference between the sectional area of the Canal and that of the vessel's midship section is so small, the piling of the water caused by the vessel's displacement is greatly increased; the friction on the sides of the vessel, as well as on those of the Canal, so retards the flow of the water that the void created by the vessel's onward movement cannot be filled sufficiently fast, and hence the steering power is destroyed, and the ship either takes the ground astern or runs bow on to the bank.

By experiment on the Indian Canals, it was found that when the relative sectional area of the vessel and canal were in the proportion of from 1 to 12 or 14, with a depth of 3 to 4 feet under the steamer's keel, there was no perceptible piling, and a speed of from 8 to 10 miles an hour could be maintained, but wherever that relative proportion fell short, piling began to take place, and the speed was proportionately diminished. If, therefore, the Suez Canal were finished more nearly to its original dimensions, or say, to 320 on the water line, and 200 at the base, with slopes of 2 to 1, it would, if 30 feet deep, contain a sectional area of 7,800 square feet, and yield a proportion of 13 to 1 to all vessels whose midship section did not exceed 600 square feet, and which would probably include all ships registering 2,000 tons. The block system, if found absolutely necessary, might still be maintained, but it would give the opportunity for such vessels to accomplish the passage of the Canal in twelve hours of daylight. The narrowness of the bottom width of the Canal not admitting of large vessels passing one another, side-cuttings, called "Gares" or stations, have been made at intervals of five miles, wherein large vessels can lie moored during the night, or whenever the necessity may arise. As observed before, the method of working the Canal is that known on railways as the block system, no vessel being allowed to proceed from one station to another until the line is signaled clear, for which purpose a perfect telegraphic communication is kept up through its entire length. The arrangements for carrying this out are described in Sir J. Stoke's report, as follows:

"Each of the superintendents of the transit at Ismailia, Suez, and Port Said has a model of the canal in his office, with miniature vessels, which enable him to fix the position of each ship as it passes through. As soon as a vessel enters the canal, either at Suez or Port Said, its counterpart is launched on the models with name affixed. As it passes each siding, which is also a telegraph station, its position is made known to the superintendents, who fix its place on the model, and the chief transmits orders for the guidance of the pilot on board. Then, whenever a vessel approaches a siding, it finds a signal directing its movements, whether it is to remain or move; if to remain, the orders are strict that it is to make fast to the bank in the siding, and to leave the navigable channel quite free."

If these orders are acted up to, it is impossible for two vessels to meet in the section between two sidings, and so collision is rendered impossible. The canal is also furnished with every accessory in the shape of light-houses, beacons, buoys, telegraphs, &c., for facilitating the navigation as far as possible.

It would have been impossible to carry on the work of the Maritime Canal without a supply of fresh water, which was wanting along the whole line. The company was therefore empowered by the Egyptian Government to construct a canal for irrigation and navigation from the River Nile, at a point below Cairo to Timsah, and thence to furnish a supply for the towns of Port Said and Suez. This canal has since been enlarged and completed with locks, and is the work which proved of such invaluable aid in the late military operations in Egypt. The extensive tract of land granted to the company, and for which irrigation

had to be supplied, was subsequently taken back by the Government, and the company empowered only to use the water for domestic purposes.

So far, then, as an engineering work, it would leave little to be desired, had vessels of the type in vogue before its construction continued to be used. But as the science of shipbuilding progressed, it has been found possible and necessary, in order to keep up with the requirements of trade, to construct vessels of much larger dimensions, and on different lines; and, consequently, ships have outgrown the existing dimensions of the canal, and demand increased accommodation. It has been seen that the original intention was to construct the canal of double the present dimensions, but the financial necessities of the company prevented them from carrying out that intention. Though forced upon them, the course adopted was quite the right one; for the canal, on the smaller scale, has thoroughly served its purpose for thirteen years; and now that it is placed in a highly satisfactory and flourishing financial condition, the time has arrived when additional capital may prudently be laid out, to increase the accommodation which a rapidly-increasing trade demands. The best means of providing additional accommodation is still a matter under consideration, notwithstanding certain measures have been publicly announced in some of the daily papers as having been definitely settled. Probably the company, finding that it will be to their own interest not to postpone any longer meeting the demands made upon them, have decided on carrying out meanwhile certain obvious improvements; but the larger and more permanent provision, that must sooner or later be made, has yet to be settled. The possible alternatives that present themselves will be explained hereafter. Meanwhile, there only remains to be noticed, under this head, the items of cost of original construction, repairs and supervision. The total capital expenditure up to 1869, the year in which it was opened, amounted to £18,000,000. The sum spent annually on repairs and improvements carried out from revenue, does not vary very much, amounting, in 1880, inclusive of supervision, to £218,900; and, in 1881, to £212,490, thus averaging less than 1¼ per cent. on the first cost—a very moderate and satisfactory outlay.

Passing on now to consider the canal in its second aspect, as a commercial undertaking, there is much of a very interesting character to be explained. The first fact is one which, outside official circles, will probably be entirely new to 99 persons out of 100, and that is that La Compagnie Universelle du Canal Maritime de Suez (The Universal Suez Maritime Canal Company) is not a French but an Egyptian company, having its principal office at Alexandria, amenable to the laws and customs of not the French, but of the Turkish Empire, and, therefore, subject to the jurisdiction of its local tribunals. The importance of this fact will be sufficiently evident, when the disputes which subsequently arose between the directors of the company and the Egyptian and Turkish Government come to be noticed. It is not too much to add that, but for this precaution at the time of the formation of the company, it would not only have been impossible for the nations of Europe to make use of the canal, but that the subsequent attempted exactions of the company might have led to very serious misunderstandings, if not to actual embroilments, amongst some of the European Powers. Before proceeding, however, to explain the more especial commercial features of the canal, it is necessary to draw attention to the nature and terms of the various "concessions" granted by the Egyptian Government, and subsequently ratified in a firman by the Sultan as suzerain. Eight concessions were granted by the Viceroy of Egypt between the years 1854 and 1869, the details of which have been published in the Parliamentary Paper, "Egypt, No. 6 (1876)." In the first of these, dated November 30th, 1854, the Viceroy confers on Monsieur de Lesseps "exclusive power to form and direct a universal company for piercing the Isthmus of Suez, and the working of a canal between the two seas within, and subject to certain specified restrictions and conditions," the most important of which, as regards the constitution of the company, were:

1. That the director of the company was to be always named by the Egyptian

Government, chosen, as far as possible, from amongst the shareholders most interested in the undertaking.

2. That the duration of the concession was to be for 99 years from the date of the opening of the canal throughout.

3. That the Government reserved to itself the power to erect any fortifications it pleased in the neighborhood.

4. That the Egyptian Government were to receive, yearly, 15 per cent. of the net profits shown in the balance-sheet of the company, without prejudice to any dividends belonging to the shares which the Government reserves a right to take on its own account, and without any guarantee on its part.

6. That the rest of the profits were to be divided as follows: 75 per cent. for the benefit of the company; 10 per cent. for the benefit of the original promoters.

7. That, on the expiration of the concession, the Government to succeed to the company, enjoy all its rights, and enter into full possession of the canal.

8. That no alterations could be made in the statutes of the company without receiving the previous approbation of the Viceroy.

The second concession, dated 5th January, 1856, is an amplification of the first one, explaining, in more detail, the nature of the charges and concessions, the most important of which are:

1. The limitation of the power of the company to carry out a canal east of the Nile, from the Gulf of Pelusium, and authorizing the construction of a fresh-water canal from Cairo to Lake Timsah.

2. The solemn declaration of the Viceroy, subject to ratification by the Sultan, of the Maritime Canal and its ports being always open as neutral passages for all merchant ships on payment of dues, without preference to any vessel, company, or person.

3. The dues on vessels navigating the canal not to exceed 10 francs per ton of capacity and per head of passengers.

This concession was made over to Monsieur de Lesseps, previous to ratification by the Sultan, to enable him to constitute the financial company. Amongst the statutes of the company approved by this concession was one appointing its seat at Alexandria, and its administrative domicile at Paris.

The statutes direct that the annual proceeds were to be applied in the following order:

1. To expenses of maintenance, working and administrative, and generally all charges of the society.

2. To interest and sinking fund of such loans as might be contracted.

3. Five per cent. on capital of society to be applied to provide for shares redeemed and not redeemed, and an annual interest of 25 francs per share; the interest attributable to shares redeemed being carried to the sinking fund.

4. Four hundredths per cent. on the capital to be applied to the sinking fund.

5. To the amount required for creating and keeping up a reserve fund for unforeseen expenses. The excess over the above proceeds to constitute the net profits, which were to be distributed as already stated.

Concession No. 3, dated 6th of August, 1860, is a financial agreement containing the first settlement of accounts with the Egyptian Government respecting its subscription; and, in March, 1863, concession No. 4 contained another financial agreement for settling the balance payable on shares subscribed for by the Egyptian Government.

At this period intervenes an important act by the Porte, which perceiving in the concession of the fresh-water canal, and of vast tracts of land, a menace to its independence, and in the stipulations for the providing of workmen, a violation of the laws under which the Ottoman Empire is governed, declared by a diplomatic note, dated 6th April, 1863, addressed to its representatives in Paris and London, its opposition to the continuance of the works. The same notification was made to the Viceroy, who communicated it to Monsieur de Lesseps, expressing his intention to treat on and settle these questions with the company, and accrediting Nubar Pasha to come to an understanding on the proposals to be submitted. The Council of Administration rejected the proposals, and then the Khedive begged the intervention of the Emperor of the French, who, after receipt of the report of the Commission which he appointed to examine into the questions involved, gave his decision; awarding to the company a sum of 84 millions of francs, or £3,360,000, the payment of which was to be spread over

a period of fifteen years, ending with 1st of November, 1879.

Consequent on this award, a sixth convention was drawn up, dated 30th January, 1866, in which the following important articles occur:

1. Egyptian Government to keep all strategical points.

2. Power for the Government to occupy sites for post-offices, customs, barracks, and other services.

4 and 5. The Government to take possession of the fresh-water canal, on the terms specified.

A further agreement was drawn up in the following February, and received the sanction of the Sultan in a firman, dated 19th March, 1866, which stated the conditions to which such a sanction was subjected. One important article being to the effect:

"That the Maritime Canal and its appurtenances remain subjected to the Egyptian police, which shall exercise its functions freely, as at every other point of the territory, so as to secure good order, the public peace, and the execution of the laws and regulations of the country."

In Article 16 occur the words: "The Universal Company of the Maritime Canal of Suez, being Egyptian, it is governed by the laws and usages of the country;" and the articles which follow declare its complete subjection to the Egyptian Government. Two agreements, dated 23d April and 26th January, 1869, complete the negotiations prior to the opening of the canal, and in them is the arrangement by which the Viceroy assigns fifty coupons from each of the 176,662 shares belonging to the Egyptian Government, for the space of twenty-five years, in settlement of the 30,000,000 francs, the value awarded for the resumption of the fresh-water canal from the company.

The recapitulation of the concessions though somewhat tedious, is essential to the right understanding of certain events which occurred shortly after the opening of the canal, as also of the obligations that regulated the amount of dividends which the company paid to the shareholders, as well as of the status of the company generally.

Turning now to the cost of the canal, already stated to be £18,000,000, it appears that of this, the amount of capital originally subscribed was only £8,000,000, in 400,000 shares of £20 each. That amount was subsequently augmented by the loan of 1867 for £4,000,000, being debentures ("obligations") redeemable in fifty years, bearing 5 per cent. interest, and having a first charge on the profits. After these, the loans of 1871 were contracted by the issue of thirty years bonds, bearing 6.7 per cent. interest, which, in their turn, were succeeded by £1,360.000, representing consolidated arrears of interest on ordinary shares, in the shape of seven coupons of 5 per cent. yearly. Lastly, came the advance on detached coupons of the Egyptian Government shares amounting to £1,200,000, styled "delegations," and entitled, for twenty-five years, to 5 per cent., and a dividend (if any) on those shares. The balance of the cost of the canal was made up from the indemnities which had been paid by the Khedive under the late Emperor's award. In 1870, the year after the opening of the canal, the prospects of the company being doubtful, a suggestion was made by the Khedive that it should be transferred to some English company.

The negotiations which followed this suggestion belong rather to the political than the commercial aspect of the canal, and will be better adverted to hereafter.

The doubtful prospects of the company, however, led to a very objectionable proceeding on the part of the Board of Administration, viz., in determining arbitrarily the interpretation of the term, "ton of capacity," on which the authorized due of ten francs was leviable. That due had been for the past three years, levied on the net register tonnage, as shown in the ship's papers, and calculated on the Moorsom system, which, after reckoning the gross tonnage of the ship, deducted the space occupied by the engines, fuel, &c. The term, "ton of capacity," not having, however, been actually defined at the outset, the company took upon themselves to interpret that term as signifying the "real capacity" of the ship, which, by a commission of their own appointment, they declared to be greatly in excess of the net register tonnage, the result being that the dues were levied on the gross tonnage, and consequently the tolls payable by every

ship traversing the canal were raised 30 per cent. The burden of such an interpretation at once made itself felt in the case of steamers, in which the machinery occupies one-third of the tonnage, and was therefore equivalent to levying 50 per cent. more from steamers than had been levied in the three previous years. Remonstrances were immediately put forward by the principal maritime companies in England, and the Messageries Nationales actually brought an action against the Canal Company in France. A decree was first given in their favor, but it was afterwards reversed on appeal.

The protest, however, by the British Government led to the assembly of an International Commission at Constantinople, and to the adoption of a style of measurement according to what are known as the Constantinople Rules. It was not, however, without great discussion, in which the tact and firmness of the English delegates, Sir John Stokes and Sir P. Francis, were eminently conspicuous, that the proceeding of the Canal Company were set aside, and the Moorsom system of measurement declared to be correct, and adopted accordingly.

Finding themselves entirely in the minority, the French delegates eventually joined in signing the report of the Commission, which set forth the rule to be followed in the measurement of vessels, and the deductions to be made in order to arrive at the net tonnage. As a set-off, however, against the diminished revenue which it was supposed would follow the first introduction of the new rules, the Commission recommended permission being given to the company to levy temporarily a graduated surtax of 3 francs per ton on vessels provided with the prescribed certificate of net tonnage measurement, on condition that the deduction did not exceed 50 per cent.

Vessels not measured according to the Moorsom system were to have their tonnage reduced by calculation to the scale of the Danube, and to pay a surtax of 4 francs per ton, as also vessels measured according to Section A, Clause 23, of the British Merchant Shipping Act of 1854.

The surtax of 3 francs to be reduced in the following proportions: To 2½ francs per ton, as soon as the net tonnage passing through the canal reached 2,100,000; to 2 francs when it reached 2,200,000; and so on, ½ franc for each annual increase of 100,000 tons up to 2,600,000 tons, at which figure the surtax was to cease altogether, and the original due of 10 francs per ton only to be levied. Vessels of war, transports, and vessels in ballast to be exempt from the surtax.

The Porte was pleased to approve of the report of the Commission, and desired the Khedive to inform the company of the same, and direct it to be put in force in three months time.

Monsieur de Lesseps first protested, and then refused to accept the new rules; but, as in the event of his persisting in his refusal the canal was to be seized by the Egyptian Government, he submitted, but under a protest, which was not withdrawn till some years afterwards.

Towards the close of the following year a rumor got abroad that a project was on foot to purchase the shares of the Canal Company belonging to the Egyptian Government. This led to the correspondence which is given in detail in Parliamentary Paper, Egypt, No. 1 (1876), and which ended eventually in the purchase of those shares, 176,602 in number, for a sum of £4,000,000.

In 1876, after the purchase had been concluded, and in consequence of a new convention, the company withdrew all opposition, accepted all that had been laid down by the Constantinople Commission, and agreed to the decrease of the surtax at fixed dates, and to its total abolition in 1884, and consented to expend one million francs per annum for thirty years in improving the canal. That convention, which is known as the Stokes-Lesseps Convention, has consequently been in force up to the present time.

It is time, now, to turn to that part of the subject which possesses the greatest interest for the English public, and that is the traffic passing through the canal, and the revenue derived therefrom; for it indicates not only the relative importance of the canal to the several nationalities which make use of it, but it incontestably proves that were it not for the trade carried by British vessels, the Canal Company must, long ere this, have entirely collapsed, and were it now

to be withdrawn through any other channel, the Suez Canal could not possibly be maintained.

Seven years ago an interesting paper was read before the Society, by Mr. Charles Magniac, on the "Commercial Aspects of the Canal," in which, on the whole, he took rather a depressing view of the prospects of British trade at the time, saying: "That the returns up to that time showed that the great expectations entertained of a large increase of trade due to the canal, had not been borne out;" "that the imports into India were feebly stationary" "that the falling off from France was immense, but greatly due to the effects of war;" "that the item most deserving of attention was that of the Mediterranean ports."

Similarly, in regard to the exports, the United Kingdom told the same story of stagnation, and it was only the Mediterranean ports that showed a buoyant and increasing trade, and that principally in the staple articles of Indian produce. But he summed up his description with the words, "Whatever we may say or do, however much industries or individuals may suffer at first, one thing we do know, that India has been brought within three weeks journey of England. We gained our supremacy there when a letter and its reply were frequently 12 months on the road. The same thing can now be done in 12 minutes. Is it to be supposed that this energetic nation will not find its advantages in such facilities as these? I am sure it will, and that even many here will live to see it." Mr. Magniac has doubtless ere this already satisfied himself of the verification of his prediction; but as possibly many are present who have not had the same opportunity, I propose briefly to call attention to the figures in the several tabular statements of the trade of India with England and the world generally, obtained from published official records of the Indian Government.

First, in regard to the imports of the United Kingdom; these show a steady increase from 1874, when they stood somewhat over 29½ millions, to over 32 millions in 1879–80, with the exception of the previous year, when, in consequence of the depression caused by the famine, they fell just below 29 millions. In 1880–81 they rose to £41,300,000, or an increase of over 33 per cent. In the foreign trade, France increased from £362,400 to £705,600, and Italy from £334,000 to £576,000.

From the rest of Europe the import trade is insignificant. The export trade to the United Kingdom does not show a similar increase in value, owing to the great fall in prices, but an examination of the quantities exhibits a satisfactory progress, while the exports to the principal European countries is more marked as regards increase in value. France having risen from £3,134,000 to £6,500,000; Mediterranean ports—Italy, £1,400,000 to £2,781,000; Austria, 1,428,000 to £2,226,000.

In five of the most important staples, cotton, rice, seeds, tea and wheat, the progress in exports has been as follows:

	1876–77.		1880–81.	
	Quantity	Value	Quantity	Value.
	Cwt.	£.	Cwt.	£.
Cotton, raw.	4,557,914	11,746,184	4,541,539	13,241,734
Rice	19,548,731	5,742,540	26,769,344	8,971,661
Seeds........	9,582,865	5,319,124	10,229,109	6,345,209
Tea..........	27,784,124	2,607,425	46,413,510	3,054,240
Wheat.......	5,583,336	1,956,333	7,444,375	3,277,942

Of the quantity, 4,541,539 cwts. of cotton 3,818,557 cwts. were taken by Europe; that is, 2,019,612 cwts. by England, 633,891 cwts. by Italy, 605,954 cwts. by France, and 559,100 cwts. by Austria.

Of the total quantity, 26,769,344 cwts. of rice, about half, or 13,582,300 cwts., were conveyed to Europe; of which, England and Malta took all but 300,000 cwts.

Of 10,229,109 cwts. of seeds, 9,705,200 were brought to Europe, nearly 5,600,000 cwts. coming to England, and 3,256,180 cwts. going to France.

Of the 46½ million pounds of tea, only one million went to the Australian market, all the rest being brought to England. In 1879–80, the price of tea was ruinously low, and a collapse in that industry was imminent; but happily, the Australian market was partially opened, and gave just sufficient stimulus to prices to avert disaster.

In 1875, only 1¾ million cwts of wheat were shipped from India. In 1880–81, there were 7½ million cwts., of which

England took 4,800,000 cwts., 1⅓ million went to France, and 790,000 cwts. to the rest of Europe.

The proportion of the whole trade which came to and left India by the Suez Canal, during the last five years, has been as follows:

	Whole Trade.	*Via* Suez Canal.	Per Cent.
1876-77 ...	113,920,539	60,243,259	52.88
1877-78....	126,252,968	68,380,719	54.16
1878-79....	109,777,084	53,398 875	48.64
1879-80 ..	122,068,908	63,033,231	51.64
1880-81....	138,108,657	81,175,876	58.78

The imports from the United Kingdom into India constitute 82.08 per cent. of the whole, and 42.33 per cent. of the produce shipped from India. The import trade is thus practically monopolized by Great Britain, the reason being that the only articles which the people of India largely consume are English products, in which other nations have not at present the capacity of competing with her for the supply of foreign markets.

On the other hand, the reason of her taking a smaller proportion of the exports arises from some of the most valuable Indian produce being shipped to other countries—notably, opium to China and the Straits; indigo, hides, and seeds to France, Italy, and the United States.

From these figures, it will be readily gathered that the commercial interests of England have not suffered from the canal, but, on the contrary have been largely augmented, and that while India has benefited by the opening of additional markets in Europe, England has also gained largely, and still maintains, a virtual monopoly of the Indian market for the disposal of her own merchandise, the value of which, in 1880-81, amounted to over 41½ millions sterling.

Passing on to the general trade through the canal, a reference to the tabular statements will show that, first, as regards the total tonnage, the number of vessels which passed the canal in 1870 was 486, and the tonnage 435,911 tons. In 1879, the vessels numbered 1,477, and measured 3,236,942; that is, while the vessels had increased threefold, the gross tonnage increased more than sevenfold. In 1870, the vessels averaged 900 tons each; whereas, in 1879, the average size reached 2,180 tons. In 1877, however, the greatest number of vessels, viz., 1,663, passed through the canal, but they only measured 3,418,950 tons, giving for the average of each vessel 2,056 tons. During the same period, the receipts of the company increased from £256,000 to £1,214,520. Great as that increase was, it is nothing compared to the increase within the last three years. In 1880, the vessels numbered 2,026, with a gross tonnage of 4,344,519 tons; and in 1881, the number rose to 2,727 vessels, measuring 5,414,050 tons.

Now, as to the nationalities of the vessels indicated in Table II. In the decade, ending 1879, out of 12,454 vessels, no fewer than 9,154, or three-quarters, were British; conveying 17,555,500 tons out of the total quantity, 23,105,535 tons.

In like manner, out of the 2,026 vessels in 1880, 1,592 were British, measuring 3,446,431 tons, out of a total of 4,344,520 tons; and, in 1881, the British numbered 2,251 out of a total of 2,727 vessels, and the tonnage, 4,792,118, out of 5,794,400, thus increasing the proportion of vessels and tonnage from two-thirds to four-fifths. The trade of 1881, then, of vessels other than British, amounted to only 1,002,282 tons, and would, at 10 francs per ton, have yielded a revenue of only £400,915.

In 1880, the total expenditure on the canal was about £218,900; interest and sinking funds of loan, £531,560; total, £750,460. So that, but for the British trade, instead of 13½ per cent. interest accruing to the original shareholders, there would not have been sufficient to pay even a moiety of the interest of the loans!

Similarity, with regard to the revenue realized. In 1879, it amounted to £1,214,520; 1880; £1,672,836; 1881, £2,187,047. The actual returns obtained from the canal will be seen from the following figures:

In 1880, the expenditure on repairs was..........................	£218,898
Amount set aside for interest and sinking funds....................	531,559
To provide for the interest at 5 per cent. on share capital and sinking fund....................	403,215
Total................	£1,153,972

Which deducted from the total receipts	£1,672,836
Leaves a balance of...	£519,164

to be allotted to reserve funds, and other purposes, as enjoined in the statutes above quoted, after deducting the 15 per cent. appropriated to the Egyptian Government. Of this sum, £350,176 was divided among the original shareholders, raising their profits to 5+4.3=9.3 per cent.

In 1881, the cost of working the canal was somewhat less than in 1880, viz., £212,490, while the interest and other charges remained about the same, but the total receipts were £2,187,047; so that the sum to be divided reached £700,-856, and the original shareholders pocketed 5+8.76=13.76 per cent. By arrangement, a certain number of shares are drawn to be paid off each year; those shareholders, therefore, who have got back their capital, cease to receive the annual 5 per cent. interest but are entitled to the dividend of 8.76 per cent. on it, and will continue to receive the enhanced dividend, whatever it may amount to, till the charter of the company expires in 1968. The dividend for 1882 is expected to reach 16 per cent.

Little comment is required on these figures, and there can be little doubt as to what nationality the future management of the canal should be entrusted, when four-fifths of the revenue is contributed by British trade. One cannot be surprised that there should arise a cry from British shipowners, that the time has arrived when the tax of ten francs per ton should be lowered. Abstractedly, it is a heavy burden, but relatively, it is less so, when it is remembered what economy in the expenses of transport has been effected by the saving in distance, and, therefore, in time, as well as in diminished risks and insurances. It is true that freights have fallen somewhat in proportion, and it may be said that, one year with another, the profits of the carriers are by no means so great as formerly; and it may be fairly urged, therefore, that the whole community is interested by the cost of transport, being kept at the lowest figure possible. A tax which represents so considerable a fraction of the whole freight, 16 per cent., I am informed, ought to be reduced, especially when it is no longer needed for the security of the capitalists from loss.

In approaching the consideration of the Suez Canal as a political problem, the difficulties which surround a satisfactory solution of it must at once be acknowledged. It has been seen, how, though originating in a commercial speculation, the scheme contained, from the very first, elements of a political character, which the British Government of the day could not possibly ignore; and how, as its operations progressed, sharply accentuated differences of international policy have been manifested, requiring the best efforts of diplomacy to arrange. Though, happily for England, no rupture in friendly relations has occurred, yet recent events have only too plainly shown "how great a matter a little fire kindleth," and how small a circumstance might vitally affect the interests of England, as regards her Eastern possessions, by the closure of this great thoroughfare to India. The immense importance to this country of those clauses in the concessions in which the Government declares the company to be Egyptian, subjects it to all the laws and usages of the Turkish Empire, and reserves to itself entire control in all particulars, military and civil, have been unmistakably manifest. Still more so, in the action of the Porte in insisting on the restoration of large tracts of land along the canal's course, which had been at first made over, without due consideration, to the company; for had that land still remained in their hands, it is extremely doubtful whether national susceptibilities might not have interfered with the able and successful strategic movement of our troops from Alexandria to Ismailia.

Again, the overwhelming preponderance of British trade over that of all other nationalities, added to the fact that its magnitude alone enables the Canal Company to exist, and the canal itself to be maintained, renders it imperative that that trade should not be imperilled, or its safety left dependent entirely on such strategic movements, the success of which a small error in judgment or a trifling miscalculation of time might wholly frustrate. It was pointed out, in a recent able article in the *Nineteenth Century*, that of all the naval powers England was furthest off from the Suez

Canal, and in the event of hostilities, her enemies might be first on the scene, and occupy it some days before her fleet could possibly arrive; but on the other hand, to be forewarned is to be forearmed and England possesses harbors nearer the canal than Portsmouth, and crusing ground nearer than the British Channel. If advantage be duly taken of these, and England is true to herself, the rest may be left safely to the judgment of Lord Alcester (late Sir Beauchamp Seymour), and such men as he has the good fortune to command.

But though such eventualities cannot be altogether ignored, yet there are, perhaps, other means left for procuring a more desirable solution of the desired end.

As far as can be judged from the sentiments expressed in the various organs of public opinion throughout Europe, there seems, with one exception, to be a tolerably unanimous desire that the management of the Suez Canal should be vested in British hands; just as there was at the time when Monsieur de Lesseps was anxious for the maritime powers jointly to purchase his scheme. The correspondence on that subject is published in Parliamentary paper Egypt, No. 2 (1876), and goes to show how nearly the canal was changing hands. As the practicability of that transaction was once seriously discussed, there is really nothing quixotic in a similar suggestion at the present time. To acquire the remaining shares, even at their enhanced value, would not be a financial operation beyond the power of England, which has, within the last few years, paid a much larger sum in wars than would probably be required to satisfy the demands of even the public-spirited and patriotic shareholders of the Suez Canal. England could well afford to take over the whole of the obligations and liabilities connected with it, as, with the prospect of an increase of traffic such as is now opening up, the surplus of the receipts over and above the necessary current expenses would quickly furnish the means of discharging those obligations and liabilities, while the mercantile community and public generally would not grudge a continuance of even the present high due of 8s. per ton, so long as there was the certain prospect before it of its diminution to the small figure requisite to defray the necessary expenses of the up-keep of the canal. For obvious reasons, details of calculations and figures cannot be entered into at the present time. If, however, international exigencies or individual susceptibilities should bar the way to such an amicable solution, and one profitable alike to all concerned, there remains a suggestion, which has elsewhere been made, of enabling the Ruler of Egypt to become the proprietor of a grand public work, which should never have been allowed to become the property of any private body of shareholders, but ought decidedly to have been undertaken solely and simply by the State. It is true that it is easy to be wise after the event, but it would equally be folly not to avail oneself of the wisdom when gained; and if, as was wittily remarked by the writer of the article in the *Nineteenth Century*, already adverted to, we are not only in the position of the *beati*, but the *beatissimi possidentes*, it will be odd if, with nine points of the law in our favor, we are not able to justify our proceedings. But even should this position be rendered untenable by a *force majeure*, there is yet a third resource, and that is, the construction of an alternative line of canal. This will, of course, be opposed on the ground that the several concessions conferred exclusive power on Monsieur de Lesseps for the construction of a canal across the Isthmus of Suez, east of the Damietta mouth. The exclusive power actually conferred, however, was that of forming a company for the purpose. If it excludes any other private individual from forming a company, it most assuredly did not exclude the Egyptian Government itself from making another canal anywhere it pleased within its dominions; and, therefore, the Egyptian Government is at perfect liberty to take whatever action it pleases in such a matter. But is it possible to make another maritime canal? Decidedly so; either east or west of the Damietta mouth. On the east, as far as can be judged from the line of coast surveys, a favorable approach and entrance could be formed in or about longitude 32.55, where the 5-fathom line runs nearest the coast, and from whence a pier, carried at an acute angle to the coast line on to the adjacent

shoal, would form a protection against the littoral currents, while the Bay of Pelusium would afford an enormous area for receiving so much of the Nile deposit as would be arrested by the pier. As, however, the pier would run not perpendicularly, but at an acute angle to the coast, its interceptive action would be much smaller than that caused by the Western pier at Port Said. The line of the canal could be taken through Lake Sirbon, and then through more or less cutting to the Bitter Lakes; from whence a course could be found more or less parallel to the present canal. The difficulty with regard to the supply of fresh water, which immediately suggests itself, could be overcome by pipes led from the Ismailia canal, under the maritime canal, into a reservoir, and pumped up from thence to every part of the new line. The probable cost of such a canal it is impossible to conjecture, in the absence of detailed surveys, but the difference between it and the existing line, if any, would be confined to the northern half. The economy which would be effected in the excavations by the experience already gained, would probably fully compensate for the increased depth of cutting, which might be unavoidable. A line to the westward could be found skirting the coast through the lagoon, and crossing the tidal rivers near their mouths as is now being done between Calcutta and Orissa.

A scheme for a fresh-water canal from Alexandria to Suez, *via* Cairo, attributed to the late Khedive, has lately been put forward, and advocated on the merits of its combining irrigating capabilities with those of navigation; but independent of its practicability, or otherwise either technically or financially, it is simply out of the question that England could hazard its present trade of 4¾ million tons, much less its future trade of possibly five times that amount, on a canal which would be exposed to such contingencies as are likely to occur to any fresh-water canal, whose navigability depends upon a series of locks, a class of works continually liable to derangement and injury.

An inter-oceanic canal ought, if possible, to be unencumbered throughout its entire length. Accidents, such as the wreck of a vessel, or the creation of shoals, may occur, of course, at any time; but a few days would, at the most, suffice to clear a passage through them; whereas, the subsidence of a lock could not be remedied under many months; and if the existing maritime canal be in unfriendly hands, as it assuredly will be were a rival line constructed, then what will become of England's trade?

There is no more zealous advocate of canals combining the two properties of irrigation and navigation than the writer of this paper; but works suitable for the requirements of inland navigation come under a wholly different category to those adapted for the oceanic traffic. The late enlightened and shrewd ruler of Egypt would not have failed to appreciate the difference had it been clearly explained to him, and would have quickly seen that the interests of Egypt would not be advanced by such a work; while one of the chief ends desired, viz., to supply irrigation to the Delta direct and by gravitation, can be attained at one-fifth of even the estimated outlay of a ship canal, and all the requirements of internal navigation be simultaneously secured. In properly devised schemes of delta works, such as those in India, several alternative navigable lines are led to the chief port, so that, in the event of one being blocked, others are still available; but deliberately to leave the traffic of distant countries dependent on a single line of canal, the masonry works of which a few charges of gunpowder in time of war, or an unforeseen accident in time of peace, might destroy, would, to say the least, be the gravest of mistakes—tantamount to an act of commercial, if not national, suicide.

In the event, however, of a satisfactory arrangement being come to as regards the existing canal, there remains the question how it can be best adapted to meet the requirements of the traffic, which has already outgrown its capabilities. Opinions seem to be divided as to the alternative of enlarging the present course, and that of constructing a parallel line. At first sight, it would seem as if there could be no doubt as to the former being the most economical, both in regard to first outlay and subsequent working expenses. If there is not much

of the excavated material in the banks to be removed, there can be no question as to the economy of widening the present canal over excavating a new one, while the cost of supervision for working the traffic would remain the same; whereas, if there is a double line, there must be an increased establishment, though not necessarily a doubling of its numbers. On the other hand, there would doubtless be an advantage in having a double line, both in minimising the chance of accidents, and in expediting the transit through the canal, by almost entirely obviating the necessity for stopping in sidings; though, as a low speed would still have to be maintained, ships would not be able to accomplish the voyage in twelve hours, and would therefore be obliged to spend, at least, one night in the canal, as at present. An additional advantage of a double canal would be, that in case of either becoming temporarily blocked, the whole traffic would not be suspended; a point of very great importance when the number of vessels increases so rapidly, as it seems likely to do.

It is concluded the expense of separate entrances at either end are not contemplated, but the present arrangements would be common to both lines. These, of course, could only exist, if the control is in the hands of England, or otherwise arranged for beyond the possibilities of risk in the time of war.

On the whole, however, after weighing the matter from all points of view, my own recommendation would be to enlarge the existing canal to a width of 200 feet at bottom, the dimensions of the great canal from the River Sutlej, recently opened in North India, with such slopes to the sides as the nature of the soil admits of at different points of its course, and substantially revetted with stone where necessary. The speed of vessels might then be increased to a maximum of eight miles per hour, but limited to four miles when passing one another in opposite directions. As few steam vessels have a lower speed than eight miles, there would be no necessity, except in a few instances, of their overtaking each other; especially if an interval between the starting of each vessel at daylight is made compulsory. A restriction of traffic during the night may still be necessary, unless very perfect arrangements can be made for avoiding collisions; but it would be safest to confine the voyage to daylight hours, and to make it feasible to pass through the canal between dawn and dusk.

There is yet one other phase in the political aspect for consideration, and that is, what are the principal dangers to which, in time of war, the Suez Canal would be exposed? They are:

1. A blockade at either end.
2. The removal of buoys, beacons, and necessary adjuncts to the navigation.
3. Blocking up the canal with obstacles of any kind.
4. The destruction or scuttling of vessels.
5. Last, but not least, cutting off the supply of fresh water from the Ismailia Canal.

The remedy for the first lies, of course, in the hands of the nation that commands the seas; and, if it is only careful to keep that command, it can equally anticipate and prevent any of the other dangers besetting the course of the canal itself. Cutting off the fresh-water supply could only occur in the event of another such eruption in Egypt as has lately been extinguished; and it would, no doubt, be done again, and done more effectually than it was on the last occasion. It rests entirely with England whether such an eruption shall be again possible; and whether, with the forewarnings she has received, and the experience which India has afforded her of a somewhat similar, though far graver, character, she shall not accept the position now thrust upon her, and herself keep the key of the Water Avenue that leads to her Eastern Empire.

It would be presumption on the part of the writer of this paper to offer further detailed suggestions on a matter which is intrusted to one of the ablest of diplomatists at England's command; but he cannot conclude without expressing his hope that the rumors of possible arrangements, as recently put forth in the daily journals, may prove to be inaccurate, and that it will soon become patent to the world that Egypt's extremity having made England's opportunity, the Suez Canal will no longer be allowed to

remain an apple of international discord, and that it will prove to be the particular link in a chain of providential circumstances leading to the land of the Pharaohs once more recovering prosperity, with the help of England's guidance, and maintaining it under the ægis of England's power.

DEFLECTIVE FORCES ON A REVOLVING SPHEROID.

By RD. RANDOLPH, C. E.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

IN the discussion of this subject objections and answers have been made having reference to the principal points of the problem, but which did not require a complete formula, embracing both Eastern and Western movements, and the correction of latitude required for the spheroid. The following is a brief statement of the whole problem, and the formula expresses the horizontal force of a body moving over the surface of a revolving spheroid acting at right angles to the line of the independent movement of the body. The form of the spheroid being adjusted to the force of its revolution.

P=the point on the surface where the force is required.

L=the angle between a line normal to the surface at P and the equatorial plane.

R=the length of the normal from P to its intersection with the axis of the spheroid.

R×cos L=the radius of the circle of revolution at P.

a=the angle between the line of the independent movement and the meridian tangent at P.

V=the velocity of revolution at P.

v=the independent velocity of the body on the surface. g=gravitation at P.

$v\times$sine a=the eastern or western component of the independent velocity.

V+v. sin a=the actual eastern velocity when a is east of the meridian plane.

V−v. sin a=the actual eastern velocity when a is west of the meridian plane.

$$\frac{V^2+2\,V.\,v.\sin a+v.^2\sin a^2}{R.\cos L.\,g}=\text{centrifugal}$$
force when a is east of the meridian.

$$\frac{V^2-2\,V.\,v\sin a+v.^2\sin a^2}{R.\cos L.\,g}=\text{centrifugal}$$
force when a is west of the meridian.

The horizontal component of these centrifugal forces is obtained by multiplying them by $\sin L=\frac{\cos L}{\cot L}$. Therefore

$$\frac{V^2+2\,V.\,v\sin a+v.^2\sin a^2}{R.\cot.\,L.\,g}=\text{horizontal}$$
component when a is east of the meridian.

$$\frac{V^2-2\,V.\,v\sin a+v.^2\sin a^2}{R.\cot L.\,g}=\text{horizontal}$$
component when a is west of the meridian.

The component of these which is at right angles to the line of independent movement is obtained by multiplying by sin a. As $\frac{V^2}{R.\cot L.\,g}$ is the centrifugal force of the surface at P, which is balanced by the curve of the spheroid of revolution, it will no longer be considered. Therefore

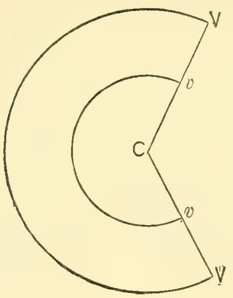
$$\frac{2\,V.\,v.\sin a^2+v.^2\sin a^3}{R.\cot L.\,g}=\text{component at}$$
right angles with line of easterly independent movement.

$$\frac{-2\,V.v.\sin a^2+v.^2\sin a^3}{R.\cot L.\,g}=\text{component}$$
at right angles with line of westerly independent movement.

The rotation of a horizontal plane which is a part of the surface of the spheroid is measured by the angle of the sector, which is developed from the cone described by a meridian tangent at the point

about which the plane rotates. Consequently this is the measure of the deflection of any of the lines of that plane. When a body is moving along a deflecting line it is subject to a deflective force equal to the centrifugal force upon a circle whose tangent, accompanying the body, deflects at the same rate.

Let VVC be the sector described in one revolution by the meridian tangent VC=R. cot L, and let the arc *vv* be the distance which the independent velocity *v* would accomplish in the same time.



Then the tangent of the arc *vv* accompanying the body moving with the velocity *v* deflects at the same rate as the radius VC. Therefore the centrifugal force on *vv* is the same as the deflective force of a body moving with the velocity *v* along the radius VC. As these arcs are in proportion to the velocities with which they are described, then $V : v :: R.\ \cot L : Cv$, the radius of the arc $vv = \frac{R.\ \cot L.v}{V}$, and the centrifugal force is $v^2 \div \frac{R.\cot L.v.}{V.} \times g = \frac{V.\ v}{R.\ \cot L.\ g}$.

As the eastern or western component of the independent velocity is involved in the centrifugal force and the body can move along the meridian line only with its meridional component, *v* in the above equation must be substituted by this component $= v.\ \cos a$. The deflective force would then become

$$v.^2 \cos a^2 \div \frac{R.\ \cot L.\ v \cos a.g}{V} = \frac{V.\ v.\ \cos a^2}{R.\ \cot L.\ g}.$$

This must be multiplied by cos a for that component of it which is at right angles to the line of the independent movement, making it $\frac{V.\ v.\ \cos a^3}{R.\ \cot L\ g}$.

As the reaction against this deflective force is to the left of the independent movement in the southern hemisphere and to the right of it in the northern, it is towards the equator in either with an eastern movement, and towards the pole in either with a western movement. Therefore, if the independent movement is westerly, the component last found is a minus quantity when added to the centrifugal force which is always towards the equator.

The whole force acting at right angles with the line of independent movement is then

$$\frac{2\ V.\ v.\ \sin a^2 + v.^2 \sin a^3 + V.\ v.\ \cos a^3}{R.\ \cot L.\ g}$$

when a is east of the meridian, and

$$\frac{-2\ V.\ v.\ \sin a^2 + v.^2 \sin a^3 - V.\ v.\ \cos a^3}{R.\ \cot L.\ g}$$

when a is west of the meridian.

In the latter case the force in question will be affected with a minus sign when reduced to its numerical value, showing it to be towards the pole. When multiplied by the weight of the body at P, the result is in pounds.

A soft alloy which attaches itself so firmly to the surface of metals, glass, and porcelain that it can be employed to solder articles that will not bear a very high temperature, can, according to *Amateur Mechanics*, be made as follows: Copper dust obtained by precipitation from a solution of the sulphate by means of zinc is put in a cast iron or porcelain lined mortar and mixed with strong sulphuric acid, specific gravity 1.85. From 20 to 30 or 36 parts of the copper are taken, according to the hardness desired. To the cake formed of acid and copper there is added, under constant stirring, 70 parts of mercury. When well mixed, the amalgam is carefully rinsed with warm water to remove all the acid, and then set aside to cool. In ten or twelve hours it is hard enough to scratch tin. If it is to be used now, it must be heated so hot that when worked over and brayed in an iron mortar it becomes as soft as wax. In this ductile form it can be spread out on any surface, to which it adheres when it gets cold and hard.

ON THE EFFECT OF THE EARTH'S ROTATION ON BODIES MOVING ON ITS SURFACE—A CORRECTION.

By J. E. HENDRICKS.

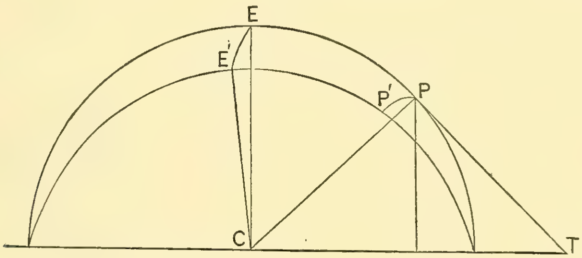
Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

IN the October number of this Magazine there was published an article by me "On the Effect of the Earth's Rotation on Bodies Moving on Its Surface," which contains several errors that I desire to correct.

In that article I obtained for the component of the deflecting force resulting from the earth's rotation about a normal axis at P, the equation,

$$f=\frac{\frac{1}{289}\,g\;24\;v\;\sin\lambda}{2\pi\mathrm{R}}, \qquad (2)$$

and I there stated that this equation was only correct on the supposition that the body did not change its longitude. I neglected, however, to determine the component of the force resulting from the rotation of the earth about its polar axis, which will be found to be exactly the same as that resulting from its rotation about the normal axis at P, as follows:



Let C represent the center of the earth, EE′ a part of the equator, EP=λ, the latitude of P, and let EE′=ωRdt be a very small arc of the equator.

If we suppose the meridian at P to be a material plane fixed in the sphere and revolving with it about the polar axis, and if the body passes P with a uniform velocity, v, it will be deflected at right angles to its line of motion by the uniform motion of the meridian plane about its polar axis, and will, therefore, in the very small time, dt, describe the circular arc, PP′; and if we suppose this motion to be continued on the tangent plane, while the tangent plane makes one complete revolution about PT as an axis, the locus of the body on the tangent plane will be a circle touching the normal axis at P, and the circumference of which will be v multiplied by the time occupied by the plane in making one complete revolution about PT, viz., one day divided by sin λ; whence we find, as a measure of the deflecting force at P, the centrifugal force in a circle which is the locus of the body on the tangent plane, or

$$f'=f=\frac{\frac{1}{289}\,g\;24\;v\sin\lambda}{2\pi\mathrm{R}}; \qquad (2')$$

therefore the total deflecting force at P, when the direction of the motion is in the meridian, is

$$f+f'=\frac{\frac{1}{289}\,g\;24\;v\;\sin\lambda}{\pi\mathrm{R}}. \qquad (a)$$

If the direction of the motion at P is not in the meridian but makes an angle β with the meridian, then the velocity of the body, in the direction of the tangent TP, is v cos β, instead of v, which being substituted for v in equation (a) gives

$$f+f'=\frac{\frac{1}{289}\,g\;24\;v\;\cos\beta\;\sin\lambda}{\pi\mathrm{R}} \qquad (a')$$

When β is 90°, cos β=0, and $f+f'$ becomes zero; but in this case we have from the body's motion in longitude a centrifugal force

$$F=\frac{\omega\,(R\cos\lambda-v)^2}{R\cos\lambda}=\omega\,R\cos\lambda-2\omega v+\frac{v^2}{R\cos\lambda},$$

and resolving this in the direction of the tangent plane it becomes

$$\omega\,R\cos\lambda\sin\lambda-2\,\omega\,v\sin\lambda+\frac{v^2\sin\lambda}{R\cos\lambda};$$

but of this force, the first term is just what is required to sustain the spheroidal figure of the earth, so that we have left for the efficient deflecting force, when the motion is to the west,

$$F'=-2\,\omega\,v\sin\lambda+\frac{v^2\sin^2\lambda}{R\cos\lambda},$$

and when the motion is to the east,

$$F'=2\,\omega\,v\sin\lambda+\frac{v.^2\sin^2\lambda}{R\cos\lambda}.$$

Or substituting for ω its equivalent, $V\div R$, and writing $\frac{1}{289}\,g$ for $V^2\div R$, we get

$$F'=\left(\pm 2+\frac{24\,v}{2\pi R\cos\lambda}\right)\times\frac{\frac{1}{289}g24v\sin\lambda}{2\pi R}.(b)$$

If the direction of the motion at P is not perpendicular to the meridian, but makes an angle β with the meridian, the motion of the body in longitude at the point P will be $v\sin\beta$, instead of v, which being substituted for v in equation (b) gives

$$F'=\left(\pm 2+\frac{24\,v\sin\beta}{2\pi R\cos\lambda}\right)\times\frac{\frac{1}{289}g24v\sin\beta\sin\lambda}{2\pi R}$$

$$=\pm\frac{\frac{1}{289}g\,24v\sin\beta\sin\lambda}{\pi R}+\frac{24\,v\sin\beta}{2\pi R\cos\lambda}\times\frac{\frac{1}{289}\,g\,24\,v\sin\beta\sin\lambda}{2\pi R}.\ (b)$$

As the deflecting force represented by (a') is the same whether the body moves north or south at the point P, it may be written either plus or minus. Hence, adding the right hand member of (a') to the first term of the right hand member of (b'), we get, for the resultant of the two forces when the motion of the body at P makes an angle β with the meridian, supposing the deviation from the meridian to be westward,

$$\Sigma(f)=-\frac{\frac{1}{289}\,g\,24\,v\sin\lambda}{\pi R}+\frac{24\,v\sin\beta}{2\pi R\cos\lambda}\times\frac{\frac{1}{289}\,g\,24\,v\sin\beta\sin\lambda}{2\,\pi\,R}.$$

Now as the first term of the right hand member of this equation, which represents the sum of all the deflecting forces, whatever the direction of the motion may be, is *minus* when the motion is westward, and *plus* when it is eastward from the meridian, and the second term is always *plus*, it follows that the deflecting force is *least* when the motion is to the west, and greatest when the motion is to the east.

Note.—Since the foregoing was written I have received the December Magazine, in which I find a very fair, and somewhat elaborate, criticism, by Mr. Rd. Randolph, of my paper in the October number, of which the foregoing is believed to be a correction. To most of Mr. Randolph's criticism I fully assent, as it agrees with the foregoing correction; but he has failed to supply the principal omission in my paper published in the October number, viz., the force f in equation ($2'$), above, and, therefore, his result is still incomplete, and does not represent the entire deflecting force.

The only part of Mr. Randolph's criticism, which demands any reply by me, is his charge that my method of determining f is "unphilosophical," "and will prove a stumbling block."

This charge is not warranted from the conception embraced in the method employed by me, but is, to a certain extent, by a verbal inaccuracy in my statement of the case. Instead of saying, as I did, "and hence will be compelled to describe a circle, and will return to P, &c." I should have said, "and hence will, at the point P, describe an indefinitely small arc of a circle, in which, if the body should continue to move, it would return to P, &c."

The actual curve described by the body on a tangent plane, if deflected by the revolving meridian, would, of course, be the spiral of Archimedes, of which P is the pole; but we only have to deal with the curve at the point P, where we know the velocity of the describing body and the amount of its angular deflection, and, consequently, know all the elements of the circle in which it is moving when in the point P. Its deflection at the point P is, therefore, the measure of the centrifugal force due the circle in which the body is moving at the point P.

THE HOWE TRUSS BRACE, AND THE GENERAL EQUATION OF THE FOURTH DEGREE.

By JOHN D. CREHORE, Cleveland, Ohio.

Contributed to Van Nostrand's Engineering Magazine.

The inscription of a rectangle of given width within a given rectangle is not a new problem; but the following solution, II, accurate far beyond the working limits of the *Howe Truss* brace and block, I have nowhere seen.

In the accompanying figure let ABCD be the given rectangle, or panel of the truss between the top and bottom chords; FGMN the required rectangular surface of brace; and FGB = MCN = the required brace-block section. Let O be the common center of the two rectangles; draw the diagonal BOC, also the lines OE and OH in the centers of their respective rectangles.

Take EB = AE = a = given half-width.
EO = OL = b = given half-length.
OH = l = required half-length of brace.
FH = HG = c = given half-breadth of brace.

Angle BOE = α, given.
BOH = ε, required.
EOH = γ, required.

I. From the figure we have angle

$$\text{BFG} = \text{EOH} = \gamma = \alpha - \varepsilon, \qquad (1)$$

$$a = c \cos\gamma + l \sin\gamma \qquad (2)$$

$$b = l \cos\gamma + c \sin\gamma \qquad (3)$$

$$\therefore \quad l = \frac{a - c\cos\gamma}{\sin\gamma} = \frac{b - c\sin\gamma}{\cos\gamma} \qquad (4)$$

$$c\sin^2\gamma - c\cos^2\gamma - b\sin\gamma + a\cos\gamma = o. \qquad (5)$$

But $\sin^2\gamma = 1 - \cos^2\gamma$; therefore equation (5) after reducing becomes

$$\cos^4\gamma - \frac{a}{c}\cos^3\gamma + \left(\frac{a^2+b^2}{4c^2} - 1\right)\cos^2\gamma + \frac{a}{2c}\cos\gamma - \frac{b^2-c^2}{4c^2} = o. \qquad (6)$$

Take a = 5 feet.
b = 7.5 "
c = 0.5 "

then equation (6) gives

$$\cos^4\gamma - 10\cos^3\gamma + 80.25\cos^2\gamma + 5\cos\gamma - 56 = 0, \qquad (7)$$

Solving by *Horner's* method, we find

$$\cos\gamma = .84498498 = \text{FB} = 2c\cos\gamma.$$

$$\therefore \sin\gamma = .53479004 = \text{BG} = 2c\sin.\gamma$$

since FG = 2c = 1 foot = radius in FBG.

$$\therefore \gamma = 32^\circ\ 19'\ 46.''82.$$

But $\tan\alpha = \frac{5}{7.5}$. $\therefore \alpha = 33^\circ\ 41'\ 24.''25$

$$\varepsilon = \alpha - \gamma = 1^\circ\ 21'\ 37.''43.$$

$$\sin\alpha = .554700196.$$

$$\cos\alpha = .832050294.$$

From (4), $2l = 17.118896$ feet.
= required length of brace.

This is the ordinary solution, and is exact so far as we please to carry out the decimals.

II. If we put $\alpha - \varepsilon$ for γ in equation (5), and take the well-known trigonometrical values of $\sin(\alpha - \varepsilon)$ and $\cos(\alpha - \varepsilon)$, and also substitute $1 - \sin^2\varepsilon$ for $\cos^2\varepsilon$, remembering that $a\cos\alpha = b\sin\alpha$, we find, after reducing,

$$\sin^2\varepsilon + \frac{a\sin\alpha + b\cos\alpha - 4c\cos\alpha\sin\alpha\cos\varepsilon}{2(\cos^2\alpha - \sin^2\alpha)}\sin\varepsilon = \tfrac{1}{2}, \qquad (8)$$

Now, since ε is always a small angle in practice, and c is also small, no appreciable error will be committed if we put $\cos\varepsilon = 1$ in the small third term of the numerator, equation (8). Making this substitution, and making no other variation from strict rule, we have

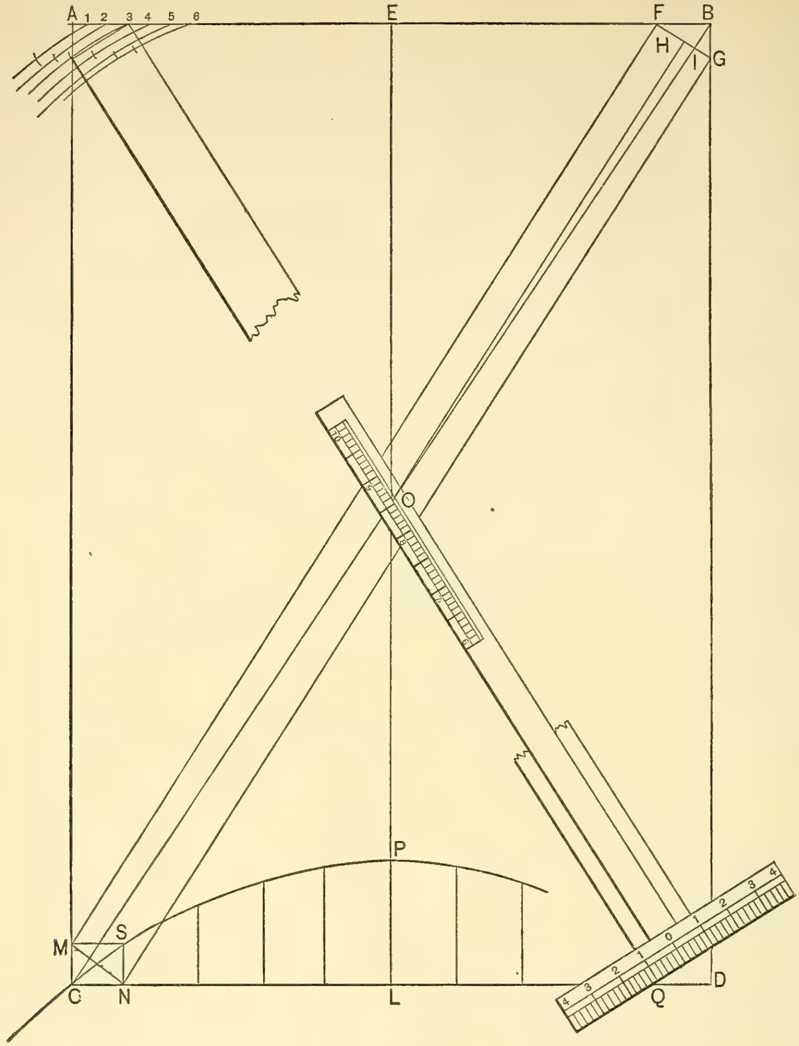
$$\sin^2\varepsilon + \frac{a\sin\alpha + b\cos\alpha - 4c\cos\alpha\sin\alpha}{2c(\cos^2\alpha - \sin^2\alpha)}\sin\varepsilon = \tfrac{1}{2}, \qquad (9)$$

which is of the second degree with respect to the unknown quantity, and gives, using the above values of a, b, c, and α,

$$\sin\varepsilon = .02374189, \quad \text{nearly.}$$

$$\therefore \varepsilon = 1^\circ\ 21'\ 37.''57, \quad \text{"}$$

$$\gamma = \alpha - \varepsilon = 32^\circ\ 19'\ 46.''68. \quad \text{"}$$



From (4), $2l=17.118910$ ft. "

$\sin\gamma=.5347894=BG$, "

$\cos\gamma=.8449854=FB$, "

since $FG=2c=1$ foot.

III. In the May number of this Magazine for 1881, Professor P. H. Philbrick has given, together with the exact solution by Horner's method, an approximate solution based upon the substitution of $\sin a$ for $\sin\gamma$, and $\cos a$ for $\cos\gamma$, in the second member of one of his fundamental equations, thereby avoiding expressions of a degree higher than the second. He also puts (continuing my notation) the length of brace,

$$2l=BC-2BI,$$

if BI in my figure were drawn at right angles to FG, as it is not.

His fundamental equations, using my figure and notation, and reducing, are

$$\sin^2\gamma+\cos^2\gamma=1 \qquad (10)$$

$$\frac{\cos\gamma}{\sin\gamma}=\frac{b-c\sin\gamma}{a-c\cos\gamma}, \qquad (11)$$

Putting a for γ in the second member of (11), and combining with (10), he gets

$$\cos\gamma=\frac{b-c\sin a}{\sqrt{(a-c\cos a)^2+(b-c\sin a)^2}} \qquad (12)$$

$$\sin\gamma=\frac{a-c\cos\alpha}{\sqrt{(a-c\cos\alpha)^2+(b-c\sin\alpha)^2}} \quad (13)$$

which give $\cos\gamma=.84430985=2c\cos\gamma=$ FB,

$\sin\gamma=.53585528=2c\sin\gamma=$BG,

with the above values of a, b, c, and α.

$\therefore \gamma=32°\ 24'\ 7.''01.$

$\varepsilon=\alpha-\gamma=1°\ 17'\ 17''.24.$

$$2l=2\sqrt{(a-c\cos\gamma)^2+(b-c\sin\gamma)^2} \quad (14)$$

$=17.118357$ ft.

Or,

$$2l=2\sqrt{a^2+b^2}-4c\sin\gamma\cos\gamma \quad (15)$$

$=17.122901$ ft.

Arranging results in tabular form for comparison, we have:

	FB.	BG.	γ.	MF.	Error assumed.
	ft.	ft.		ft.	
By equation (6)..	.84498498	.53479004	32° 19′ 46.″82	17.118896	o.
" " (9)..	.84498540	.53478940	32° 19′ 46.″68	17.118910	cos ε .0002820
Error resulting...	+.00000042	−.00000064	−... ....0.″14	+.000014	
By eq. (12)(13)(14)	.84430985	.53585528	32° 24′ 7.″01	17.118357	sin γ, +.0199102
Error resulting...	—.00067513	+.00106524	+.... 4′ 20.″19	—.000539	cos γ, —.0129347
By eq. (15)... ..	....	....	....	17.122901	
Error resulting...	....	....	....	+.004005	

From this example, which is an extreme case since the depth of brace is 1 foot and the panel comparatively small, it will be seen that equation (9) is almost absolutely exact, while equations (12), (13), (14), give results practically accurate.

The reader will smile at the precision implied in the remote decimals of this table; but he will also see that these decimals were necessary in order to show the error resulting from the use of equation (9).

IV. Again in this Magazine for December, 1878, Professor W. Allan has discussed this problem and given the graphical solution indicated at CPL in my figure.

He inscribes the two rectangles FGMN, G3MQ, having the common vertices G and M, and gets the fundamental equation

$$y^2-x^2-2by+2ax=0, \quad (16)$$

where $x=2c\cos\gamma=$FB,

$y=2c\sin\gamma=$BG,

of my notation, and eq. (16) is identical with equations (4), (5), (11).

Then, after pointing out certain relations of the areas of the rectangles, Professor Allan makes this strange remark:

"The face or hypothenuse of the block s equal to the breadth of the brace, and is the dimension given. The relation between this and one of the sides of the block leads, as Professor Woods remarks in his book, to an equation of the fourth degree, which is insoluble."

Now I suppose it is widely known that even the general equation of the fourth degree is solvable, and that *numerical* equations of any degree yield to such methods as Horner's. But it may be helpful to some to insert at the end of this article a solution of the general equation of the fourth degree, which I invented in 1865, before I knew that it was essentially Descartes' method invented long ago.

Professor Allan finally constructs the equilateral hyperbola CSP, from equation (16), the origin being at C, since, if

$y=o$, $x=o$ or $2a$,

$x=o$, $y=o$ or $2b$;

showing that one branch of the hyperbola passes through C and D, the other through A and B.

Then with C as a center and a radius $=2c=$the chord CS, find the point S, whose co-ordinates are manifestly x and y. This is a neat and sufficient solution, provided great care be taken in drawing the hyperbolic arc.

V. The *Mathematical Monthly* for March, 1859, edited by Professor J. D. Runkle, contains a practical solution of this problem, by Professor D. H. Mahan, which is shown in my figure at A.

With O as a center and a radius, or radii, about equal to half the unknown

length of the brace, describe the arcs 1, 2, 3, etc., near A. Then with these points 1, 2, 3, etc., as successive centers, and a constant radius$=2c=$width of brace, describe arcs intersecting the former arcs. Through these points of intersection draw a curve; the point where this curve meets the side AC, is one vertex of the inscribed rectangle required; the other vertex 3 is easily found.

VI. In conclusion, I would suggest an easy solution by means of the graduated T square shown in place in the lower right hand quarter of my figure. The drawing is a sufficient explanation of the construction and use of the instrument, when it is stated that, along the graduated edge of the scale at O, there is a very narrow slit exactly in the center of the long arm of the square, in which a very fine needle point piercing the paper at O, slides as the other scale is adjusted to the given width of brace at Q. Of course the needle may be attached to a sliding Vernier plate at O, and the end-scale may also have a Vernier reading.

VII. A method of reducing the general equation of the fourth degree to the general equation of the third degree, believed to be unpublished hitherto.

Let the given equation be

$$x^4+mx^3+dx^2+hx+n=o. \qquad (17)$$

As this equation contains two pairs of roots, real or imaginary, we may suppose its first member resolved into two factors of the form

$$(x^2+sx+t)\,(x^2+s_1x+t_1)=o \qquad (18)$$

$$\therefore \quad x^2+sx+t=o \qquad (19)$$

$$x^2+s_1x+t_1=o \qquad (20)$$

each of which contains one pair of the required roots, each pair being both real or both imaginary.

The assumed co-efficients s, s_1, t, t_1, are to be determined.

The product of (19) and (20) is

$$x^4+(s+s_1)\,x^3+\,(t+ss_1+t_1)\,x^2+\,(s_1t+st_1)\,x+tt_1=o \qquad (21)$$

which must be identical with the given equation (17). Therefore,

$$\left.\begin{aligned} m&=s+s_1\\ d&=t+ss_1+t_1\\ h&=s_1t+st_1\\ n&=tt_1\end{aligned}\right\} \qquad (22)$$

Eliminate s and s_1 from (22).

$$\therefore\ mh(t+t_1)-m^2n-h^2=d(t^2-2tt_1+t_1^2)-(t^3-t^2t_1-tt_1^2+t_1^3) \qquad (23)$$

Form (22) $\quad 4dn=4dtt_1 \qquad (24)$

and $\quad -4(t+t_1)n=-4t^2t_1-4tt_1^2 \qquad (25)$

The sum of (23), (24), and (25) is, after reducing,

$$(t+t_1)^3-d(t+t_1)^2+(mh-4n)\,(t+t_1)+4dn-m^2n-h^2=o \qquad (26)$$

which is of the third degree as required, and gives $t+t_1$, so that t, t_1, s, s_1, may be found from (22), and therefore x, from (19) and (20).

It will be noticed that this reduction differs from that of Descartes in these particulars:

1. The second term of (17) is not made to disappear. 2. Four undetermined quantities are assumed instead of three. 3. The resulting equation (26) is of the third degree involving two undetermined quantities, instead of involving only one to the sixth degree.

We may continue this solution by the use of the well-known Cardan's formula, which in the present case becomes

$$t+t_1=\frac{d}{3}+\left(\frac{v}{2}+\sqrt{\frac{v^2}{4}+r^3}\right)^{⅓}+\left(\frac{v}{2}-\sqrt{\frac{v^2}{4}+r^3}\right)^{⅓} \qquad (27)$$

if we put $-\dfrac{d^2}{9}+\frac{1}{3}(mh-4n) \qquad =r,$

and $\dfrac{2d^3}{27}-\dfrac{d}{3}(mh\text{–}4n)-4dn+m^2n+h^2=v.$

Comparing equations (7) and (17), we find

$$x=\cos\gamma$$
$$m=-10$$
$$d=80.25$$
$$h=5$$
$$n=-56$$
$$\therefore\ r=-657.5625$$
$$v=46029.09375$$

From (27), $t+t_1=80.012392809$

From (22), $4tt_1=-224$

$$\therefore\ t-t_1=-81.400141296$$
$$t=-.6938742435$$
$$t_1=80.7062670525$$

From (22), $s+s_1=-10$

$$ss_1=d--(t+t_1)$$

$$\therefore\ s-s_1=9.952365107$$

$$s=-.0238174465$$

$$s_1=-9.9761825535$$

From (19), $\cos^2\gamma-0.0238174465\ \cos\gamma=.6938742435$.

$$\therefore\ \cos\gamma=.844984987;$$

which is sufficiently near to the value found by Horner's method, to establish the correctness of both solutions.

Only such values of the undetermined quantities as conduct to the desired result, have been retained, and the "irreducible case" of Cardan's rule, happily not being required in this example, has not been discussed.

However gratifying it may be to work by a complete formula, it is casy to see that Horner's method of approximation for numerical equations requires far less labor than the general solution when many decimals must be used and you have only seven decimals in your logarithmic tables.

MANGANESE BRONZE.*

By Mr. P. M. PARSONS, M.I.C.E.

From "Iron."

Before entering on the immediate subject of this paper, I propose to give a brief description of what has previously been done in the same direction, and to review the theoretical considerations which have led to the production of manganese bronze. Many samples of bronze made by the ancients have been found on analysis to contain a small percentage of iron, but, as far as I am aware, no traces of manganese have ever been discovered. It is not unlikely the ancients knew that the addition of iron to bronze would increase its hardness, and introduced it with that view. In more recent times, the combination of iron with the brass alloys seems to have engaged the attention of inventors considerably, and a few have also introduced manganese by reducing the black oxide, and combining it with the copper, but none of these alloys appear to have shown sufficient advantages to lead to their permanent adoption. Among the earliest of these inventors was James Kier, who, as far back as the year 1779, proposed an alloy of 10 parts of iron with 100 of copper, and 75 of zinc. Alloys of a similar character to this, but containing less iron and different proportions of copper and zinc, were subsequently introduced under the name of sterro metal and Aitch metal, and Sir John Anderson, late superintendent of the Royal Gun Factories and inspector of machinery to the War Department, carried out a number of experiments with similar alloys, and with some very good results, but no practical applications of any of them appear to have been made. The addition of iron unquestionably increased the strength and hardness of these alloys, but the experiments I have made show that they acquire these qualities at the expense of ductility and toughness, and it is probably on this account that they have not come into general use. Besides these, various other inventors have proposed to combine iron with the brass alloys; but only Mr. Alexander Parkes and the late Mr. J. D. Morris Stirling, both eminent metallurgists, proposed the use of manganese, and appear to have carried their ideas into practice. Mr. Parkes' inventions consisted in combining manganese alone with copper, and using this alloy instead of ordinary copper with zinc, to form improved alloys of brass, yellow metal, &c., of which to make sheathing, rods, wire, nails, and tubes, &c. Mr. Everitt, of Birmingham, has also lately brought forward an alloy made in a similar manner.

No comparative experiments as to the strength, hardness, or ductility, or other qualities of these alloys, have come under my notice, but I believe the only ef-

* A paper read at the meeting of the British Association at Southport.

fect of the manganese alone, is to add somewhat to the toughness and ductility of the alloys, and allow copper and zinc, of a somewhat inferior quality, to be used in the manufacture of brass and other similar alloys, which without the manganese would not stand the working necessary to shape them into the various articles for which they were destined. Mr. Morris Stirling, in 1848, however, proposed to use manganese in various brass alloys, in which iron was present, but in a very different manner from that employed by me. Mr. Stirling first combined about 7 per cent. or less of iron, with the zinc, and added to the copper a small percentage of manganese, by reducing the black oxide of manganese with the copper, in the presence of carbonaceous materials, and then added to it the requisite quantity of the iron and zinc alloy to make the improved brass required. Mr. Stirling described a method of combining the iron with the zinc by fusion, but in practice he found a more ready way of procuring the zinc and iron alloy by employing the deposit found at the bottom of the tanks used for containing the melted zinc for galvanizing iron articles; this product consists of zinc with from 4 to 6 per cent of iron, but this percentage is very variable, and this material is useless if the amount of iron is required to be adjusted with accuracy. A variety of metal made by this process was in use for some time for carriage bearings, on the London and Northwestern Railway and others, with very good results; but it has long since been superseded, and I feel satisfied it was never introduced for any purposes where the requirements were great strength, hardness, ductility, &c., which may be partly accounted for by the defect which all these alloys possess in common, viz., the great difficulty of producing sound castings of them in sand moulds with any certainty.

These, then, were the chief inventions that have come under my notice at all approaching mine in character or similarity, at the time I introduced it, and which I will now proceed to describe:

The manganese bronze is prepared by introducing and mixing with the copper (to be afterwards made into alloys, similar to gun metal, brass, bronze, or any other alloy of which copper forms a base) a small proportion of ferro-manganese. The ferro-manganese is melted in a separate crucible, and is added to the copper when in a melted state, and at a sufficiently high temperature. The effect of this combination is similar to that produced by the addition of ferro-manganese to the decarburised iron in a Bessemer converter; the manganese in a metallic state, having a great affinity for oxygen, cleanses the copper of any oxides it may contain, by combining with them and rising to the surface, in the form of slag, which renders the metal dense and homogeneous. A portion of the manganese is utilized in this manner, and the remainder, with the iron, becomes permanently combined with the copper, and plays an important part in improving and modifying the quality of the bronze and brass alloys, afterwards prepared from the copper thus treated; the effect being greatly to increase their strength, hardness, and toughness, the degrees of all of which can be modified at will, according to the quantity of the ferro-manganese used, and the proportions of the iron and manganese it contains. By these variations, together with variation in the proportion of copper, tin, and zinc employed, a most valuable range of new alloys have been produced, possessing qualities in the way of strength, hardness, and toughness, &c., far beyond anything yet obtained in any similar alloys. It will be seen that the process described of making the manganese bronze is altogether different, both in principle and effect, from Stirling's or Parkes' inventions. By Stirling's method, combining the iron with the zinc, in order to introduce it into the alloys, altogether precludes its use in any but those alloys in which a considerable portion of zinc is employed, such as brass or yellow metal. It could not be applied to any of those important alloys, of the nature of gun metal, or bronze, in which copper and tin are the chief ingredients, and which form some of the most important qualities of the manganese bronze; but an equally important difference in the manufacture of manganese bronze consists in adding the manganese in its metallic state, in the form of ferro-manganese, to the copper, by which the copper is cleansed from oxides as before explained, which can

never be the case when the manganese is reduced from the black oxide and combined with the copper by one and the same operation, in the manner pursued by Parkes and Stirling.

Another point of great importance is the very great nicety with which both the iron and manganese can be adjusted, and the effect controlled by adding the ferro-manganese to the copper, as pursued in the manufacture of manganese bronze. The amount of manganese required for deoxidizing the copper, and for permanent combination with it, having been ascertained by experience, it is found that very slight variations in quantity have a perceptible and ascertained effect in modifying the qualities of the alloys produced; that is to say, the toughness can be increased, and the hardness diminished, or *vice versa*, at will, precisely as is done in the manufacture of steel, by increasing or diminishing the dose of carbon and manganese. In preparing the ferro-manganese for use, that which is rich in manganese, containing, say, from 50 to 60 %, is preferred; this is melted with a certain proportion of the best wrought-iron scrap, so as to bring down the manganese to the various proportions required. About four qualities are made, containing from about 10 to 40 per cent. of metallic manganese. The lower qualities are used for those copper alloys in which the zinc exceeds that of the tin, and the higher qualities in which tin is used alone, or exceeds that of the zinc used in combination; and the amount of ferro-manganese added varies generally from about 2 to 4 per cent. After a number of experiments and tests, the Manganese Bronze and Brass Company, who are the sole manufacturers of the manganese bronze, have adopted the manufacture of five different qualities of manganese bronze, although other varieties can be produced for special purposes. The distinctive features, peculiarities, and purposes for which these qualities are suited, are as follows:

No. 1. In this quality the zinc alloyed with the copper is considerably in excess of the tin.

It is cast into ingots in metal moulds, and then forged, rolled, or worked hot, and made into rods, plates, sheets, sheathing, and it may also be worked cold, and drawn into tubes, wire, &c. When simply cast it has a tensile strength of about 24 tons per square inch, with an elastic limit of from 14 to 15 tons. When rolled into rods or plates it has a tensile strength of from 28 to 32 tons, with a limit of 15 to 23 tons per square inch, and it stretches from 20 to 45 per cent. of its length before breaking. When cold rolled, the elastic limit rises to over 30 tons, and the breaking strength to about 40 tons, and it still elongates about 12 per cent. before breaking.

No. 2 is similar to No. 1, but still stronger, and it can, with the required care, be cast in sand when it is required to produce castings for special purposes, possessing the greatest strength, hardness, and toughness, but it must be melted in crucibles; passing it through the reverberatory furnace injures the metal, and causes unsound castings. It is not, therefore, adapted for general brassfounders' purposes, and those only who understand its peculiarities, and are experienced in its use, should attempt casting it in sand.

One of the most important applications of this quality is that of producing articles cast in metal moulds under pressure. Blocks of this metal thus simply cast have all the characteristics of forged steel, as regards strength, toughness, and hardness, without any of its defects. It is perfectly homogeneous, and, while not possessing a fibrous texture derived from rolling or hammering, it is still fibrous in character, and this in not one but in all directions alike, and when broken shows a beautiful silky fracture. Its tensile strength is from 32 to 35 tons per square inch, and its elastic limit from 16 to 22 tons, with an ultimate elongation of from 12 to 22 per cent. It can be cast on to any object, and will shrink on to it with a force equal to its elastic limit, and when released will show an amount of resilience of about double that of steel. Thus a hoop, shrunk on to a solid cylinder of iron, gave the following results: It stretched when hot ·03 of its diameter, in the process of contraction,

and when cold and released sprang back about ·003 of its diameter. As regards hardness, it is about equal to mild steel. To ascertain its efficiency in this respect, and to compare it with gun metal, wrought iron and steel, the following tests were made, by forcing a knife edged angular die into the flat surface of each of these metals, and the No. 2 manganese bronze cast under pressure. To make a dent of equal length in each of these, the following pressures were recorded:

Gun metal	12 cwt.
Wrought iron	15 „
Mild steel	20 „
Mild steel, oil hardened	25 „
Manganese bronze as cast	20 „
Manganese bronze as cast, hardened by pressure	22 to 23 „

All these results point to this material as a most suitable one for the construction of hydraulic and other cylinders, required to stand great strains, and particularly for ordnance. The Manganese Bronze and Brass Company are now making arrangements for casting a block in this metal to be made into a gun, and the results are being looked forward to with much interest, as, should this prove successful, the material is likely to become a formidable rival to steel and iron, for the construction of artillery, as, although the metal itself is more costly, the simple way in which it can be manipulated will make the total cost less, and the time required to construct a gun of it will probably be less than one-fourth of that required to build up iron or steel guns.

No. 3. This is an equally important alloy with the last, but possessing altogether different qualities, and suited to different and more varied applications.

It is composed principally of copper and tin, in about the proportions of gun metal, combined with a considerable dose of ferro-manganese. Its chief characteristics are very great transverse strength, toughness and hardness, the facility with which it can be cast, and the soundness and uniformity of the castings produced, without any special care having to be taken beyond what is ordinarily given in casting gun metal. It also possesses this very important advantage in the production of large castings, that it may be melted in an ordinary reverberatory furnace without injury to the metal; very careful analysis of this alloy before and after passing through the reverberatory furnace, showing that there is no appreciable alteration in its constituents. A bar of this metal cast in sand in the ordinary way, one inch square, placed on supports 12 inches apart, requires upwards of 4,200 lb. to break it, and before breaking, it will bend to a right angle, and it will sustain from 1,700 to 1,800 lb. before taking a permanent set. These results are in every respect fully up to those of the best rolled wrought iron, as some test bars of both exhibited will show; we have, therefore, in this a material which can be cast with facility into any intricate form, which it would not be possible to forge in iron, yet possessing all its strength, toughness, and hardness. This quality of manganese bronze is used for a variety of purposes, including spur, bevel, and all kinds of toothed wheels, gearing, worms, and worm wheels, framing brackets, and all kinds of supports and connections of machines, crank pin brasses, the shells of main and other bearings of marine and other engines, axle-boxes, and other parts of locomotive engines, and it has been found admirably adapted for statuary and art purposes generally, being much admired for its fine color, but the latter quality is quite a matter of taste, and the members of the association will be able to form their opinion thereon by examining the beautiful clock and ornaments, kindly lent by Messrs. Elkington & Co., made of the manganese bronze. The metal also seems to be peculiarily adapted for large bells. The advantages in this latter application are that bells cast from it possess the same or greater sonorousness, with a more mellow tone, and are at the same time so tough that they cannot by any means be cracked like bells made of ordinary bell metal, which is obliged to be made brittle in order to acquire the requisite sonorousness. The sound of a bell is also, to some extent, a matter of taste, and those who take an interest in this question may form an opinion as to the suitableness of the manganese bronze

for this purpose by sounding the one exhibited.

But the most important application from a commercial point of view is undoubtedly that of steamship propellers. Owing to the great strength of this metal, and its non-liability to corrosion, propellers of it can be made thinner than even those of steel; the surface is beautifully smooth, and when cast they are theoretically true to form, whereas in steel propellers allowance has to be made against the corrosion which takes place, and their deficiency in toughness, by increasing their thickness, and their form becomes distorted in the annealing oven they have to pass through after being cast. For these reasons the manganese bronze has a great advantage over steel. It has been proved conclusively by the logs of a number of steamships that have had their steel propellers replaced by manganese bronze blades that their speed has been increased, and the consumption of coal diminished, while the weight, vibration, and strain on the ship and machinery is considerably reduced. In addition to this, all these advantages are secured at a considerably less ultimate cost than by the use of steel, taking it upon the average life of a vessel; for although the first cost of a manganese bronze propeller, or a propeller with manganese bronze blades, is double that of steel, it is indestructible, whereas at the end of about every three years, the steel blades become so pitted and corroded that their renewal will be indispensable, which brings up the total cost of the steel blades on an average to two or three times that of manganese bronze. That the manganese bronze propellers are incorrodible, and in every other respect efficient, has now been proved by experience, as some have been at work approaching three years, and are as perfect in every respect as when first applied. Some time after introduction of the No. 3 quality for propellers, the No. 2 was used for some propeller blades, as fears were entertained as to the No. 3 setting up galvanic action and corroding the stern frames. Most of these propellers stood well, but some of the blades failed, and it was found on examination that the castings were unsound, owing to the metal having become deteriorated by melting in a reverberatory furnace. In consequence, it has now been determined to adhere solely to the No. 3, as this quality has always given the greatest satisfaction, both as to its facility in casting and efficiency under trial; and further experience proves the supposed galvanic action to be only a myth, or if there should be a tendency to it, it is effectually prevented by lining the inside of the stern frame with zinc strips. A proof of its soundness and tenacity was shown in an accident which occurred to one of the blades of the Garth Castle, at its launch from the yard of Messrs. John Elder & Co., in 1880, when one of the blades came in contact with the jetty, and was bent round without even a crack to nearly a right angle, and was afterwards hammered back cold to its original form without detriment.

The other qualities, Nos. 4 and 5, of the manganese bronze, have no particular claims to strength, but are most effective for the purpose of bearings, slide valves, slide blocks, piston rings, &c., and in all situations where friction occurs, and much more durable than ordinary gun metal. Before concluding, I may add a few words on the art of brass founding generally, and I cannot help saying that, as at present practised, it is very far behind what might be expected in these days of progress. In the manufacture of iron and steel an amount of scientific knowledge has been brought to bear, which elevates these industries into scientific processes, but I can discover nothing of the kind in bronze and brass founding—everything is there done by the rule of thumb, and that in a most clumsy manner. The idea of combining the various metals to form the alloys required in atomic proportions, does not seem to have been ever entertained, and even the books written for the practical guidance of brassfounders, ignore this important principle altogether. I must not be understood as applying this remark to Dr. Percy, or Mr. Mallet, and other scientific metallurgists, who have drawn attention to the subject, and made valuable suggestions respecting it in their well-known works, but I allude to that class of books generally termed handbooks, and the like, which contain instruction of the most clumsy and unscientific character, for making different alloys,thus for gun metal the proportions given are 1 lb. of copper to 2 ozs. of tin, or

if required to be harder $2\frac{1}{4}$ ozs., or $2\frac{1}{2}$ ozs., and so on; then, as regards brass, it may be 70 lb. of copper and 30 lb. of zinc, or 60 lb. of copper and 30 lb. of zinc, or 60 lb. of copper and 40 lb. of zinc, for yellow metals. Now, not one of these alloys or others described are in atomic proportions, and that is the reason why unsatisfactory results are constantly occurring in ordinary brass founding; not only are the copper alloys thus produced weak, soft, spongy and porous, but it is a constant occurrence that the constituents vary in different parts of the casting.

This is the case principally in the gun metal and bronze alloys. The surplus tin above that forming a definite alloy in atomic proportions, seems to be held in mechanical suspension, which separates by liquation, and collects at the top of the casting as it cools and solidifies, causing the well-known tin spots, sponginess, &c. The only remedy the ordinary brassfounder has for this, is to use as large a proportion of scrap metal as he can get—he does not know why, he only knows that he gets better castings by using it, but the true reason is that the scrap metal has adjusted its constituents in atomic proportions during the several re-meltings it has undergone, any surplus tin or zinc being got rid of by liquation and oxydation, but if in the original manufacture of the alloy the metals are combined in atomic proportions, nothing of this kind happens, the castings are sound and the alloys homogeneous. In the manufacture of manganese bronze, this principle is always kept in view, and all the different qualities produced have the metals they are composed of combined in atomic proportions.

Whether by this a really chemical combination is effected it is difficult to say, but this much I can vouch for, that the alloys thus produced are finer in texture, more homogeneous, stronger, and of a very much more stable character, than when not so combined; thus in the No. 3 quality the addition of $\frac{1}{4}$ per cent. of tin, instead of making it harder and stronger, as it ought to be, according to the ordinary accepted ideas, actually makes it softer, weaker, and the grain coarser, and the same thing occurs if the additional tin is increased $\frac{1}{2}$ or 1 per cent. until the tin arrives at another definite atomic proportion, when an alloy of a different character appears, but it then again becomes close grained, sound, homogeneous and stable. As a further proof of the soundness of this theory, the No. 3 quality may be passed through an ordinary reverberatory furnace, and although, in being thus treated, it is exposed for a considerable time to the action of an oxydizing flame, no appreciable diminution of the tin in its composition has been detected. Then, again, both the No. 1 and No. 2 may be remelted several times in the crucible, if it is done with care, without any alteration of their components. It is well known how difficult it is to melt brass and yellow metal, even in a crucible, when every precaution is taken, without some of the zinc escaping in fumes; this also, to a certain extent, occurs in melting the No. 1 and No. 2 manganese bronze, but the zinc apparently carries with it its atomic complement of copper, so that the proportions of what remains are not disturbed. I am led to this belief, not only by examining the metals after re-melting, but by the color of the condensed fumes, which, instead of being white as they are when produced from zinc alone, have a beautiful pink color, which I can only attribute to the presence of copper. Another and perhaps still more palpable proof of the value of combining the metals in their atomic proportions, is that, when this is done, the specific gravity of these alloys is perceptibly increased over those not so combined, even though in the latter case the heavier metal be in excess. I was much struck by this fact in taking the specific gravity of some No. 1 manganese bronze, which contains a large amount of zinc, and which, judging by its constituents, ought to be a comparatively light metal; but the trial proved that it was about equal to that of ordinary gun metal, composed of copper and tin, and very considerably above the mean weight of the metals composing it, indicating to my mind that these metals must have combined in such a manner as each to fit into and fill up the infinitesimal spaces between the molecules of the other, and if not actually forming what chemists would admit to be a perfect chemical combination, certainly more nearly approaching it than when the

metals are mixed together in the haphazard manner usually prevailing. I have no doubt that these combinations and the stable quality of the manganese bronze alloys is also due very materially to the action of the metallic manganese on the copper, in freeing it from the oxides it contains, and thus bringing the metals added to it into actual contact, and enabling them to combine in a more perfect manner than has been accomplished hitherto.

ACCOUNT OF SOME TESTS OF RIVETED JOINTS FOR BOILERWORK.

By CHARLES HENRY MOBERLY, M. Inst. C. E.

From Selected Papers of the Institution of Civil Engineers.

Having to design the joints for some steel boilers which the author's firm had to construct, in the spring of 1881, he decided to be guided by the following, as the most suitable data of which he had any knowledge:

1. Some double-riveted lap joints of $\frac{5}{16}$-inch Landore S plates, with Landore steel rivets, tested by Mr. Kirkaldy for the author's firm in 1879, giving results as under:

Breaking strength of the solid plate per square inch=25.85 tons.

Breaking strength of the plate in the joint per square inch of original area fractured=23.21 tons.

Shearing strength of rivets in the joints per square inch=19.4 tons.

2. Professor Kennedy's experiments on steel-riveted joints, made for the Research Committee of the Institution of Mechanical Engineers, from which the following is taken as a fair conclusion:

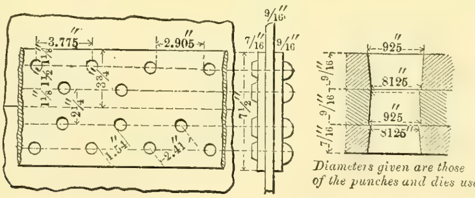
Breaking strength of the solid plate per square inch=29 tons.

Breaking strength of the plate in the joint per square inch of original area fractured=33 tons.

Shearing strength of rivets in the joints per square inch=22 tons.

The discrepancy between these two sets of conclusions is considerable, but not greater, perhaps, than is continually met with, as yet, in experiments on riveted steel-plate joints. The joint designed for the boilers in question was a double-riveted butt-joint with double covers, the inside cover being $\frac{7}{16}$-inch thick, and the outside one $\frac{9}{16}$ inch thick, to bear the caulking, as shown in Fig. 1, viz:

Fig. 1



Pitch of rivets, 3.775 inches.

Distance between rows of rivets, 1.5 inch.

From centre of rivets to edge of plate, 1.125 inch.

Lap, 3.75 inches.

Diameter of rivets, $\frac{3}{4}$-inch nominal.

" punch, $\frac{13}{16}$ inch=0.8125 in.

" " $\frac{13}{15}+\frac{1}{5}\times\frac{9}{16}=0.925$ inch.

Mean diameter of holes $=\frac{1}{2}$ (0.8125 +0.925) = 0.8687 inch, say 0.87 inch. Net length between the holes along the straight line=3.775−0.87=2.905 inches for each pitch; and along zigzag line=2 (2.41−0.87)=3.08 inches.

Area of one rivet $\frac{13}{16}$ inch in diameter =0.5185 square inch; hence rivet area

to resist shearing $=4 \times 0.5185 = 2.074$ square inch per pitch. Net area of the plate between the holes along the straight line$=\frac{9}{16}\times 2.905=1.634$ square inch, and area of solid plate per pitch$=\frac{9}{16}\times 3.775 = 2.123$ square inches.

The breaking strength of the joint came out as follows:

	By tests of $\frac{5}{16}$ inch plate joints.	By Professor Kennedy's data.
Solid plate..................	$2.123\times 25.85=54.88$ tons.	$2.123\times 29=61.57$ tons.
Through holes...............	$1.634\times 23.21=37.93$ "	$1.634\times 33=53.92$ "
Shearing rivets..............	$2.074\times 19.40=40.24$ "	$2.074\times 22=45.63$ "
Least ratio of strength.......	$\frac{37.93}{54.38}=0.69$ "	$\frac{45.63}{61.57}=0.74$ "

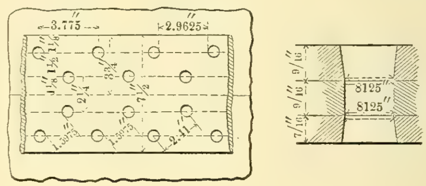
These results are all with punched and unannealed plates; and as it is generally admitted that the strength of joints is increased by drilling the holes and annealing the plates—and either or both operations could be resorted to if necessary—the author considered the joint, as designed, satisfactory for the purpose for which it was intended. But, having regard to the uncertainty still attaching to the strength of steel-riveted joints for boilers, it seemed desirable to have some tests made, and sample joints were prepared accordingly. The result of these tests was so different from what was expected, and so unsatisfactory otherwise, that further experiments were made in order to determine the proper proportion for these particular joints. All these tests, taken together, make a fairly complete series for the form of joint of $\frac{9}{16}$-inch steel plates in question.

In order to determine the rules which should regulate the proportions of all descriptions of riveted joints, a sufficient number of results of tests must be compared. Although a good many such results have, from time to time, been made public, many of them are incomplete, and more appear to be required before safe conclusions can be deduced for practice.

The author offers the present account of tests as a contribution towards such a collection. The plates tested were all of Landore S quality, $\frac{9}{16}$-inch thick, and the rivets were of Landore rivet steel. A few tests of single-riveted lap-joints for the circular seams of the same boilers were made together with the others, and will be included in this account. The whole of the tests were carried out by Mr. D. Kirkaldy, copies of whose reports are annexed (numbered 1 to 7).

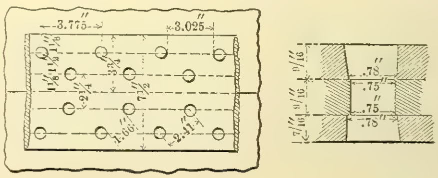
The first set of specimens consisted of nine joints (marked A, B, C, D, E, F, G, H and I), and six rivets (Annexed Reports, Nos. 1, 2 and 3). Joints A, B and C had all the holes punched as already described and shown in Fig. 1. Joints D, E and F had the holes in the covers punched, but those in the joint-plates were punched with a $\frac{5}{8}$-inch punch, and drilled out to $\frac{13}{16}$-inch in diameter, as shown in Fig. 2. Joints G, H and I also

Fig.2



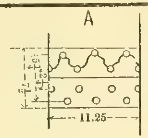
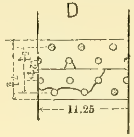
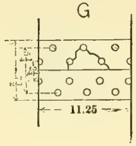
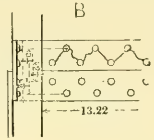
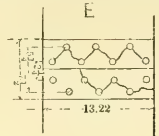
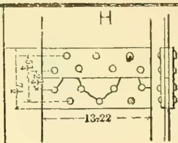
had the covers punched, whilst the holes in the joint-plates were punched with a $\frac{5}{8}$-inch punch, and drilled out to $\frac{3}{4}$-inch diameter, so that the larger side of the holes was barely cleaned out by the drill, as shown in Fig. 3.

Fig.3



It was intended to have all the joints shaped like B, E and H, Fig. 4; but Mr. Kirkaldy objected to this form, because he considered the results would not be reliable, and wished to have them all shaped like A, D and G, Fig. 5. It was

REPORT No. 1.—RESULTS OF EXPERIMENTS to ASCERTAIN THE ULTIMATE TENSILE STRENGTH OF SIX RIVETED JOINTS RECEIVED FROM MESSRS. EASTON AND ANDERSON. All Steel Plates. Nominal thickness, nine-sixteenths of an inch.

Sketch of Joint with Sizes	A 	D 	G 	B 	E 	H 
Test No. and thickness....	P. 2125 $\frac{9}{16}$ inch.	P. 2134 $\frac{9}{16}$ inch.	P. 2143 $\frac{9}{16}$ inch.	P. 2128 $\frac{9}{16}$ inch.	P. 2137 $\frac{9}{16}$ inch.	P. 2164 $\frac{9}{16}$ inch.
Description of joint........	Butt double riveted.	Butt double riveted.	Butt double riveted.	Butt double riveted.	Butt double riveted.	Butt double riveted.
Machine or hand-riveted...	Machine.	Machine	Machine.	Machine.	Machine.	Machine.
Rivet holes, per your description.	"Punched $\frac{13}{16}$."	"Punched $\frac{5}{8}$ and drilled $\frac{13}{16}$."	"Punched $\frac{5}{8}$ and drilled $\frac{3}{4}$."	"Punched $\frac{13}{16}$."	"Punched $\frac{5}{8}$ and drilled $\frac{13}{16}$."	"Punched $\frac{5}{8}$ and drilled $\frac{3}{4}$."
Plates, Brand.						
Plates, width and thickness.	11.25 × 0.540	11.25 × 0.545	11.25 × 0.540	13.22 × 0.545	13,22 × 0.545	1 322 × 0.545
Plates, sect. area, gross....	6.075	6.131	6.075	7.205	7.205	7.205
	lbs. tons.	lbs. tons.	lbs. tons.	lbs. tons.	lbs. tons.	lbs. tons.
Stress total................	263,340 = 117.5	263,690 = 117.7	256,180 = 114.3	288,620 = 128.8	323,230 = 144.3	319,910 = 142.8
Stress per square inch of gross area, joint.	43,348 = 19.3	43,009 = 19.1	42,169 = 18.8	40.058 = 17.9	45,861 = 20.0	44,401 = 19.8
Stress per square inch of plates, solid.	67,537 = 30.2	67,380 = 30.1	69,105 = 30.8	66,747 = 29.7	67,260 = 30.0	68,612 = 30.6
Ratio of joint to solid plate, percentage.	64.19	63.83	61.02	60.02	66.70	64.71
Where fractured..........	Plate at rivet holes, silky.	Plate at rivet holes, silky. One rivet sheared.	Plate at rivet holes, silky and granular, Two rivets sheared.	Plate at rivet holes. silky.	Plate at rivet holes, silky. One rivet sheared.	Plate at rivet holes, silky and granular. Two rivets sheared.
Rivets: dia., area and number.	Steel 0.81 = 0.515 × 6 × 2	Steel 0.81 = 0.515 × 6 × 2	Steel 0.75 = 0.441 × 6 × 2	Steel 0.81 = 0.515 × 7 × 2	Steel 0.81 = 0.515 × 7 × 2	Steel 0.75 = 0.441 × 7 × 2
Rivets: sectional area, total square inches.	6.180	6.180	5.292	7.210	7.210	6.176
Shearing stress per sq. inch of rivet area.	42,611 = 19.0	42,668 = 19.0	48,408 = 21.6	40,031 = 17.9	44,831 = 20.0	51,799 = 23.1
Tensile stress per sq. inch of rivet area.	64,362 = 28.7	64,362 = 28.7	64,362 = 28.7	74,362 = 28.7	64,362 = 28.7	64,362 = 28.7
Ratio of shearing to tensile, percentage.	Rivets not sheared.	One rivet sheared.	Two rivets sheared.	Rivets not sheared.	One rivet sheared.	Two rivets sheared.

Reshaped here to pitch of riveting as shown above. Tested as shaped by you as shown above. (Signed) DAVID KIRKALDY.

REPORT No. 2.—SUMMARY OF THE RESULTS OF EXPERIMENTS TO ASCERTAIN THE ELASTIC ALSO OF SIX STEEL RIVETS, RECEIVED

Plates—all cut Lengthway

Specimens out of Riveted Joint.	Test No.	Thickness.	Stress. Elastic per Square Inch.	Stress. Ultimate per Square Inch.	Ratio of Elastic to Ultimate.	Contraction of Area at fracture.	Stress per Square Inch of fractured Area.	Extension-set in 5 Inches. At 50,000 lbs. per Square Inch.	Extension-set in 5 Inches. At 60,000 lbs. per Square Inch.	Extension-set in 5 Inches. Ultimate.	Appearance of fracture.
	P	In.	Lbs. Tons.	Lbs. Tons.	%	%	Lbs.	%	%	%	
P 2, 125. A	2,126	.53	33,300	58,455	55.0	48.6	133,386	4.52	9.64	36.4	Silky
	2,127	.55	36,200	66,620	54.3	48.5	129,473	5.48	12.04	36.8	"
	Mean....		37,250=16.6	67,537=30.2	54.6	48.5	131,429	5.00	10.84	36.6	
P 2, 135. D	2,136	.56	35,800	67,440	53.0	42.1	116,561	5.08	11.38	35.6	"
	2,137	.54	35.800	67,320	53.1	49.6	133,648	5.22	11.46	36.8	"
	Mean....		35,800=16.0	67,380=30.1	53.0	45.8	125,104	5.15	11.42	36.2	
P 2, 143. G	2,144	.55	37,800	69,920	54.0	46 0	129,699	3.18	6.98	34.2	"
	2,145	.53	36,700	68,290	53.7	46.9	128.802	4.76	10.48	36.0	"
	Mean....		37,250=16.6	69,105=30.8	53.8	46.4	129.250	3.97	8.73	35.1	
P 2, 128. B	2,129	.54	35,800	66,835	53.5	47.3	131,957	4.96	11.38	35.2	"
	2,130	.55	35,300	66,660	52.9	49.1	130,189	5.34	12.38	35.8	"
	Mean....		35,575=15.8	66,747=29.7	53.2	49.2	131,073	5.15	11.88	35.5	
P 2, 137. E	2,138	.55	35,400	67,415	52.5	46.4	125,901	4.64	10.04	35.6	"
	2,139	.54	35,200	67,105	52.4	47.9	128,955	5.02	11 10	36.4	"
	Mean....		35,300=15.7	67,260=30.0	52.4	47.1	127,428	4 83	10.57	36.0	
P 2, 146. H	2,148	.55	37,800	69,990	54.0	41.8	120,279	3.48	7.06	33 6	"
	2,147	.54	36,500	67,235	54.2	52.7	142,378	5.10	11.42	37.2	"
	Mean....		37,150=16.5	68,612=30.6	54.1	47.2	131,328	4.29	9.24	35.4	
	Total Mean		36,337=16.2	67,773=30.3	53.5	47.3	129,268	4.73	10.38	35.8	

finally arranged to try both forms, and the joints were shaped accordingly, as shown in Figs. 4 and 5.

Finding that the plate broke in all these joints, the remaining three, C, F and I, were used to ascertain the shearing strength of the rivets by drilling out four rivets, and leaving three only on each side, as shown in Fig. 6 (Report No. 3).

The riveting in all these experimental joints was done by a hydraulic riveter, with an accumulator placed next to the machine, and the pressures given are those in the accumulator. In the nine joints now under consideration this pressure was 35 tons.

The plates were not annealed in any of the joints tested.

It will be most convenient to consider the results of the rivet shearing tests first (Reports Nos. 2 and 3).

The rivets happened to be rather bare in diameter, so that, the same rivets being used in all cases, those for the $1\frac{3}{16}$-inch holes had to be longer than would other-

AND ULTIMATE TENSILE STRENGTH AND QUALITY OF THE STEEL IN SIX RIVETED JOINTS; FROM MESSRS. EASTON AND ANDERSON.

Rivets.

Nominal Size of Rivets.	Test No.	Original Diar.	Original Area.	Stress. Elastic per Square Inch.	Stress. Ultimate per Square Inch.	Ratio of Elastic to Ultimate.	Fractured. Diar.	Fractured. Area.	Fractured. Difference. Area.	Fractured. Difference. Per cent.	Stress per Square Inch of fractured Area.	Extension.	Appearance of fracture.
	P	In. trnd	Sq. In.	Lbs. Tons.	Lbs. Tons.	%	In.	%			Lbs.		
$\frac{3}{4}$ in. dia.	2154	.504	.200	37,500	66,120	56.7	.32	.080	.120	60.0	165,300	Too short for ascertaining the Extension.	Silky.
"	2153	"	"	37,400	65,880	56.7	.33	.085	.115	57.5	155,011		"
"	2155	"	"	37,400	65,060	57.4	.31	.075	.125	62.5	173,493		"
"	2152	"	"	36,800	64,510	57.0	.31	.075	.125	62.5	172,026		"
"	2157	"	"	36,200	64,320	56.2	.31	.075	.125	62.5	171,520		"
"	2156	"	"	34,500	60,280	57.2	.30	.071	.129	64.5	169,802		"
		Mean...		36,633=16.3	64,362=28.7					61.6	167,859		

(Signed) DAVID KIRKALDY.

wise have been the case. Thus the rivets for the $1\frac{3}{16}$-inch holes were upset much more than those for the $\frac{3}{4}$-inch holes.

The tensile breaking strain (Report No. 2) was 28.7 tons per square inch, whilst the shearing strains were:

In C, with holes punched $1\frac{3}{16}$-in., 23.8 tons per sq. in.

In F, with holes punched $\frac{5}{8}$-in., and drilled out to $1\frac{3}{16}$-in. 24.2 " " "

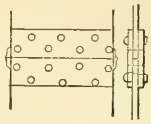
In I with holes punched $\frac{5}{8}$-in., and drilled out to $\frac{3}{4}$-in. 25.8 " " "

Mean shearing strain....=24.6 " " "

In these cases, therefore, the shearing strain was $\frac{24.6}{28.7} \times 100 = 85.7$ per cent. of the tensile strain. But it will be observed that the lowest tensile strength in report No. 2 is considerably below all the others; it may, therefore, be more fair to take the mean of the four middle tests only omitting the highest as well as the lowest. This gives the tensile strength=29 tons per square inch; and the shearing strength becomes $\frac{24.6}{29} \times 100$

REPORT No. 3.—Results of Experiments to Ascertain the Ultimate Shearing Strength of Three Riveted Joints received from Messrs. Easton and Anderson.

Steel Plates, nominal thickness, nine-sixteenths of an inch.



Test. No.	Description.	Rivets.		Shearing Stress.	
		Diameter Area and Number.	Sect. Area.	Total.	Per Square Inch.
P		Inch. Sq. In.	Sq. In.	Lbs. Tons.	Lbs. Tons.
2131	Punched holes, $\frac{13}{16}$ C	Steel .81=515×3×2	3.090	164,880=73.6	53,359=23.8
2140	Punched $\frac{5}{8}$, bored $\frac{13}{16}$ F	Steel .81=515×3×2	3.090	167,810=74.9	54,307=24.2
2149	Punched $\frac{5}{8}$, drilled $\frac{3}{4}$ (I)	Steel .75=441×3×2	2.646	152,790=68.2	57,774=25.77

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Fig.4

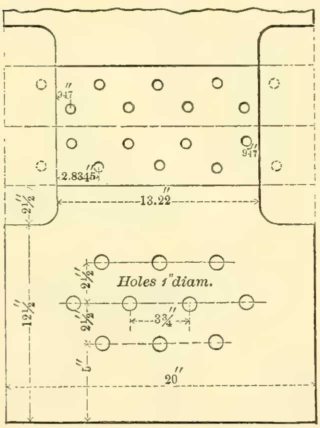
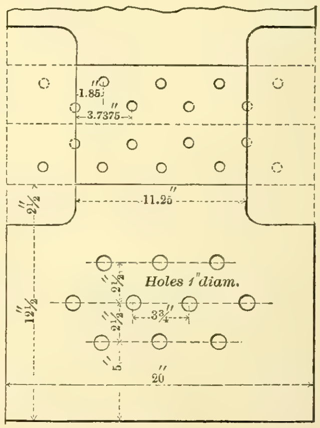


Fig.5



=84.82 per cent. of the tensile strength. Thus the shearing-strength of the rivets may be considered to be 85 per cent. of their tensile strength.

The breaking of joints A, B, D, E, G and H must be considered next. Here it may be explained that all the joints for these experiments were riveted up for the full width of the plates, as shown in dotted lines, and caulked on the edges as they would be in boiler work; they were afterwards shaped to the required form.

Report No. 1 gives the results of the tests, and shows the lines of fracture in each case, and, with the assistance of Figs. 1 to 5 and the particulars of the holes already given, the length of the line of fracture, the original sectional area along that line, and the original sectional area of rivets sheared is obtained.

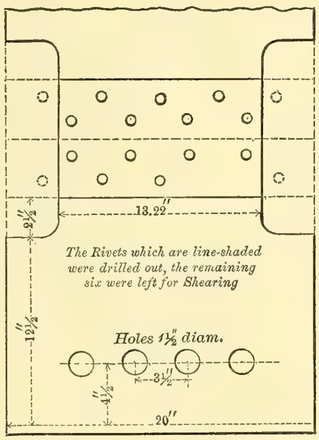
1. Joints A and B, with $\frac{13}{16}$-inch holes, punched in the ordinary way.

A broke through the rivet holes with

117.5 tons; length of fracture $= 9.24$ inches; area of fracture $= 9.24 \times 0.54 = 5$ square inches; strength of plate per square inch of fracture $= \frac{117.5}{5} = 23.5$ tons $= \frac{23.5}{30.2} = 0.778$ of solid plate; ratio of strength of joint to solid plate $= 64.19$ per cent.

B broke through the rivet-holes with 128.8 tons; length of fracture $= 10.27$ inches; area of fracture $= 10.27 \times 0.545 = 5.6$ square inches; strength of plate per square inch of fracture $= \frac{128.8}{5.6} = 23$ tons $= \frac{23}{29.7} = 0.774$ of solid plate; ratio of strength of joint given in report No. 1 is 60.02 per cent.

Fig. 6



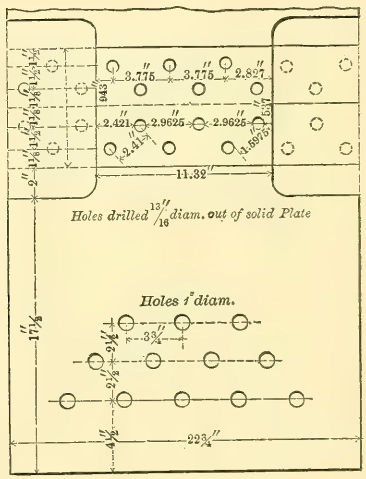
But this is not the correct ratio of the strength of the joint, when used in a boiler, because it may fairly be assumed that the fracture would have continued in a zigzag line in the continuous joint, instead of running along the straight line of rivets from the two outer rivets to the edge, as it did in this case. The specimen being 13.22 inches wide, the length of fracture along the zigzag line would have been 10.77 inches, instead of 10.27 inches, and its breaking strength would have been $\frac{10.77}{10.27} \times 128.8 = 135$ tons, making the strain per square inch of gross area of joint $= \frac{135}{7.2} = 18.75$ tons.

Hence the ratio of strength of the joint becomes $\frac{18.75}{29.7} \times 100 = 63.13$ per cent., which corresponds fairly with the result of joint A.

2. Joints D and E, with holes punched $\frac{5}{8}$ inch, and drilled out to $\frac{13}{16}$ inch in diameter.

D broke with 117.7 tons, by shearing one rivet and breaking along a line of fracture $= 6.72$ inches; area of fracture $= 6.72 \times 0.545 = 3.66$ square inches; sheared area of one rivet $= 2 \times 0.515 = 1.03$ square inch; shearing strain of ditto $= 1.03 \times 24.6 = 25.34$ tons; leaving $117.7 - 25.34 = 92.36$ tons to break 3.66 square inches of plate; hence, strength of plate per square inch of fracture $= \frac{92.36}{3.66} = 25.23$

Fig. 7



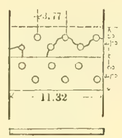
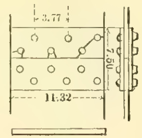
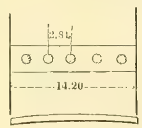
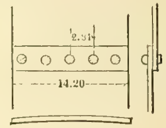
tons $= \frac{25.23}{30.1} = 0.838$ of solid plate; ratio of strength of joint to solid plate $= 63.83$ per cent., as per report No. 1.

But it must be observed that all the four half-rivets were more or less displaced sideways, though they were not entirely forced out of the joint. The holes were, of course, elongated. The half-hole on the right side measured $\frac{7}{8}$ inch, and the hole in which the rivet had been sheared measured $\frac{15}{16}$-inch full lengthways, after testing, as shown in Fig. 8.

E broke with 144.3 tons, by shearing one rivet and breaking along a line of

REPORT No. 4.—Results of Experiments to Ascertain the Ultimate Tensile Strength of Four Riveted Joints received from Messrs. Easton and Anderson.

All Steel Plates. Nominal thickness, nine-sixteenths of an inch.

Sketch of Joint with Sizes	K 	L 	M 	N 
Test No., and thickness.	P 2945 $\frac{9}{16}$ inch.	P 2948 $\frac{9}{16}$ inch	P 2951 $\frac{9}{16}$ inch	P 2954 $\frac{9}{16}$ inch.
Description of joint	Butt, double-riveted	Butt, double riveted.	Lap, single-riveted.	Lap, single-riveted.
Machine or hand-riveted	Machine.	Machine.	Machine.	Machine.
Rivet holes	Plate drilled, covers punched.	Plate drilled, covers punched.	Punched.	Punched.
Plates. Brand.				
Plates, width and thickness	11.32 × 0.56	11.32 × 0.56	14.20 × 0.555	14.20 × 0.555
Plates, sectional orea, gross	6.339	6.339	7.881	6.881
	lbs. tons.	lbs. tons.	lbs. tons.	lbs. tons.
Stress total	288,130 = 128.6	292,810 = 230.7	227,290 = 101.4	229,410 = 102.4
Stress for square inch of gross area, joint	45,453 = 20.3	46,192 = 20.6	28,920 = 12.9	29,109 = 13.0
Stress per square inch of plates, solid	67,940 = 30.3	67,280 = 30.0	65,002 = 29.0	64,900 = 29.0
Ratio of joint to solid plate, percentage	66.9	68.6	44.5	44.8
	67.75		44.65	
Where fractured	Plate at five rivet holes, and one rivet sheared.	Plate at four rivet holes, and two rivets sheared.	Five rivets sheared.	Five rivets sheared.
Rivets: diameter, area and number	Steel 0.81 = 0.515 6 × 2	Steel 0.81 × 0.515 × 6 × 2	Steel 1.04 = 0.849 × 5 × 1	Steel 1.04 = 0.849 × 5 × 1
Rivets: sectional area, total square inches	6.180	6.180	4.245	4.245
Shearing stress per square inch of rivet area	46,623 = 20.8	47,338 = 21.1	53,543 = 23.9	54,042 = 24.1
Tensile stress per square inch of rivet area Ratio of shearing to tensile, percentage	Rivets were not sent for ascertaining the tensile strength and quality.			

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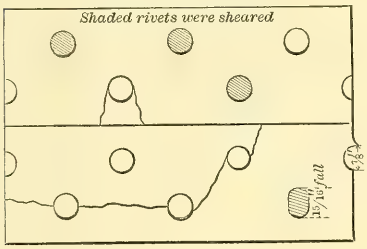
fracture=9.2 inches; area of fracture= $9.2 \times 0.545 = 5$ square inches; shearing strain of one rivet=25.34 tons; leaving $144.3 - 25.34 = 118.96$ tons to break 5 square inches of plate; hence, strength of plate per square inch of fracture = $\frac{118.96}{5} = 23.79$ tons $= \frac{23.79}{30} = 0.793$ of solid plate; ratio of strength of joint of solid plate=66.7 per cent. This joint is remarkable, because the second half was nearly broken through at the same time that the first half gave way altogether.

Fig. 8



3. Joints G and H, with holes punched $\frac{5}{8}$ inch, and drilled out to $\frac{3}{4}$ inch in diameter.

G broke with 114.3 tons, by shearing two rivets and breaking along a line of fracture=4.82 inches; area of fracture $= 4.82 \times 0.54 = 2.6$ square inches; sheared area of two rivets $= 4 \times 0.441 = 1.764$ square inch; shearing strain of ditto= $1.764 \times 24.6 = 43.4$ tons; leaving $114.3 - 43.4 = 70.9$ tons to break 2.8 square inches of plate; hence, strength of plate per square inch of fracture $= \frac{70.9}{2.6} = 27.27$ tons $= \frac{27.27}{30.8} = 0.885$ of solid plate; ratio of strength of joint to solid plate=61.02 per cent., as per report No. 1.

But the four half-rivets were again displaced sideway, and the holes elongated, as in joint D. The measurement of the elongated holes, after testing, was: half-hole on left $\frac{27}{32}$ inch (half-rivet nearly sheared); half-hole on right $\frac{7}{8}$ inch; hole in which the rivet was sheared on left $\frac{7}{8}$ inch bare, and on right 1 inch full, as shown in Fig. 9.

H broke with 142.8 tons, by shearing two rivets and breaking along a line of fracture=7.47 inches; area of fracture $= 7.47 \times 0.545 = 4.07$ square inches; shearing strain of two rivets=43.4 tons; leaving $142.8 - 43.4 = 99.4$ tons to break 4.07 inches of plate; hence, strength of plate per square inch of fracture $= \frac{99.4}{4.07} = 24.42$ tons $= \frac{24.42}{30.6} = 0.798$ of solid plate; ratio of strength of joint to solid plate=64.71 per cent.

Before drawing any conclusions it appeared desirable to try the effect of drilling the holes in the joint-plates out of the solid, the covers being punched as before.

Two joints, K and L, were therefore prepared in this way, the arrangement of rivets being the same as before. A further difference was also introduced in the form of the specimens, which were shaped as in Fig. 7 (Report No. 4). The holes were drilled $\frac{13}{16}$ inch in diameter. The pressure used in riveting was 35 tons, as before.

K broke with 128.6 tons, by shearing one rivet and breaking along a line of fracture=7.3 inches; area of fracture= $7.3 \times 0.56 = 4.09$ square inches; shearing strain of one rivet=25.34 tons, as in D; leaving $128.6 - 25.34 = 103.26$ tons to break 4.09 square inches of plate; hence, strength of plate per square inch of fracture $= \frac{103.26}{4.09} = 25.25$ tons $= \frac{25.25}{30.3} = 0.833$ of solid plate; ratio of strength of joint to solid plate=66.9 per cent.

L broke with 130.7 tons, by shearing two rivets and breaking along a line of fracture=5.55 inches; area of fracture= $5.55 \times 0.56 = 3.1$ square inches; shearing strain of two rivets=50.68 tons; leaving $130.7 - 50.68 = 80.02$ tons to break 3.1 square inches of plate; hence, strength of plate per square inch of fracture $= \frac{80.02}{3.1} = 25.81$ tons $= \frac{25.81}{30} = 0.86$ of solid plate; ratio of strength of joint to solid plate=68.6 per cent.

The results may now be considered with reference to—

I. The effect of the form of the specimen.

II. The effect of the mode of making the holes.

III. The general arrangement or proportions of the joint.

1. The effect of the form of the specimen may be judged by a comparison of A, D and G, Fig. 5, with B, E. H. Fig. 4, as shown in the following table:

Mark of joint	A	D	G	B	E	H
Breaking-strain of section per square inch in tons	23.5	25.23	27.27	23.00	23.79	24.42
Ratio of same to that of solid plate	0.778	0.838	0.885	0.774	0.793	0.798
Ratio of strength of joint to that of solid plate, per cent	64.19	63.83	61.02	63.13	66.70	64.71

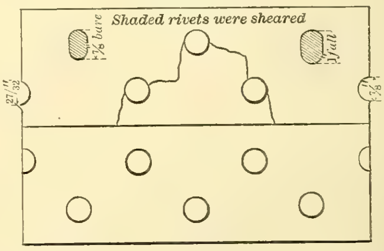
Comparing joints A and B, which not only were riveted, but also broke in the same way, namely, through the rivet-holes only, it will be seen that the strength per square inch of the plate, along the line of fracture, as well as the ratio of this strength to that of the solid plate, were practically identical in both cases. The ratio of the strength of the joint is slightly greater in A than in B, but the difference is not greater than may fairly be expected between similar test pieces. But as a correction has to be applied to the direct result obtained from B, it is fair to infer that the form of A gives a more reliable result than the form of B.

D and E, which were riveted in the same way, give more discordant results. As regards the strength of the section and the ratio of this to that of the solid plate, E compares very well with B and A. The ratio of strength of the joint is, however, different. In D the strength of the solid plate (30.1 tons per square inch) was practically the same as in E (30 tons per square inch), but the strength of the plate along the line of fracture appears to be 0.838 of that of the solid plate in D, compared with the ratios 0.778, 0.774, and 0.793 in A, B and E. The line of fracture in D passes behind the half-rivet on the left side, and to the inside of the one on the right side of the joint (Fig. 8). It is clear that the right-hand half-rivet must have been forced out of place, so as to allow the parts of the broken plate to separate, by part of the strain applied to break the joint; but as the half-rivet was not sheared, it is impossible to say how much strain it bore, though this must, of course, have been less than the strain that would have sheared it.

Whatever the amount of this strain, it is all credited to the section of the line of fracture, and hence the apparent increased strength of the plate in the joint and its ratio to the solid plate, given in the table. But, by whatever amount the strain sustained by the right-hand half-rivet fell short of its full shearing strain, by that same amount was the total proper breaking strain of the specimen joint reduced; hence the ratio of the strength of the joint also appears to be less than it really is. It seems, therefore, that the form of E gives as reliable results, in regard to the strength of the fractured plate and its ratio to the solid plate, as A and B did; but that the form D does not give reliable results.

Lastly, take G and H, which were again riveted in a similar manner. The plate in H was slightly stronger than the previous ones, but the ratio of its strength to that of the solid plate was nearly the same as in E. In short, H compares very well with A, B and E. In G the line of fracture passed inside both the half-rivets (Fig. 9), and the remarks made in reference to the right-hand half-rivet in D apply to both half-rivets in this joint; the more so as the left-hand one was nearly sheared. The effect would therefore be greater than in D. Accordingly, the strength of the plate in the joint end its ratio to the strength of the solid plate is still more increased than in D, whilst the ratio of strength of the joint is further diminished.

Fig. 9



The conclusions drawn are:

1. When none of the rivets are sheared,

and fracture takes place through all the rivet holes, the specimens, shaped through the centers of the rivets, give the most accurate result.

2. When rivets are sheared at the same time that fracture takes place, the specimens, shaped along a line between the rivets give, the most accurate results.

3. The correct way of estimating the strength of the joint is, to compute it from the strength of the plate along the line of fracture, as obtained from the tests.

4. The best way of getting a direct result would be to have two sets of joints prepared, one set being one pitch wider than the other set. The difference between the mean breaking-strain of these two sets of joints would at once give the breaking-strain for one pitch, which is what is wanted.

II. The effect of the mode of forming the holes.

The holes were formed in four different ways, as already stated.

1. By punching with a punch $\frac{13}{16}$ inch in diameter, as in A and B.

2. By punching with a $\frac{5}{8}$-inch punch and drilling out to $\frac{13}{16}$ inch in diameter, as in D and E.

3. By punching with a $\frac{5}{8}$-inch punch and drilling out to $\frac{3}{4}$ inch in diameter, as in G and H.

4. By drilling $\frac{13}{16}$ inch in diameter out of the solid, as in K and L.

As the results obtained from D and G are not reliable, for reasons already stated, they are not used in the present comparison.

The mean results from A and B, and K and L, and also those from E and H separately, are shown in the following table:

Marks of joints	A and B	E	H	K and L
Breaking-strain of section in tons per square inch	23.25	23.79	24.42	25.53
Ratio of same to that of solid plate	0.776	0.793	0.798	0.846

It is unfortunate that only one specimen of each of the classes 2 and 3, with punched holes, drilled out, can be used; the more so as it seems rather anomalous that H, with the holes drilled only to $\frac{3}{4}$-inch in diameter, so as barely to remove the punched surface, gives a slightly better result than E, in which the holes were drilled out to $\frac{13}{16}$ inch in diameter, and the punched surface was well cleared out. No comparison can be instituted between E and H, but they are so nearly alike that they may be classed together, and the mean ratio of 0.795 may be taken for the strength of the plate in the joint, as compared with the solid plate in both cases.

Thus the ratio of the strength per square inch of the plate along the line of fracture, compared with the strength of the solid plate, is:

1. For punched holes 0.776
2 & 3. " holes punched small and drilled out.................. 0.795
4. " holes drilled out of the solid. 0.846

It appears that little is gained over the ordinary punching, by punching the holes small and drilling them out larger; but by drilling the holes out of the solid the strength of the plate is increased 9 per cent., as compared with the punched holes.

III. The general arrangement or proportions of the joint.

For the purposes of this comparison, it is assumed that the fracture takes place along the zigzag line. The strength of the joints is then computed, from the data of the experiments, as follows:

Multiply the net length of the line of fracture, per pitch, by the breaking-strength per square inch of the metal in the joint, and by 100, and divide the product by the length of the pitch multiplied into the breaking-strength per square inch of the solid plate; the quotient is the strength of the joint in a percentage of the strength of the solid plate.

Omitting D and G as unreliable, the following results are thus obtained:

	Strength of joint. per cent.
Mean of A and B—holes punched $\frac{13}{16}$-inch	63.23
E, holes punched $\frac{5}{8}$-inch, and drilled out to $\frac{13}{16}$-inch	67.12
H, holes punched $\frac{5}{8}$-inch, and drilled out to $\frac{3}{4}$-inch	70.18
Mean of K and L—holes drilled $\frac{13}{16}$-inch	71.67

These results are not quite the same as those previously obtained, because they are computed for a supposed uniform line of fracture, as stated. It will be seen that the drilled holes in K and L give an excess of strength of more than 13 per cent. over the punched holes in A and B, but do not give much excess of strength over the punched and drilled holes in E and H. The strength of the weakest of these joints was sufficient for the purpose in this particular case; but even the strongest of them was not satisfactory, as it should certainly be 75 per cent. of the solid plate. It is clear that the rivets were stronger than the plate, although calculation shows that when the diameter was reduced to $\frac{3}{4}$ inch, as in G and H, the shearing strength of the rivets was nearly the same as the tensile strength of the plate in the joint.

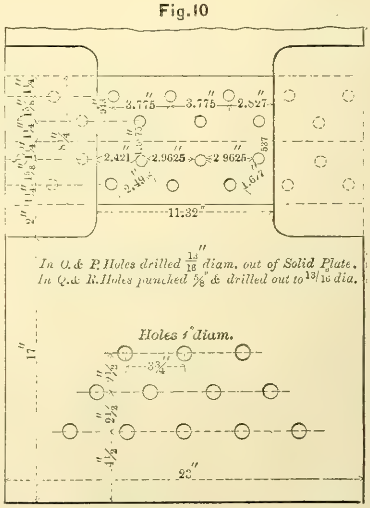
By increasing the pitch of the full-sized rivets, the joint would be strengthened, but $3\frac{3}{4}$ inches pitch is quite wide enough for $\frac{9}{16}$-inch plates to ensure tightness. If D and E be excepted, as the fracture in these two joints went past the rivet holes instead of through them, all the joints broke along the zigzag line, or from the rivet-holes to the edge of the plate. This was probably due to the rows of rivets being too close together, as well as too near the edge, and also to the pressure used in riveting having been too great.

To settle this point it was determined to have four more joints tested, two with $\frac{13}{16}$ inch drilled holes (O and P), to be 75 per cent. of the strength of the solid plate, and two (Q and R) of the same proportions, but with holes punched $\frac{5}{8}$ inch and drilled out to $\frac{13}{16}$ inch in diameter. All the joints were made from similar plates and rivets to those previously tried, but the accumulator-pressure was reduced to 25 tons. The pitch was 3.775 inches as before, and the breaking-strength of the solid plate 30 tons per square inch. Then for 75 per cent. strength of joint the breaking-strain per pitch should be $3.775 \times 0.75 \times \frac{9}{16} \times 30 = 47.78$ tons. For a shearing strength of rivets=24.5 tons per square inch, the area of each rivet$=\frac{47.78}{4 \times 24.5}=0.49$ square inch, so that $\frac{13}{16}$-inch diameter, giving a shearing area of 0.5185 square inch, will do for the rivets.

As not one fracture occurred from hole to hole along the straight line, the strength of the plate is assumed to be sufficient in that direction. Taking the strength along the zigzag line to be 0.85 of that of the solid plate, the net length between the holes should be $\frac{3.775 \times 0.75}{0.85}$ $=3.33$ inches, and this would be obtained by a distance between the rows of the centers of the rivets of 1.606 inch. This distance was made $1\frac{5}{8}$ inch, so that the net length of zigzag line of fracture was 3.354 inches per pitch. The distance from the center of the holes to the edge of the plate was increased from $1\frac{1}{8}$ inch to $1\frac{1}{4}$ inch.

This joint is shown in Fig. 10, and the result of the tests given in report No. 6, whilst the tensile tests of the rivets are stated in report No. 7.

Fig. 10



The calculated strength of joint with $\frac{13}{16}$-inch drilled holes is $\frac{3.354 \times 0.85 \times 100}{3.775}$ $=75.52$ per cent. of the solid plate.

REPORT No. 5.—SUMMARY OF THE RESULTS OF EXPERIMENTS TO ASCERTAIN THE ELASTIC AND ULTIMATE TENSILE STRENGTH AND QUALITY OF THE STEEL IN FOUR RIVETED JOINTS RECEIVED FROM MESSRS. EASTON AND ANDERSON.

Plates—all cut Lengthway.

Specimens out of Riveted Joints.	Test No.	Thickness.	Stress. Elastic per Square Inch.	Stress. Ultimate per Square Inch.	Ratio of Elastic to Ultimate.	Contraction of Area at Fracture.	Stress Per Square Inch of Fractured Area.	Extension-set in 10 inches. At 50,000 lbs. per Sq. Inch.	At 60,000 lbs. per Sq. Inch.	Ultimate.	Appearance of Fracture.
	P.	Inch.	Lbs. Tons.	Lbs. Tons.	%	%	Lbs.	%	%	%	
P 2,945. K	2,946	0.57	35,700	68,405	52.1	44.5	123,389	3 71	7.61	26.6	Silky
	2,947	0.55	35,500	67,475	52.6	45.7	124,326	3.55	8.40	26.8	"
	Mean.......		35,600=15.9	67,940=30.3	52 3	45.1	123,857	3.63	8.00	26.7	
P 2,948. L	2,949	0.57	35,600	68,140	52.2	41 0	115,595	3.52	7.39	25.5	"
	2,950	0.55	35,600	66,420	53.6	48.1	128,179	3.88	9.08	27.1	"
	Mean......		35,600=15.9	67,280=30.0	52.9	44.5	121,887	3.70	8.23	26.3	
P 2,951. M	2,953	0.55	31,800	65,445	48 5	49.1	128,783	4.48	9.80	29.4	"
	2,952	0.56	31,800	64,560	49.2	47.4	122,762	4.46	11.10	29.6	"
	Mean......		31,800=14.2	65,002=29.0	48.8	48.2	125,772	4.47	10.45	29.5	
P 2,954. N	2,956	0.55	32,600	65,110	50.0	34.3	99,198	4.03	9.20	26.5	"
	2,955	0.56	32,400	64,690	50.0	46.6	121,361	4.55	11.10	29.3	"
	Mean......		32,500=14.5	64,900=29.0	50.0	40.4	110,279	4.29	10.15	27.9	
	Total Mean.		33,875=15.1	66,280=29.6	51.0	44.5	120,448	4.02	9.21	27.6	

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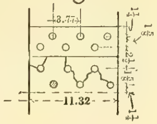
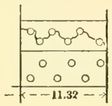
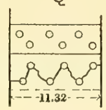
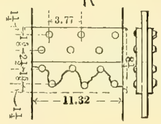
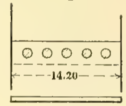
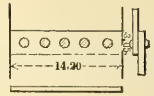
The tensile strength of the rivets in report No. 7—varying from 31.45 to 38 tons per square inch—shows such irregularity, and is so much in excess of the former tests (the rivets being taken from the same lot), that it cannot be accepted as trustworthy. The shearing strength of the rivets will therefore be taken as before, at 24.6 tons per square inch.

O broke with 133.6 tons, by shearing one rivet and breaking aloug a line of fracture=7.8 inches; area of fracture= $7.8 \times 0.555 = 4.33$ square inches. The strain that sheared one rivet was $24.6 \times 2 \times 0.515 = 25.34$ tons, leaving $133.6 - 25.34 = 108.26$ tons to break 4.33 square inches of plate; hence, strength of plate per square inch area of fracture $= \frac{108.26}{4.33} = 25$ tons $= \frac{25}{29.6} = 0.845$ of solid plate.

The strength of this joint is given at 72 per cent. in report No. 6; but computing it in the manner already explained, the strength becomes $\frac{3.354}{3.775} \times 84.5 = 75$ per cent. If this joint had broken along the zigzag line throughout, like the next one P, the line of fracture for the pitch in which the rivet was sheared would have been 3.354 inches long, requiring a strain of $3.354 \times 0.555 \times 25 = 46.5$ tons to break it; whereas that pitch is here credited with a breaking-strain for the plate of $1.6875 \times 0.555 \times 25 = 23.4$ tons, and for the rivet of 25.34 tons, or a total strain=48.74 tons.

It is, therefore, fair to conclude that the rivet offered less resistance to shearing than has been assumed, and, consequently, that the strength of the plate along the line of fracture, as also the computed strength of the joint, was greater than given. But, considering the uncertainty of this conclusion, and the fact that the strength of the fellow-joint

REPORT No. 6.—RESULTS OF EXPERIMENTS TO ASCERTAIN THE ULTIMATE TENSILE STRENGTH OF SIX RIVETED JOINTS RECEIVED FROM MESSRS. EASTON AND ANDERSON. All Steel Plates. Nominal thickness, nine-sixteenths of an inch.

Sketches of Joint with Sizes	O 	P 	Q 	R 	S 	T 
Test No. and thickness.....	P 4035 $\frac{9}{16}$ inch.	P 4038 $\frac{9}{16}$ inch.	P 4041 $\frac{9}{16}$ inch.	P 4044 $\frac{9}{16}$ inch.	P 4047 $\frac{9}{16}$ inch.	P 4050 $\frac{9}{16}$ inch.
Description of joint........	Butt, double-riveted.	Butt, double riveted,	Butt, double-riveted.	Butt, double-riveted,	Lap, single-riveted.	Lap, single-riveted.
Machine or hand-riveted....	Machine.	Machine.	Machine.	Machine.	Machine.	Machine.
Rivet holes...............	"Plates drilled, covers punched $\frac{13}{16}$."	"Plate drilled, covers punched $\frac{13}{16}$."	"Punched $\frac{5}{8}$ and bored $\frac{13}{16}$"	"Punched $\frac{5}{8}$ and bored $\frac{13}{16}$"	"Punched 1."	Punched 1."
Plates, width and thickness.	11.32 × 0 555	11.32 × 0.54	11.32 × 0.535	11.32 = 0.555	14.20 × 0.555	14.20 × 0.555
Plates, sect. area, gross....	6.282	6.112	6.056	6.282	7.881	7.881
	lbs. tons.	lbs tons.	lbs. tons.	lbs. tons.	lbs. tons.	lbs. tons.
Stress total................	299,310 = 133.6	287,480 = 128.3	277,880 = 124.0	313,240 = 139.8	221,190 = 98.7	221,840 = 99.0
Stress per square inch of gross area, joint.	47,646 = 21.2	47,035 = 21.0	45,885 = 20.5	49,863 = 22.5	28,320 = 12.6	28,404 = 12.7
Stress per square inch of plates solid.	66,197 = 29.6	63,257 = 28.2	63.562 = 28.3	69,587 = 31.1		
Ratio of joint to solid plate, percentage.	72.0	74.3	72.2	71.6		
	73.15		71.91			
Where fractured..........	Plate at 5 rivet holes and 1 rivet sheared, 100 per cent. silky.	Plate at 6 rivet holes, 100 per cent., silky.	Plate at 6 rivet holes, 100 per cent., silky.	Plate at 6 rivet holes, 100 per cent., silky.	Five rivets sheared.	Five rivets sheared.
Rivets: dia., area and number...................	Steel 0.81 = 0.515 × 6 × 2	Steel 0.81 = 0.515 × 6 × 2	Steel 0.81 = 0.515 × 6 × 2	Steel 0.81 = 0.515 × 6 × 2	Steel 1.04 = 0.849 × 5 × 1	Steel 1.04 = 0.849 × 5 × 1
Rivets: sectional area total square inches.	6.180	6.180	6.180	6.180	4.245	4.245
Shearing stress per sq. inch of rivet area.	48,432 = 21.6	46,517 = 20.7	44,964 = 20.1	50,686 = 22.6	52,106 = 23.2	52,259 = 23.2
Tensile stress per sq. inch of rivet area.	78,505 = 35.1	78,505 = 35.1	78,505 = 35.1	78,505 = 35.1	63,365 = 28.3	63,365 = 28.3
Ratio of shearing to tensile, percentage.	One rivet sheared.	Not sheared.	Not sheared.	Not sheared.	82.2	82.4
					82.3	

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P is so much greater than that of K and L (which also had $\frac{13}{16}$ inch drilled holes), it seems preferable to adhere to the computed strength, and to use it together with the results of joint P, for comparison with other joints.

P broke with 128.3 tons, by shearing through the rivet-holes along a line of fracture=9.46 inches; area of fracture $=9.46\times0.54=5.1$ square inches; strength of plate per square inch of fracture $=\frac{128.3}{5.1}=25.15$ tons $=\frac{25.15}{28.2}=0.89$ of solid plate.

The ratio of strength of joint given in report No. 6 is 74.3 per cent. But, computed from the strength of the plate in the joint as explained, it becomes $\frac{3.354}{3.775}\times 89=79$ per cent., or more than was expected.

Q broke with 124 tons through the rivet-holes, like P; length of fracture= 9.46 inches; area of fracture $= 9.46\times 0.535=5.06$ square inches; strength of plate per square inch of fracture $=\frac{124}{5.06}=24.5$ tons $=\frac{24.5}{28.3}=0.866$ of solid plate.

The ratio of strength of joint in report No. 6 is 72.2 per cent. But, computed from the strength of the plate in joint, the ratio becomes $\frac{3.354}{3.775}\times 86.6 =$ 76.9 per cent.

R broke with 139.8 tons through the rivet-holes, like Q and P; length of fracture=9.46 inches; area of fracture $=9.46\times0.555=5.25$ square inches; strength of plate per square inch of fracture $=\frac{139.8}{5.25}=26.63$ tons $=\frac{26.63}{31.1}=0.856$ of solid plate.

The ratio of strength of joint in report No. 6 is 71.6 per cent. But, computed from the strength of the plate in the joint, the ratio becomes $\frac{3.354}{3.775}\times 85.6$ =76 per cent.

The following table gives the results for comparison of the means of K and L with O and P; and E with Q and R:

Marks of joints	K and L	O and P	E	Q and R
Ratio of strength of metal in joint to solid plate	0.846	0.867	0.793	0.861
Computed ratio of strength of joint per cent	71.67	77.0	67.1	76.45

Thus the widening of the distance between the rows of rivets and between the rivets and the edge of the joints resulted in 7·57 per cent. increase of strength of the joint in the case of the drilled holes (K, L, and O, P), and 13.9 per cent. in the case of the punched holes (E and Q, R) drilled out; whilst the drilled holes in O and P show an advantage of only 0·85 per cent. over the punched and drilled holes in Q and R.

In every case the metal in the joint is weaker than in the solid plate; and this reduction of strength is least with the holes drilled out of the solid, and greatest with the punched holes. This result has very generally been obtained by most experimenters. Possibly the diminished pressure in the accumulator for riveting the four last joints, O, P, Q, and R, may have contributed to reduce the weaking of the metal in the joint; but as the experiments do not supply any data for considering this point it cannot be noticed here, except to suggest that, if the pressure is too great for the diameter of rivet, thickness of plate, and number of plates riveted, the metal round the holes may be injured and the strength of the joint weakened. This is a point that appears worthy of special investigation.

Whatever the true explanation may be of the diminution of strength of the metal in the joint, it is clear that the presence of the holes in the plate must somehow produce this result.

Supposing that the metal round the hole is not competent to bear the full strain due to its section, it may be assumed, without appreciable practical error, that a zone of metal round the whole does no work at all, and the rest of the section of the plate bears the full breaking-strain of the solid plate.

REPORT No. 7.—SUMMARY OF THE RESULTS OF THE EXPERIMENTS TO ASCERTAIN THE JOINTS AND EIGHT RIVETS RECEIVED

Plates—all cut Lengthway.

Specimens out of Riveted Joint.	Test No.	Thickness.	Stress. Elastic per Square Inch.	Stress. Ultimate per Square Inch.	Ratio of Elastic to Ultimate.	Contract of Area at Fracture.	Stress per Square Inch of fractured Area.	Extension-set in 10 Inches. At 50,000 lbs. per Square Inch.	Extension-set in 10 Inches. At 60,000 lbs. per Square Inch.	Extension-set in 10 Inches. Ultimate.	Appearance of Fracture.
	P.	In.	Lbs. Tons.	Lbs. Tons.	%	%	Lbs.	%	%	%	
P 4,035. O & P	4,036	.56	36,500	66,655	54.7	51.4	13 7231	3.49	7.79	28.5	Silky
	4,037	.55	35,600	65,740	54.1	53.5	141,514	4.11	9.39	29.8	"
	Mean....		36,050=16.1	66,197=29.6	54.4	52.4	139,372	3.80	8.58	29.8	
P 4,038. O & P	4,039	.54	33,700	63,310	53.2	52.0	131,998	5 35	12.8	28.6	"
	4,040	.54	33,700	63,205	53.3	53.0	134,637	5.54	12 5	27.8	"
	Mean....		33,700=15.0	63,257=28.2	53.2	52.5	133,317	5.44	12·8	28.2	
P 4,041. Q & R	4,042	.52	33,800	63,710	53.0	52.5	134,397	4.66	11.1	31.2	"
	4,043	.53	33,800	63,415	53.3	52.0	132,322	4.81	11.3	30.4	"
	Mean....		33,800=15.1	63,562=28.3	53.1	52.2	133,359	4.73	11.2	30.8	
P 4,044. Q & R	4,046	.55	39,200	69,720	56.2	52.9	148,054	2.08	5.61	27.5	"
	4,045	.56	39,500	69,455	56 8	52.1	147,369	1.59	5.19	25.9	"
	Mean....		39,350=17.6	69,587=31.1	56.5	52.5	147,711	1.83	5.40	26.7	
	Total Mean		25,725=15.9	65,651=29.3	54.3	52.4	138,439	3.95	9.49	28.7	

Let d = diameter of the hole.

d_1 = " " inoperative zone of metal.

p = distance from center to center of the holes diagonally.

f = ratio of the strength of metal in the joint to the solid plate.

Then $(p-d)\times f=p-d_1$, and $d_1=p-(p-d)\times f$.

inch.

For K and L $d_1=2.41-1.5975\times0.846$
$=1.060$ and $d_1-d=0.2475$

" O " P " $=2\ 49-1.6770\times0.867$
$=1.036$ and $d_1-d=0.2235$

inch.

For E and $d_1=2.41—1.5975\times0.793$
$=1.143$ and $d_1-d=0.3305$

" Q " R " $=2.49-1.6770\times0.861$
$=1.046$ and $d_1-d=0.2335$

" A " B " $=2.41-1.5400\times0.776$
$=1.215$ and $d_1-d=0.3450$

" H " $=2.41-1.6600\times0.798$
$=1.085$ and $d_1-d=0.3350$

If this view is correct, the net effective section thus obtained should, for the joints tested, be greater along the straight line than along the zigzag line, as none of them broke along the straight line. The net effective length of plate per pitch is thus:

Elastic and Ultimate Tensile Strength and Quality of the Steel in Four Riveted from Messrs. Easton and Anderson.

Steel Rivets.

Nominal Size of Rivets.	Test No.	Original Diar.	Original Area.	Stress. Elastic per Square Inch.	Stress. Ultimate per Square Inch.	Ratio of Elastic to Ultimate.	Fractured. Dia.	Fractured. Area.	Fractured. Difference. Area.	Fractured. Difference. Per cent.	Stress for Square Inch of fractured Area.	Extension.	Appearance of fracture.
—	P.	In. trnd	Sq. In.	Lbs. Tons.	Lbs. Tons.	%	In.	Sq. In.					
$\frac{3}{4}$ in. dia.	4,055	.504	.200	47,300	85,130	55.5	.36	.102	.098	49.0	166,921	To short for ascertaining the Extension.	Silky.
	4,053	.504	.200	47,100	84,810	55.5	35	.096	.104	52.0	176,687		"
"	4,054	.504	.200	44,400	73,620	60.3	.32	.080	.120	60.0	184,050		"
"	4,056	504	200	42,200	70,460	59.8	.30	.071	.129	64.5	198,478		"
	Mean	....		46,550=20.1	78,505=35.1	57.8				56.4	181.534		
1 in. dia.	4,057	.714	.400	34,700	65,740	52.7	.45	.159	.241	60.2	165,383		"
	4,059	.714	.400	34,600	64,110	53.9	.45	.159	.241	60.2	161,283		"
"	4,060	.714	400	34,200	62,330	43.8	.44	.153	.248	62.0	164,026		"
"	4,058	.714	.400	34,100	61,280	55.6	.44	.153	.248	62.0	161,263		"
	Mean	....		34,409=15.3	63,365=28.2	54.2				61.1	162,980		

(Signed) DAVID KIRKALDY.

inches.
For K and L, along the straight line 2.715
along zigzag 2.700

" O " P, along the straight line 2.739
along zigzag 2.900

" E along the straight line 2.632
along zigzag 2.534

" Q " R, along the straight line 2.729
along zigzag 2.888

" A " B, along the straight line 2.560
along zigzag 2.390

" H along the straight line 2.690
along zigzag 2.650

At first sight these results do not bear out the supposition of an inoperative zone of metal round the hole, as in two cases, namely O and P and Q and R, the zigzag length is greater than the straight one. But these joints were made with an increased distance between the rows of rivets; and it seems fair to infer that the metal along the zigzag line is weaker than that along the straight line. More experiments should be made to clear up these two points.

Meanwhile, as regards the proportions of the joints experimented on, with a pitch of 3·775 inches, the distance between the rows of rivets might probably be increased to $1\frac{3}{4}$ inch, with a slight addition of strength to the joint.

The ratios of strength are summarized in the following table:

Description of Joints.		Ratio of Strength of Plate in Joint to Solid Plate.	Ratio of Strength of Joint to Solid Plate.
			Per cent
$1\frac{1}{2}$ inch between rows of rivets, and $1\frac{1}{8}$ inch from center of rivet to edge.	A and B with holes punched $\frac{13}{16}$-in. in diameter.	0.776	63.23
	E with holes punched $\frac{5}{8}$-inch and drilled out to $\frac{13}{16}$-inch in diameter..................	0.793	77.12
	H with holes punched $\frac{5}{8}$-inch and drilled out to $\frac{3}{4}$-inch in diameter....................	0.798	70.18
	K and L with holes drilled $\frac{13}{16}$-in. in diameter..	0.846	71.67
$1\frac{5}{8}$ inch between rows of rivets, and $1\frac{1}{4}$ inch from center of rivet to edge.	O and P with holes drilled $\frac{13}{16}$-inch in diameter..	0.867	77.00
	Q and R with holes punched $\frac{5}{8}$-inch, and drilled out to $\frac{13}{16}$-inch in diameter.......	0.861	76.45

The obvious conclusion is that the punched holes make the weakest joint, and the drilled ones the strongest, whilst those with holes punched small and drilled out come between the two. Another conclusion is that widening the distance between the rows of rivets materially increased the strength of the joint. But there is yet a third conclusion of some importance, which appears to be justified by these results, namely, that increasing the distance between the rows of rivets reduces the difference in strength between the joints with drilled holes and those with holes punched small and drilled out.

Would a further increase of distance between the rows of holes practically abolish this difference in strength? And furthermore, would it increase the strength of the joint with punched holes so much as to make it little inferior to that with drilled holes? These are questions which require further experiments for their answer, and they appear to be well worthy of consideration. One point has still to be noticed in reference to these experiments, namely, the mode of attachment of the specimens to the machine. Figs. 4, 5, 6, 7, and 10, show how this was effected in each case. In joints B, E, H, A, D, G, ten 1-inch holes were used, namely, three in the first row, four in the second, and three in the third or back row. In all cases these holes were elongated by the test strain, the elongation being greatest in the first row, and rather less in each of the succeeding rows. It was also almost always greatest in the holes on or nearest the center line of the joint, diminishing to either side as the holes recede from the center line. The elongation of the front center hole varied from $\frac{1}{16}$ inch bare to $\frac{1}{16}$ inch full, whilst that of the holes in the back rows was only the difference between an easy and a tight fit of the callipers. Joints K, L, O, P, Q, and R, were each held by twelve 1-inch pins, two more being put in the back row than before, with the effect of diminishing the elongation of the holes, so that it was hardly perceptible in the back row. But again the elongation appeared to be greatest at the center of each row of holes. Joints C, F, and I, in which the rivets were sheared, were attached by four $1\frac{1}{2}$ inch pins. The elongation of the holes was slight, rather more in the two middle holes than in the side ones. Thus in the joints B, E, H, A, D, G, with ten 1-inch holding pins disposed in three row, where the greatest elongation occurred, the bearing pressure under breaking strain varied from 20 to 26 tons per square inch. In the joints K, L, O, P, Q, R, with twelve 1-inch holding pins, disposed in three rows, when the elongation was less, the bearing pressure was 18 to 21 tons per square inch. And in the joints C, F, I, with four $1\frac{1}{2}$ inch holding pins, all in one row, the bearing pressure was 20 tons per square inch, with hardly any elongation of the holes.

The last four tests of the series were with single-riveted lap joints, of the proportions of the circular seams in the boilers. The strength of the joint was evidently sufficient for the purpose, and it was not intended to test either this or

the strength of the plate in the joint. The object of the tests was twofold—first, to ascertain the shearing-strength of 1 inch rivets, and secondly to ascertain the effect on the same of the plates being curved transversely to the direction of the strain, which is the condition of these joints in boilers.

Two joints were made with the plates curved as in the boiler shell to a diameter of 10 feet 3 inches, and marked M and N, Fig 11; and two others, exactly the same in all other respects, were left flat, and marked S and T, Fig 12. The particulars of the tests of these joints are given in Reports Nos. 4 and 6, and those of the plates and rivets in Reports Nos. 5 and 7.

The diameter of the rivets was 1 inch nominal, the pitch 2.84 inches, and the lap 3 inches nominal. The holes were punched and the joints riveted with an accumulator pressure of 35 tons.

The results were as follows:

	Curved.		Flat.	
Mark of joints............	M	N	S	T
Total breaking strains in tons...........................	101.4	102.4	98.7	99.0
Shearing strain of rivets, tons per square inch...........	23.9	24.1	23.25	23.32
Mean tensile strength of rivets, tons per square inch.....	28.3	28.3	28.3	28.3
Ratio of shearing to tensile strains......................	0.843	0.852	0.822	0.824

In each case the plate was bent in the usual way, in the width of the lap, from the straight line, as shown in Figs. 11 and 12, namely, in M $\frac{3}{8}$ inch bare, in N $\frac{7}{16}$ inch bare, in S $\frac{3}{8}$ inch, and in T $\frac{7}{16}$ inch, or slightly more in the flat joints than in the curved ones. If the curved specimens had been wider no doubt this bending back would have been less, whilst in a complete circle it would have been very much less. The mean shearing strength of the rivets was: in the curved joints=24 tons per square inch= 0·848 of tensile strength; in the flat joints=23·28 tons per square inch= 0·823 of tensile strength. The mean result was the $\frac{3}{4}$-inch rivets in double shear was 24·6 tons per square inch= 0·857 of tensile strength.

Fig. 11

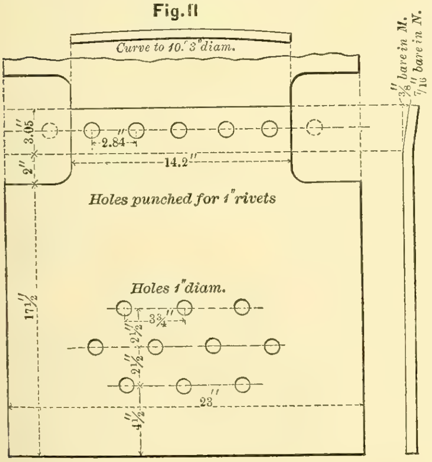
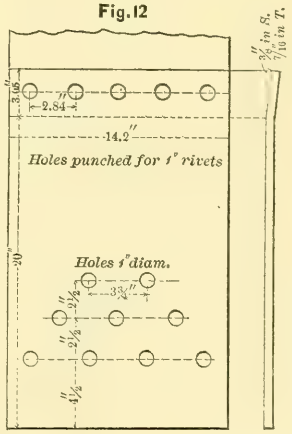


Fig. 12



The conclusions drawn are as follows:

1. The curved lap-joint is stronger

than the flat one, even in the comparatively narrow specimens tested. In a complete cylinder the excess of strength over a flat joint would probably be greater.

2. The 1-inch rivets are slightly weaker to resist shearing than the $\frac{3}{4}$-inch rivets; but probably this difference in strength would disappear in a complete circular joint, as the curved specimens give a greater strength than the flat ones.

3. Taking the tensile strength of rivets at 28 to 29 tons per square inch, it seems fair, for practical purposes, to assume the shearing strength to be 85 per cent. of the tensile strength for double-riveted, double-cover, butt-joints, and also for the complete circular lap-joints, and 82 per cent of the tensile strength for flat lap-joints.

In conclusion, the attachment of the four last joints to the machine has now to be noticed, as shown in Figs. 11 and 12.

Joints M and N were attached by ten 1-inch pins, and placed between curved blocks to retain the plates in shape. The elongation of the holes was hardly perceptible.

Joints S and T were attached by nine 1-inch pins, but with two pins only in the front row, and the elongation (though still very slight) was greater than in joints M and N, whilst the elongation of the holes in the front row was perceptibly greater than in the back row.

The bearing pressure of the pins on the plate at the breaking strain was $18\frac{1}{2}$ tons per square inch of surface for joints M and N with ten pins, and 20 tons per square inch for S and T with nine pins.

ELECTRICAL CONDUCTORS.

By Mr. W. H. PREECE, F.R.S., M.I.C.E.

From "Iron."

The first aerial conductors were made of copper, and the first gutta-percha-covered wires were of iron; but the positions were soon changed, copper being universally used for insulated conductors, and iron, until lately, for overhead lines. Sir William Thomson detected great variations in the quality of copper, and Matthiessen detected the causes, and established a standard of purity. Such improvements have been made in the quality, that copper wire was now twice as good as it was in 1856. Increased speed of working, improved efficiency of apparatus, and reduced waste of energy had followed the great increase in the purity of the copper. Temperature was a disturbing agent in the conductivity of the wire. Resistance increased more than 20 per cent. between winter and summer temperatures. Copper had recently been much used for aerial lines, it was less attacked by acids, and had great durability. Hard-drawn wire was now produced which had a breaking strain of 28 tons on the square inch, iron wire giving only 22 tons on the same area. Age did not seem to affect its quality, nor did it appear to be influenced by the currents of electricity employed for telegraphic purposes. The conductors of all cables remained constant. Lightning was supposed to render it brittle. The ultimate effect of the powerful currents employed for electric lighting was not yet known. The size of conductors was controlled by commercial considerations. Sir William Thomson had laid down the law that should control the size of leads for electric light, while that for cables followed strictly theoretical conditions. The best copper for electrical purposes came from Japan, Chili, Australia, and from Lake Superior; but much pure copper was obtained by electro-deposition, either directly from a solution,, or by using impure copper as the anode in a depositing bath. Electro-deposited copper had not the strength of ordinarily refined copper. The electrical resistance of commercial iron was from six to seven times that of copper, but its variation, due to the presence of impurities, was even greater. The weight of a

cylindrical wire one mile in length, and giving one ohm resistance at 60° Fahr., was called an ohm-mile. While the first iron wire was specified to give an ohm-mile of 5,500 lb., it was now obtained as low as 4,520 lb., and the maximum resistance was specified at 4,800 lb. The ordinary best puddled iron was at present used only for fencing purposes, but a mild English Bessemer steel was largely used for railway telegraphs and for stays; however, the resistance was very high, owing to the presence of manganese.

The wire used by the Post Office was made from Swedish charcoal iron, with an ohm-mile resistance of about 4,520 lb. Swedish Bessemer, or a specially prepared low-carbon English Bessemer, was adopted by the Indian Government, with an ohm-mile resistance of about 5,000 lb. Cast-steel wire, with a breaking weight of about 80 tons to the square inch, had been adopted on the Continent for telephone currents, with an ohm-mile resistance of 8,000 lb., while in England, where speed of working was the prime consideration, and length of span was negligible, electricians were satisfied with a breaking-strain of 22 tons on the square inch; in the colonies, where long spans were essential, and speed of working was not so important, the specification of 30 tons on the square inch. The electrical conductivity of iron wire increased with the percentage of pure iron, except where the percentage of manganese was high; an increase in the percentage of manganese augmented the electrical resistance considerably more than an increase in the percentage of sulphur or phosphorus. The durability of iron wire was maintained by galvanizing. When the galvanized wire was to be suspended in smoky districts it was additionally protected by a braided covering, well tarred. In some countries galvanizing was not resorted to, but dependence was placed on simple oiling with boiled linseed oil. Such a wire was erected in 1856 between London and Crewe, but the result was very unsatisfactory. More recently (1881) the experiment had been repeated with a similar result. In this climate galvanization was imperative. But it was not alone in smoky districts that iron wire decayed. It suffered much along the seashore. The salt spray decomposed the zinc oxide into soluble compounds, which were washed away and left the iron exposed, and this was speedily reduced to mere thin red lines. Where external decay was not evident time seemed to have no apparent effect on iron wire. Thirty-nine years of incessant service in conveying currents for telegraphy had not apparently altered the molecular structure of the iron wires in the open country on the London and South-Western Railway. Swedish charcoal iron was imported either in bloom or in rods, principally in rods. Each rod was rolled down to about 0.26 inch in diameter, and weighed on the average about 1 cwt. Iron wire could be rolled and drawn into coils 0.171 inch in diameter, weighing 400 lb. and measuring 1 mile; but 110 lb. was about the best practical limit for transport and use. The Swedish iron owed its value, not only to its comparative purity, but to the fact that it was smelted and puddled entirely with charcoal. The best qualities were a mixture of various ores, and they were known by various brands, the conditions determining those brands being secrets.

The operation of testing was a most important one, and requisite not only for the user, but also for the manufacturer. Flaws, impurities, faults, notwithstanding the greatest care, would occur, and they could be detected only by the most rigid examination and tests. Tests were mechanical and electrical. The mechanical tests embraced one for breaking strain, another for elongation, and a third for resistance to torsion. For hard steel wire, in place of the torsion test it was usual to specify that the wire should bear wrapping round its own diameter and unwrapping again without breaking. The electrical test was simply that for resistance—$\frac{1}{30}$ of a mile of the wire to be examined was wound round a dry wooden drum, and its electrical resistance was taken in ohms by means of a Wheatstone's bridge. Galvanization was tested by dipping in sulphate of copper, and by bending or rolling round a bar of varying diameter, according to the size of the wire. Special machines were constructed for the mechanical tests, the condition to be fulfilled being that for the breaking strain the increasing load or stress should be applied uniformly, without jerks or jumps, and the elongation machine should

correctly register the actual stretch without the wire slipping. The resistance to torsion of the wire was determined by an ink mark which formed a spiral on the wire during torsion, the number of spires indicating the number of twists taken before breaking. The perfection to which the manufacture of iron wire had been brought was very much due to the care bestowed upon the specifications by the authorities of the Post Office. The standard had been gradually raised, until it had attained a very high one. Many administrations objected to the expense of thorough inspection, with the result that they were the recipients of the rejected material of those who did rigidly inspect. One break in the wire cost far more than its inspection, and one extra ohm per mile affected the earning capacity of the wire in inverse proportion. It was, however, necessary to remark that the mechanical quality of charcoal-iron wire sometimes changed with time—its electrical quality remaining unaffected. Tests repeated at some subsequent period might therefore be deceptive unless allowance were made for the effect of time. Bessemer or homogeneous iron wire as a rule improved in its mechanical properties by being kept in stock. The Post Office authorities had decided to abandon a gauge altogether as applied to conductors, and to define size by diameter and weight. In future, all copper wires would be known by their diameters in "mils," or thousandths of an inch, and all iron wires by their weight in lb. per mile. Steel wire was used for long spans, or for places where great tensile strength was needed; but it was for the external strengthening of deep-sea cables that steel wire was principally adopted. It was first employed in the Atlantic cable of 1865 for this purpose. It had since been generally used for deep-sea cables. The usual diameter was 0.099 m., and it was specified to bear a breaking strain of 1,400 lb., which was equivalent to 81 tons on the square inch. Steel wire had been produced giving a much higher tensile strength. A compound wire of steel and copper was introduced in America about 1874, and it had been extensively tried in both hemispheres, but without success. Recently a compound wire had been erected between New York and Chicago, a distance of 1,000 miles, giving only 1.7 ohm resistance per mile. It had a steel core 0.125 inch in diameter, and was coated with copper electrolytically to a diameter of 0.25 inch. It weighed 700 lb. per mile. Hard-drawn copper, or silicious bronze of a much lighter character, would be equally efficient.

Phosphor-bronze, the hard mechanical qualities and great resisting powers of which were well known, was introduced for telegraph wire about five years ago. Several lengths were erected by the Post Office. Two long spans crossed the channel that separated the Mumbles Light house from the headland near Swansea. The object in view was to obtain great tensile strength with a power to resist oxidation, especially active where the wire was exposed to sea spray. This was done in 1879, and in November, 1883, not the slightest change was noticeable in the wire. But phosphor-bronze, though extensively used, had high electrical resistance; its conductivity was only 20 per cent. that of copper. Moreover, the phosphor-bronze supplied was irregular in dimensions and brittle in character. It would not bear bends or kinks. A new alloy, silicious bronze, had recently been introduced to remedy these disadvantages. Phosphor-bronze had disappeared for telegraph wire, and had been replaced by silicious bronze. The electric resistance of silicious bronze could be made nearly equal to that of copper, but its mechanical strength diminished as its conductivity increased. Wire, whose resistance equalled 90 per cent. of pure copper, gave a tensile strength of 28 tons on the square inch; but when its conductivity was 34 per cent of copper, its strength was 50 tons on the square inch. Its lightness, combined with its mechanical strength, its high conductivity and indestructibility, rendered it eminently adapted for telegraphs. If overhead wires were erected of such a material, upon sight supports, and with some method, there would be an end to the meaningless crusade now made in some quarters against aerial lines. These, if constructed judiciously, and under proper control, were far more efficient than underground lines. Corporations and local authorities should control the erection rather than force ad-

ministrations to needless expense and to reduced efficiency by putting them underground. Not only did light wires hold less snow and less wind, but they produced less electrical disturbance, they could be rendered noiseless, and they allowed existing supports to carry a much greater number of wires. German-silver was employed generally for rheostats, resistance-coils, and other parts of apparatus in which high resistance was required. It consisted of copper four parts, nickel two parts, and zinc one part. It possessed great permanence, and the variation in its resistance due to changes of temperature was small. The effect of age on German-silver was to make it brittle. Mr. Willoughby-Smith had found a smilar change with age even with wire drawn from an alloy of gold and silver. The form and character of electrical conductors must vary with the purposes for which they were intended. For submarine cables and for electric-light mains, where mechanical strength was not required, and where dimensions were of the utmost consequence, the conductors must be constructed of the purest copper producible, for copper was the best practical material at command. For aerial lines they must not only have great tensile strength, but in these days of high-speed apparatus they must have high conductivity, low electrostatic capacity, expose to wind and snow the least possible surface, and must be practically indestructible. Iron had hitherto occupied the field, but copper and alloys of copper seemed destined in many instances to supplant that metal, and to fulfill all the conditions required in a more efficient way, and at no greater cost per mile.

REPORTS OF ENGINEERING SOCIETIES.

American Society of Civil Engineers, November 21, 1883.

The Society met at 8 P.M., Director Geo. S. Greene, Jr., in the chair, John Bogart, Secretary.

A paper by E. H. Keating, M. Am. Soc. C. E., upon the Shubenacadie Canal, was read.

This canal is located between the City of Halifax, Nova Scotia, and the Basin of Mines, an arm of the Bay of Fundy. It was commenced in 1826, and the intention was to build it so as to accommodate vessels drawing 8 feet of water, with the idea that at comparatively small additional expense it could be used by vessels drawing 11 feet. It was to have 15 locks, 87 feet in length and 22 feet in width, with a lockage ascending from Halifax of 95 feet 10 inches, and descending to the Bay of Fundy of 95 feet 4 inches. The total length is about 54 miles, the greater portion of which was to be in the Shubenacadie River, and in a chain of lakes existing along the line of the canal.

Mr. Thomas Telford, the celebrated engineer, made a very favorable report upon the proposed canal and its prospects. Up to the close of 1831, £72,000 had been expended upon the work which was, however, in an entirely uncompleted state. Some of the locks near Halifax had not been commenced, and large and expensive work remained to be done upon the line of the canal. All the available capital being exhausted, the works were abandoned and fell into ruin, never having been completed on the original plan. In 1856, a report was made by Mr. W. H. Talcott, C. E., upon a scheme for completing the works upon a very much smaller scale than at first proposed, substituting for certain of the locks, an incline plane near Halifax, with a lift of 55 feet, and a similar plane with a lift of 33 feet at another point. The planes to be worked by hydraulic machinery. This report was adopted, and the work was completed in 1862, at a cost of $200,000. The diminished canal has, however, proved a failure as a commercial enterprise, and since 1870 no trade of any account has been carried on through it.

There was also presented a description, by Charles C. Smith, M. Am. Soc. C. E., of a hydraulic canal built at Minneapolis, Minn,, during the severely cold winter of 1881. This canal is under Main Street, Minneapolis, its entrance being at right angles to the street. It is covered with a semi-circular rubble stone arch of $17\frac{1}{2}$ feet span, and where the line turns the angle of 90° the abutments of the arch were built of curved lines of the radius of $31\frac{1}{4}$ and $48\frac{3}{4}$ feet. The arch was built of rubble masonry of stone varying from 4 to 6 inches thick, and from 18 to 36 inches long; the joints at the soffit being slightly hammered off to approximately form beds conforming to the radial lines of the arch. The mortar was made of one part Louisville cement to two parts of sand, and was mixed in hot water without salt. During its construction the weather was extremely cold, the frost having penetrated the ground to the depth of six feet.

An examination of this work having been made quite recently, it was found to be perfectly sound and free from any indication of settlement or rupture two years after its construction.

A discussion followed by the members present, more particularly in reference to the best methods of laying masonry in very cold weather, the experience of a number of members being favorable to the use of a strong solution of salt in the water with which the mortar was made.

The recent adoption of a system of time standards by the railways was discussed, and Prof. Julius E. Hilgard, M. Am. Soc. C. E., gave a statement as to the measures which were in progress in reference to securing a standard prime meridian, together with other measures

pertaining to the determination of standard time by the various nations of the world.

He also described the results of the recent meeting at Rome, Italy, of the Superintendents of the Geodetic Surveys of various nations.

Engineers' Club of Philadelphia—Record of Regular Meeting, November 17th, 1883.—Vice-President Wm. A. Ingham in the chair.

Mr. Edward I. H. Howell presented a sketch, based upon his personal observation while in England on a business visit, of the practice and peculiarities of the English machinists, with regard to machine tools.

He also exhibited specimens of polished shafting, from $1\frac{1}{8}''$ to $2\frac{3}{4}''$ in diameter, cold drawn, like wire, but omitted full description, as he expected the inventor to be present at a later meeting.

The Secretary read an illustrated paper by Mr. G. T. Gwilliam, upon the methods of making and placing the mattresses and fascines at the extension of the Delaware Breakwater Harbor.

The Secretary presented notes, by Mr. John J. Hoopes; to illustrate methods of computing tables by successive additions instead of separate calculations. Illustrations are given for the first, second, third and fourth powers, triangles and circles.

Mr. John Haug presented illustrated notes upon boiler construction, touching especially upon what should be shown in drawings and specifications for boilers.

Mr. George S. Strong exhibited specimens of cylindrical and corrugated flues. The former readily yielded to the pressure of the fingers, while the latter was trampled upon without injury.

The Secretary read, for Mr. C. J. Hexamer, a description of his experiments upon, with a discussion of the causes of, dust explosion in mills. Mr. William A. Ingham considered that some explosions in coal mines are probably attributable to the immense quantity of fine dust in the air, and Mr. T. Mellon Rogers, in response to Mr. Hexamer's comments upon the general absence of adjustable rolls in Philadelphia mills being a common cause of ignition, by the friction of foreign metallic particles in the stock, spoke of their general use in the West.

ENGINEERING NOTES.

The Panama Canal.—According to a statement by M. Dingler, Director-General of the Panama Canal work, the amount of earthwork will be 100 millions, instead of 80 millions as previously estimated. These, however, it is now said, will cost less than 80 millions would have done had the rock expected been met with. It is now affirmed that the very hard rock does not exist, and even the hard rock is much less thick than a survey had indicated. The soil being thus much less refractory than was originally supposed, the mode of cutting has been changed. The embankments of the canal, which in the rocky part would have been almost like vertical walls, will be flatter, involving the removal of more cubic meters This accounts for the total of 100 million meters instead of 80. But the first excavations, according to the engineers, would have cost 10f. a meter, whereas the new work will only cost on an average a-third of this sum. The 100 millions of cubic meters to be removed, with the accessory labor, expenses of administration—in short, with everything included, may be expected to cost in all 500,000,000f. With 100,000,000f. for meeting the unforeseen, a total of 600,000,000f. is the expenditure indicated by M. de Lesseps for the completion of the canal, provided, of course, there is no new miscalculation. It was believed at the outset that the canal might be ready in 1888, viz., five years. Last year this calculation became a certainty. The unexpected fact is reported that at some sections whither machines had been brought to perform the excavations, the negro workmen hired in great numbers merely as hodmen, asked to be allowed to do the excavating themselves, and have, in fact, succeeded in doing it cheaper. The machines were, therefore, conveyed to another yard.

The Arlberg Tunnel.—The piercing of the mountain was, we are glad to hear, successfully competed as far as the advance heading is concerned, on Tuesday, the 13th ult. The tunnel proved to be 3 meters shorter than had been calculated, and thus the meeting took place a day sooner than was intended. A similar miscalculation in the St. Gothard Tunnel was attributed to the attraction of the mountain. The official ceremony of leveling the rock is to take place on Monday in presence of the Austrian Minister of Commerce and other distinguished guests from Austria, Switzerland and Italy. Another great Alpine highway will then be preliminarily opened up, just two years after the first experimental trip conveyed about sixty passengers—contractors, engineers and their friends—through the tunnel of the St. Gothard. The new tunnel is 10,270 meters in length, while the Mont-Cenis Tunnel is 12,323, and the St. Gothard 14,900 meters. The first took fourteen years and a-half, and the second about eight to bore; the Arlberg Tunnel will have taken, when vaulted and ready to receive the first locomotive, about four years. Dynamite has been largely used, and the Brandt revolving rock drill has been employed, as well as the Ferroux percussion drill. For these drills several streams from the heights of the snow-covered Arlberg, were gathered on the eastern side into reservoirs from which turbines which compressed the air to five atmospheres, for the Ferroux borers, were worked; while on the western side pumped water was passed through pipes to the pressure of over a hundred atmospheres, to work the Brandt revolving borer, which cuts cylindrical blocks of rock from the mountain. The gallery has been driven on a level with the bottom of the future tunnel, and not on the Belgian system, as was formerly done, on a level with the top. Large money premiums were granted for completing the work before the stipulated time—in which premiums the contractors allowed their workmen to share. The two halves of the work

were allotted on December 21st, 1880, to two contractors—Ceconi for the eastern part, and the Brothers Lapp for the western side; but the piercing of the galleries, effected in the beginning by ordinary tools, as the nature of the stone did not allow the employment of boring machines, had already begun in June, 1880. On November 13th and November 17th respectively, the percussion and the rotating borers began their work, which advanced on each side at an average of from 5 to 7 meters daily, the greatest effort having been achieved in 1882, when 3590 meters were bored, while the St. Gothard Tunnel had a maximum of boring in 1878 of only 2530 meters. The whole cost, including the double-tracked railway through the tunnel, will not exceed eighteen million florins, or one and a-half million pounds, including the premium to the contractors for early completion; while the cost of the whole railway line from Innsbruck, in the Tyrol, to Bludenz, in the Austrian province of Vorarlberg, passing through the Arlberg Tunnel, will be forty million florins. The third Alpine Tunnel connects parts of the same country, and not foreign countries as in the case of its forerunners.

The New Eddystone Lighthouse.—At the meeting of the Institution of Civil Engineers, held on November 27, Mr. Brunlees, president, in the chair, a paper was read on "The New Eddystone Lighthouse," by Mr. William Tregarthen Douglas, Assoc. M. I. C. E. The necessity for the construction of a new lighthouse on the Eddystone rocks had arisen in consequence of the faulty state of the gneiss rock on which Smeaton's tower was erected, and the frequent eclipsing of the light by heavy seas during stormy weather. The latter defect was of little importance for many years after the erection of Smeaton's lighthouse, when individuality had not been given to coast lights; but with the numerous coast and ship lights now visible on the seas surrounding this country, a reliable distinctive character for every coast light had become a necessity. The tower of the New Eddystone was a concave elliptic frustum, with a diameter of 37 feet at the bottom, standing on a cylindrical base 44 feet in diameter, and 22 feet high, the upper surface forming a landing platform 2 feet 6 inches above high water. The cylindrical base prevented in a great measure the rise of heavy seas to the upper part of the tower, and had the further advantage of affording a convenient landing platform, thus adding considerably to the opportunities of relieving the lighthouse. With the exception of the space occupied by the fresh water tanks, the tower was solid for 25 feet 6 inches above high-water spring-tides. At the top of the solid portion the wall was 8 feet 6 inches thick, diminishing to 2 feet 3 inches in the thinnest part of the service room. All the stones were dove-tailed both horizontally and vertically, as at the Wolf Rock Lighthouse. Each stone of the foundation courses was sunk to a depth of not less than one foot below the surface of the surrounding rock, and was further secured by two Muntz metal bolts 1½ inch in diameter, passing through the stone and 9 inches into the rock below, the top and bottom of each stone being fox-wedged.

Irrigation Works in Italy.—The irrigation system of Italy is probably the most complete in the world, and still it is constantly being increased; it forms a part of the elaborate system of defense against floods necessitated by the conformation of the northern provinces. According to the latest official statistics the irrigation canals of Piedmont alone give 125,550 gallons per second, distributed over 1,340,000 acres; and those of Lombardy 35,355 gallons per second, distributed over 1,680,400 acres. These great works have not been, comparatively speaking, expensive. The Cavour canal, constructed within the last few years draws its supply from the rivers Po and Dora Baltea. It gives a flow of 29,200 gallons per second, waters nearly 40,000 acres, and cost £1,600,000, about £32,200 per mile. It was constructed in four years, and measures are now under consideration for increasing its debit by 5,300 gallons per second. A smaller canal subsidiary to it, gives 18,540 gallons per second, and cost £24,154 per mile. The largest canals are the Cavour and its subsidiary canal just mentioned; the Muzza, Agliano, and Naviglio Grand. The smaller of these gives 13,200 gallons per second. Below this point the canals become very numerous, and interspersed all over the country. These canals are not only used for purposes of irrigation, but also to supply motive power, by which again the water is raised to districts lying upon a higher level. On the steep slope of the Dora Baltea, not far from Turin, three canals (the Torea, Agliano, and Rotho) flow parallel to each other, on different levels, while the water is used at the top of the hill, 62 feet above the highest of them. The arrangement adopted is as follows: A stream of 154 gallons per second is diverted from the Torea canal and carried down the hill in a leaden pipe until it meets the Agliano canal. Here it is pumped up to the summit level by eight pumps, worked by four turbines driven by a fall of water taken from the Agliano canal, and allowed to flow down into the Rotho. By joining this latter it is used for irrigation, and thus not a drop is wasted. The great principle of Italian engineers is to work on a large scale, thus attaining at the same time efficiency and economy, and avoiding constant alterations and additions; and it is by such means that the extraordinary fertility of Northern Italy is produced and maintained.

IRON AND STEEL NOTES.

On the Conditions in Which Carbon Exists in Steel.—Abel and Deering have made a series of experiments in the hope of obtaining information on the condition in which carbon exists in steel as it is left by the cold-rolling, as well as in its hardened, annealed, and intermediate conditions. Two series of experiments were made. In the first series the steel was in the form of 12 disks 2.5 inches in diameter, and 0.01 inch thick, each weighing about 6.5 grams. In one disk, hardened, the silicon was determined to be 0.20 per cent. A cold-rolled disk gave 1.108 total carbon, a hardened one, 1.128: an annealed inside disk

0.924, and a similar outside one, 0.860. Three disks were used for estimating the so-called uncombined carbon, by gently heating them for three hours in 100cc of hydrochloric acid of sp. gr. 1.10. The cold-rolled one left 0.096, the annealed inside one 0.052, and the hardened one 0.035 per cent. carbon. Three others were placed in a cold saturated solution of potassium dichromate acidulated with $\frac{1}{20}$ of sulphuric acid, being supported on sieves of platinum gauze. The cold-rolled, the annealed and the hardened disk each left a small quantity of black particles which appeared spangly under the microscope, and which were attracted by the magnet. Upon analysis the particles from the cold-rolled disk gave 1.039 per cent. carbon, and 5.87 per cent. of iron; those from the annealed outside disk 0.830 and 4.74, and those from the hardened disk 0.178 and 0.70; calculated on the total weight of the disks. Thus the chromic acid treatment has left nearly the whole of the carbon from the cold-rolled and the annealed disks in the form of a carbon-iron compound corresponding closely to the formulæ Fe_6C_5. But, on the other hand, in the case of the hardened disk only about one-sixth of the total carbon was left undissolved by the chromic treatment. The last disk was submitted to the action of a chromic solution containing a large excess of sulphuric acid. The black residue gave on analysis 0.84 carbon, and 1.104 iron, showing that the carbide had broken down. To study the carbon-iron compound more carefully, and especially to learn (1) whether it is independent of the strength of the chromic solution, (2) whether the percentage of carbide in the steel is constant, and (3) how much of its carbon remains unconverted into hydrocarbons upon treatment with hot hydrochloric acid, a second series of experiments was made. The steel used was in the form of a thin sheet 0.008 inches thick, weighing 175 grams, cold-rolled from an ingot melted from cemented blister steel. On analysis it contained 1.144 carbon, 0.166 silicon, and 0.104 manganese. Four experiments were made, from 7 to 7.5 grams of steel being used in each, the strength of the chromic solution varying. The results seem to show that the material separated from cold-rolled steel by the action of chromic and sulphuric acid below a certain strength contains an iron carbide approximating to the formula Fe_2C or its multiple. Hence these experiments appear to sustain the view that the carbon in this steel is not simply diffused mechanically through the mass of steel, but exists in the form of an iron carbide, a definite product capable of resisting the oxidizing effect of an agent which exerts a rapid solvent action upon the iron through which the carbide is distributed. Future experiments must decide whether the carbide varies in its composition in different descriptions of steel similarly treated, though the experiments above given suggest that hardening diminishes the power of the carbide to resist the decomposing effect of the chromic solution.

On the Effects of Compression on the Hardness of Steel.—By M. Lan.—Experiments were made at the ironworks of Saint Jacques, at Montlucon, confirmatory of the results already presented by Mr. Dumas in the name of Mr. Clémandot, that in compressed steel there was an increased hardness as compared with uncompressed. Further results have been obtained. Steels have been analyzed, compressed, and uncompressed, containing different proportions of carbon. The proportion of combined carbon, as regards the total carbon, has always been found to be greater in the compressed as compared with the non-compressed steels. The experiments were made on elongated shot, the samples being taken from four different points in the depth, and the combined carbon was tested by the Eggertz process, and the total carbon by Boussingault's. The comparative results are so constant that one table of results will suffice:

	Compressed.		Uncompressed.	
Carbon, total per cent,	0.70		0.70	
Combined carbon at A	0.60	Mean 0.585	0.49	Mean 0.49
B	0.59		0.50	
C	0.55		0.47	
D	0.60		0.50	
Free carbon by difference	0.115		0.21	

Thus the compressed steel has more combined and less uncombined carbon. The same results were obtained by sudden cooling in the ordinary manner of hardening. Hence compression produces the same physical effects as sudden cooling in steels.

RAILWAY NOTES.

A Railroad in Palestine.—The first railroad in Palestine is being laid out, and the preliminary survey has been completed as far as the Jordan. It is to be run between Acre and Damascus, and is called the Hamidè line, being named after the Sultan Abdul Hamid, and probably one reason why the firman has been granted so easily lies in the fact that it passes through a great extent of property which he has recently acquired to the east of the plain of Esdraelon. The concession is held by ten or twelve gentlemen, some of whom are Moslems and some Christians, but all are Ottoman subjects resident in Syria. Among the most influential are the Messrs. Sursock, bankers, who own the greater part of the plain of Esdraelon, and who have therefore, a large interest in the success of the line.

Starting from Acre, it will follow the curve of the bay for ten miles, in a southerly direction, at a distance of about two miles from the beach. Crossing the Kishon by a 60 foot bridge, it will turn east at the junction of a short branch line, two miles long, at Haifa. Hugging the foot of the Carmel Range, so as to avoid the Kishon marshes, it will pass through the gorge which separates that mountain from the lower ranges of the Galilee Hills, and debouch in the plain of Esdraelon. This plain it will traverse in its entire length. The station of Nazareth will be distant about twelve miles from that town; there may, however, be a short branch to the foot of the hills. So far there has only been a rise from the sea level in twenty miles of 210ft., so that

the grade is imperceptible, It now crosses the watershed and commences to descend across the plains of Jezreel to the valley of the Jordan. Here the Wady Jalud offers an easy incline as far as Beisan, the ancient Bethshan, and every mile of the country it has traversed so far is private property, and fairly cultivated.

At Beisan it enters upon a region which has, partly owing to malaria and partly to its insecurity, been abandoned to the Arabs, but it is the track of all others which the passage of a railway is least likely to transfigure, for the abundance of the water, which is now allowed to stagnate in marshes, and which causes its unhealthiness, is destined to attract attention to its great fertility and natural advantages, which would, with proper drainage, render it the most profitable region in Palestine. Owing to the elevation of the springs, which send their copious streams across the site of Beisan, the rich plain which descends to the Jordan, 500ft. below, can be abundantly irrigated. There is a little bit of engineering required to carry the line down to the valley of the Jordan, here 800ft. below the level of the sea, which is then followed as far north as the Djisr el Medjomich.

Near this ancient Roman bridge of three arches, which is used to this day by the caravans of camels which bring the produce of the Hauran to the coast, the new railway bridge will cross the Jordan, probably the only one in the world which will have for its neighbor an actual bridge in use which was built by the Romans, thus, in this now semi-barbarous country, bringing into close contact an ancient and a modern civilization. After crossing the Jordan the line will follow the banks of that river to its junction with the Yarmuk, which it will also cross, and then traverse a fertile plain of rich alluvium, about five miles long and four wide, to the banks of the ridge which overlooks the eastern margin of the Sea of Tiberias. This is the extent to which the survey has been completed.

It is not decided whether to rise from the valley by the ridge which overlooks the Yarmuk, or to follow the east shore of the Lake of Tiberias to the Wady Semakh, which offers great advantages for a grade by which to ascend nearly 3,000ft. in about fifteen miles. This is the toughest bit of engineering on the line, and is in close proximity to the steep place down which the swine possessed by the devils are said to have rushed into the sea. Once on the plateau, it will traverse the magnificent pasture lands of Jaulan and the grain growing country of Hauran, with probably a short branch to Mezrib, which is the principal grain emporium, and one of the most important halting places on the great pilgrimage road from Damascus to Mecca. It is calculated that the transport of grain alone from this region to the coast will suffice to pay a large dividend upon the capital required for the construction of the road, which will be about 130 miles in length. The grantees have also secured the right to put steam tugs upon the Lake of Tiberias, and under the influence of this new means of transportation, the desolate shores will undergo transformation.

BOOK NOTICES.

Publications Received.

The Magazine of American History. Edited by Mrs. J. Lamb.

Third Annual Report of the United States Geological Survey. By J. W. Powell, Director.

Proceedings of the Institution of Civil Engineers. Iron and Steel in Tension, Compression Bending, Torsion and Shear. By Percy Vavasseur Appleby, stud., Inst. C. E.

Abstracts of Papers in Foreign Transactions and Periodicals.

Monthly Weather Review for October. Washington: Signal Office.

The Elements of Military Administration. Part I. By Major J. W. Buxton. London: Kegan Paul, Trench & Co.

The title of this book sufficiently explains its scope to the military profession. The divisions of the subject are somewhat intricate, doubtless necessarily so. The five chapters which form this part are:

Chap. I. Introductory; Chap. II., The War Office and Central Administrative Department; Chap. III., District Administration; Chap. IV., Regimental Administration; Chap. V., Conclusion of First Part.

The second, third, and fourth chapters are subdivided three times into numbered and lettered sections.

The treatise bears marks of careful and patient labor.

Military Law: Its Procedure and Practice. By Major Sisson C. Pratt, R. A. London: Kegan Paul, Trench & Co.

This is the fifth volume of the Military Hand-Book Series. The editor of the series, Colonel C. B. Brackenbury, says of the present manual: "It has been compiled by an acknowledged authority on the subject, in order to aid officers and non-commissioned officers in overcoming the many difficulties which attend the study of the legal part of their profession. It must be looked upon as an aid to, and not a substitute for the ordinary legal code."

The whole subject is presented in twenty-seven chapters, which are subdivided into numbered paragraphs.

Electricity, Magnetism, and Electric Telegraphy. By Thomas D. Lockwood. New York: D. Van Nostrand. Price $2.50.

The advance sheets of this book are alone available at the moment of reviewing.

The author, with a full comprehension of what is already published upon these subjects, and with a thorough appreciation of what a learner, who is inclined to be practical, desires to know, has set forth the elementary principles of electrical science in a manner easily comprehended by an unscientific reader.

The work is not offered as a substitute for such books as Fleeming Jenkins' Electricity and Magnetism, and Culley's Hand-Book of Practical Telegraphy, but as an aid to the learner in comprehending them.

The catechism method has been employed throughout; the principle or fact to be ex-

plained is demanded in the form of a brief question, which is immediately answered in that direct and concise way which this plan ot presenting truths demands.

An author who constantly maintains the attitude of answering the questions of a learner is bound to avoid wearying his pupil by prolixity. The learner will have no cause to complain here. The elucidations are all brief, but sufficient for the proposed object. The student who is possessed of but a small stock of scientific knowledge may become quite familiar with the theory and practice of telegraphy.

REPORT OF THE NEW YORK STATE SURVEY FOR THE YEAR 1882. By James T. Gardner, Director. Albany: Weed, Parsons & Co.

All engineers who are interested in accurate methods of surveying must surely regard with interest the progress of this survey.

The brief text of the present report makes mention of the fact that on account of the unsteadiness of the air in the day time between 8 A. M. and 4 P. M., night signaling has been resorted to. This has been done by quite inexpensive apparatus, and in the opinion of the Director promises to give even for distances of forty or fifty miles satisfactory results.

Large additions have been made to the geographical locations, and to the list of elevations of stations.

ELECTRICITY IN THEORY AND PRACTICE, OR THE ELEMENTS OF ELECTRICAL ENGINEERING. By Lieut. Bradley A. Fiske, U. S. N. New York: D. Van Nostrand, publisher.

(Abstract from Electrical Review.)

We are constantly in the receipt of letters from all parts of the country asking us to explain the theory of this or that application of electricity. Many of these come from persons whom we would really like to accommodate. Unfortunately for us, however, there are but seven days, of which one is usually a day of rest, and the space of a newspaper, like the duration of human existence, is limited. We are, therefore, not always able to satisfy those who are, perhaps, not inclined to be pleased, since we are not always able to satisfy ourselves.

This being the case, we were not a little gratified, as we turned from page to page of a recent production, in which the theory and practice of electrical engineering go, as one might say, hand-in-hand, and where neither are administrated *ad nauseam.* Its author, Mr. Bradley Fiske, is a conscientious, able workman, and we cordially recommend his work to the attention of the multitude of ambitious and ingenious electricians and amateurs throughout the country, who are not so fortunate as to possess a theoretical education.

Like many objects which are shown us as evidences of the handiwork of remote ages, the theories, which from time to time are presented to us, as explanatory of certain natural laws, are apt to mislead us, unless we have confidence in those who introduce them.

There are those who form theories before the evidence is all in, and having once made up their minds are wont to look upon the very evidence, which should have served to set them right, as a corroboration of the fallacy which they have adopted as the truth. The evil done by such persons is incalculable; it misleads the unskilled, casts a doubt upon the work of intelligent thinkers, and adds still further to the number, already very large, of inaccurate textbooks.

Judging from the work before us, the author is one of those men, unhappily somewhat rare, who are by no means quick to accept theories unless well sustained, and only then after careful examination. The caution and lack of precipitation, which is evinced where dangerous ground is trodden, is at once his best recommendation and the student's safeguard.

A portion of the work is, as its title indicates, rudimentary; but we think it all the more valuable on that account. Those who have experience with electricians and mechanics are aware how inaccurate is technical information many of these possess, and need not be told that some of them know little of the theory of their work. Such persons have here in compact form and regular order a large amount of theoretical information concerning the most recent electrical work of the day.

We have heard professors in the colleges hereabouts expatiate for hours upon electrical themes of the last century, whilst the major portion of their audience sunk into pleasant slumbers, and the lesser went out to sleep under the trees where it was cooler. Had they had such a book as this before them, they could have so interested their audience, that it would have been very glad to remain awake.

If you want to go through any of the old and white-haired electrical tricks with amber and gutta-percha and friction machines, and frogs' legs, &c., you needn't look in this book. It is devoted to other purposes. Again, we have seen some books, containing valuable information, too, such as the "Linear Algebra," and the like, which, to a certain extent fail of their object because they are not sufficiently explanatory and clear. A man who has made mathematics a life's study, and talking to another with a similar contempt for life's objects, may not unreasonably begin in the middle of a complex explanation with the remark, "It is obvious that the *n*th power" of so and so is equal to so and so; but when he is addressing a miscellaneous audience, composed of but indifferent mathematicians, he should know that the computation is obvious only to himself.

Fortunately for the reader, Mr. Fiske has not the bad habit of presupposing in his auditor an experienced and skillful electrician; but takes the trouble to explain the theory carefully and lucidly as he goes along, and one has not to read far to discover that he is thoroughly familiar with that which he essays to describe.

Passing over the more elementary chapters in which magnetism, frictional electricity, work and potential voltaic batteries, and the laws of currents are dwelt upon, we come to a chapter on secondary or storage batteries, in the explanation of which we could have wished the author had devoted more space. For the possibilities of the secondary battery are very

great; many able men are devoting themselves towards lessening their defects, and the small successes which from time to time are recorded, would indicate that the secondary battery of to-day bears about as much resemblance to the future secondary battery as the bladder which Dr. Clayton used as a gas-holder in 1688 does to the gasometer of city gas works, or as James Watt's crude engine does to the Atlantic steamer.

We are not surprised to find that Mr. Fiske devotes much space to that very important and often neglected department of electrical science, measurement. Nor are we surprised to find that he treats the subject skillfully. We have, if we are not very much mistaken, seen some work of this kind before in "*Van Nostrand's Engineering Magazine*," which had his name appended; work which, if Mr. Fiske is, as we suppose, commencing his career, promises a position in that part of the profession which in this country has never yet been known to be crowded, viz., the top. So far as we could see, the author makes no attempt to devise new methods of measurement, but wisely contents himself with giving clearly and succinctly the theories upon which those in vogue are founded.

While there is absolutely nothing new in the author's "Measurement of Electrical Resistance," we do not remember ever to have seen the measurement so clearly demonstrated in so few words, and where a tangent or sine galvanometer is used his explanatory formulæ, considering how complex they might have been made, are models of neatness.

The paragraph following the title, "Limit to Telephonic Transmission," is like all the theoretical handiwork of the author, lucid and to the point; but it is to be regretted that Mr. Fiske, who seems to be an adept in the matter, did not dwell longer on this point, and since this work is likely to find much favor, as well with the public reader as with the practical engineer, he had opened to him a fine opportunity to disabuse the public of the prevalent idea that the telephone is about to supersede the telegraph in long-distance transmission, which, outside the laboratory, is not likely to be the case.

In treating the incandescence system of electrical lighting, we think Mr. Fiske has, unintentionally, of course, done grave injustice to the man who, in these later days, succeeded, after nights and days of toil, in perfecting a mechanism which should make incandescent lamps of small intensity a practical success. The credit he gives to Star is just, but the impression he gives that the modern incandescence lamp is but an adaptation of the mechanism put together by Star outside of the use of carbon in a vacuum, will not stand the test of a comparison. Again, such persons as Lane-Fox, who made an alleged improvement in something he had no hand in thinking out, should not be mentioned in the same breath with the man whose property they have filched, any more than in speaking of a highwayman and his victim we should say, "these two gentlemen differ as to who is the rightful owner of the loot."

It is not necessary that one should be a partisan in order to render unto each man his own: a just man can allow the claims even of his bitterest foe, and openly deny the claims of his nearest friend as opposed to them.

Mr. Fiske seems to espouse the cause of one who, though making some valuable improvements in the telephone could not even by his firmest ally be credited with having invented it; but when he finds that another man, many years ago, made some only partially successful experiments in incandescence lighting, he seems inclined to disallow the claims of one who has made a tangible success out of a little more than a crude conception.

We are sure that this is not intentional, but we speak of the effect Mr. Fiske's book is likely to produce in the mind of the public reader, who is inclined to adopt the opinions contained in the books he reads.

Under the heading, "Electrical Efficiency of Machines," Mr. Fiske gives some interesting and valuable formulæ, and as these seem the most direct way of ascertaining the electrical efficiency of machines, they are likely to prove of service to all working electricians.

MISCELLANEOUS.

THE COURSE OF ELECTRICAL ENGINEERING AT CORNELL UNIVERSITY.—It was announced last spring that Cornell University would, at the opening of the fall term, receive students for a course of instruction in electrical engineering.

The requirements of admission are the same as to the other technical courses, civil and mechanical engineering, etc., that have been so long established, viz.: algebra and geometry, in addition to the common English branches.

The course in the university includes the higher mathematics, the French or German language, rhetoric, descriptive geometry and mechanical drawing, physics, chemistry, the steam engine and other motors, besides the special work in electricity, which constitutes the special features of the course.

The instruction in electricity includes the use of all the instruments employed in making electrical measurements, with the means employed in testing their accuracy, and determining their constants; the construction and testing of telegraph instruments; the methods of testing telegraph lines and cables; the theory, construction, and testing of dynamo machines; the study of the different systems of electric lightning, etc., etc.

The course is designed to give a thoroughly practical, combined with a theoretical training, and the electrical engineer will find use for every subject named. The higher mathematics and pure sciences will be found of especial importance, now in the infancy of the applications of electricity, when so much is in the tentative stage.

The facilities for this institution at Cornell are of the best. A large new building has just been completed, in which are several rooms specially equipped for electrical tests and measurements. All the important instruments for measurements are provided. Dynamo machines

and systems of electric lighting are in practical operation in the building, and, as far as possible, all the applications of electricity are exemplified.

The student spends a considerable portion of his time in the laboratory, making experiments with various instruments. He determines the laws and constants of galvanometers; measures the resistances of various conductors; measures the currents used for electric lights; determines the energy expended in charging, and that developed in discharging storage batteries; determines the efficiency of machines and motors; in short, does exactly what the engineer needs to do in the factory or office.

The Meteorological Society.—Cloud Observations.—At a meeting of the Meteorological Society, the Hon. F. A. Rollo Russell, M.A., read a paper upon "Cirrus and Cirro-Cumulus." By way of preface to some of his remarks, we may state that the attention of meteorologists is now being seriously drawn to the phenomena presented by the higher clouds, it having been discovered that they often afford trustworthy clues to the nature of coming weather. As a celebrated example it may be mentioned that the Rev. Mr. Ley, who is an authority on the subject, one fine bank holiday happened to be in London, and noticing certain indications given by the upper clouds, he telegraphed from the Strand to Mr. Robert Scott, of the Meteorological Office, "Heavy thunderstorm ordered for four o'clock this afternoon," and surely enough at four o'clock a thunderstorm of the heaviest type was crashing over London. One reason why cirrus clouds give early indications of approaching changes in the weather is that the axis of a cyclone is not vertical, but the upper part is inclined in the direction in which the whole cylone is moving, consequently it sometimes affects the upper clouds before the influence of the cyclone is felt at certain places on the earth below, from which these clouds are within view.

Mr. Rollo Russell said that next to frequent readings of the barometer and a knowledge of the distribution of atmospheric pressure, cloud observations, especially of cirrus, were of great use in forecasting the state of the weather. Cirrus is generally supposed to float at heights varying from 16,000 ft. to 40,000 ft., and more; but according to Mr. Glaisher's balloon observations, the height may probably be sometimes more than fifteen miles. It is not easy to definitely describe cirrus. Its appearance suggests electrical influence in the determination of form; it is the only cloud which presents angular forms and nearly parallel threads, apparently kept apart from each other by repulsion; it is the only cloud which is not normally rounded in outline, and which is sometimes composed of striæ nearly at right angles to each other. It is also the only cloud which sometimes appears to radiate from a point near the horizon, thus showing that the lines are parallel to each other, and their real length in their apparent direction.

The speaker then proceeded to classify the cirrus clouds observed by him during the last eighteen years, chiefly in the neighborhood of London and the South of England. He divided them into twelve classes, and in some cases stated what kind of coming weather the presence of one or other of the forms of cirrus indicated. He said that "bar or ribbed cirrus," though somewhat uncommon, is at least equal in value to the falling barometer as a danger signal; it consists of nearly parallel bars of dense cirrus, separated by intervals through which the sky is visible; sometimes this form is so developed as to remind the observer of a gridiron. This kind of cirrus generally gives from twelve to forty-eight hours' notice of an approaching storm.

Mr. Russell then argued that the Government should incur some little expense in establishing daily cirrus observations. He stated that a great and terrible loss of life occurred on the east coast in the storm of the 14th of October, 1881, and as this storm was traveling with great rapidity, and broke upon the west coast during the night, when the office was closed, a warning from London was out of the question. The barometer had sunk to a low point on the Berwickshire coast when the fishermen went out, but this warning seems to have been altogether neglected on the spot.

The cirrus observed on the 13th of October gave earlier information than the barometer of the coming storm, and with a regular system all the ports might have received warning on the afternoon of the 13th. It may be stated generally, he said, that cirrus of a long, straight, feathery kind, with soft edges and outlines, or with soft delicate colors at sunset and sunrise, is a sign of fine weather. Curly wisps and blown back pieces are not a bad sign, but their exact appearance should be noted, the rapidity of their movement, and the definiteness of their outlines. When the tails are turned downwards, fair weather or slight showers often follow. Misty, confused, and curdled cirrus should be carefully noted, but does not always foretell bad weather. The harder and more distinct in outline, and the more particular forms are repeated, the worse the result. Long, hard, greasy-looking streaks, with rounded edges or knobs, whether crossed with fibres at right angles or not, are a sign of storms; but the storm may pass at some distance from the point of observation. Cottony shreds, either by themselves or detached from a long streak, rounded and clear in outline, something between cirrus and cirro-cumulus, indicate dangerous disturbances. Regular wavy tufts, with or without cross-lines, are bad, especially if the tufts end not in fibres, but in rounded knobs. Feathery cirrus in thick patches at equal distances apart is a sign of a storm, or any appearance of definite waves of alternate sky and cloud. So is any regular repetition of the same form. Slightly undulating lines of cirrus occur in fine weather, but anything like a deeply indented outline prcedes heavy rain or wind. Cirrus simply twisted or in zigzag lines of a fibrous character often appears in fine weather, and if not hard or knotted or clearly marked off from a serene sky, does not often precede any important change; but detached patches, like little masses of wool or knotted feathers, in a clear sky and of unusual figure, moving at more than the average rate, precede disturbances of great magnitude.

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SOME EXPERIMENTS UPON THE "OTTO" GAS ENGINE.*

BY MORGAN BROOKS AND J. E. STEWARD, Stevens Institute of Technology, 1883.

INTRODUCTION BY PROF. R. H. THURSTON.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

INTRODUCTION.

The following report on the trials of the Otto Gas Engine, by Messrs. Brooks and Steward, embodies a very careful examination, both theoretically and experimentally, of the performance of that form of motor, made with a view to determine, not only the actual efficiency and economy of the machine, but also the extent and the proportions of the losses met with in its ordinary operation. Some earlier investigations had been directed into this line of research, and a fairly good idea had been obtained of the causes of waste in the gas-engine. It seemed to the writer, however, that the collection of other data bearing upon this subject was desirable, and he had already obtained some figures from experimental work that were instructive and interesting. Among the latter may be mentioned a series of results obtained during the investigations, made under his direction, and which included trials of the Otto engine as well as other forms of gas-engine, in which the distribution of heat in useful and lost forms of energy was determined with care.

In these cases, the consumption of water-gas varied from 21.2 to 23.4 cubic feet per hour and per horse-power in engines of 6 or 7 indicated horse-power, up to 23.5 to 24.5 with engines of two horse-power or less. The friction of mechanism ranged from 4 to 5 per cent of the total energy of combustion, and from 40 % power, in the smaller to 20 per cent in the larger engines; the waste at the exhaust was from 12 per cent of the total heat of combustion, in the small to 24 per cent in the large engines, and from 100 to 200 per cent of that transformed into useful work.

The water-jacket carried off from 45 to 55 per cent. of all the heat supplied by the combustion of the gas. For example: the distribution of the heat of combustion, in one case worked for the writer by his senior assistant, Mr. Cartwright, was as below:

Useful dynamometric work..	14.27
Work of the pump.........	0.42
Friction of mechanism......	4.10
Lost in exhaust............	23.55
Ditto in water-jacket........	46.90
Radiation, &c..............	10.76
Total heat supplied.....	100.00

* We take pleasure in acknowledging our obligations to Mr. A. W. Schleicher, of Schleicher, Schumm & Co., Philadelphia, for the use of the ten horse-power engine, supplied at the request of Prof. Thurston, for our work, and upon the performance of which our results are based. We are similarly indebted to the American Meter Co., of New York City, for the loan of standard meters used during the tests.—M. B. and J. E. S.

This engine developed seven horse-power at the brake, and indicated 8.9 H. P., consuming 21.2 or 27.6 cubic feet of gas, accordingly as the indicated or the dynamometric horse-power was made the basis of the calculation.

It seemed to the writer desirable that this method of investigation should be further developed, and that a comparison of the actual with the thermodynamic performance of the gas engine should be made, systematically determining all the data needed to make such a comparison complete, and as nearly exact as possible. This work was undertaken by Messrs. Brooks and Steward. These gentlemen had already had sufficient experience, and had shown themselves sufficiently skillful, in work of this character to insure exact results. They were given every facility that the Stevens Institute of Technology could furnish, and, at the request of the writer, Messrs. Schleicher, Schumm & Co., the builders of the engine, prepared one of their 10 horse-power Otto engines for trial, and forwarded it to the writer from Philadelphia. This engine was set up at the Stevens Institute of Technology, as described in the report, and connected as shown in the plan, Fig. 1. At the request of the writer, the American Meter Co. promptly and cheerfully supplied meters, including one of unusual size for the purpose of measuring the air, as well as the gas supplied, a measurement never before undertaken, so far as the writer is informed. The Meter Co. also afforded all needed facilities for testing the meters, before and after the trials.

The results of the investigation are given with all necessary detail in the body of the report. It will be seen, as one result of the precaution taken to measure the supply of air, that the relative volumes of air and gas are found not to be precisely determinable from figures obtained without the use of an air meter. This precaution was also found to have value as permitting a correct determination of the effect of varying the supply of air and of gas independently, either with or without change of proportions. (Section 5.)

The determination of the proper proportion of air to gas is important and interesting (section 9); and the comparison of the lines of the indicator with theoretic curves is still more interesting and novel, as well as very instructive (sections 12 and 13). The fact that combustion is progressive, even into the expansion period, is probably here, for the first time, exhibited by direct investigation (section 15). The analysis of the efficiency of the engine affords a means of making a comparison of the thermodynamic with the actual efficiency of this class of heat-engine. It is seen that the total heat accounted for thermodynamically, consisting of that transformed into work and that expelled with the exhaust, amount to 34 per cent. of the total heat actually supplied, and that the other wastes, by way of the water-jacket and otherwise, amount to 66 per cent. The thermodynamic efficiency is therefore about 17-32ds, and the actual efficiency 17-100ths, or 52 and 17 per cent., respectively. It may probably be stated, as a general fact, that in gas engines having water-jackets, about twice as much gas is demanded by even good machines, as thermodynamic calculation would indicate, and it would seem equally evident that, could this loss by the jacket be evaded, the gas-engine would at once assume a vastly more important position as prime-motor than it to-day occupies. Even now, the gas-engine has found innumerable applications; and, as its economy is increased by improvements in design and construction and by the introduction, as suggested in the closing paragraph of the report, of special heating gases, costing sometimes but 50 cents per thousand feet, and bringing down the cost per day to but about $2.50, its field will indefinitely widen. Its convenience and its safety are advantages which go far also toward compensating every economical disadvantage.

Robert H. Thurston.

Stevens Institute of Technology,
January, 1884.

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1. The Engine and Accessories.

Below are the dimensions of the principal parts of the engine, and of such accessories as are involved in the calculations.*

* For complete description see U. S. Letters Patent, No. 196,473, dated October, 1877.

Stroke	14 in.	(356 mm.)
Diameter of piston	8.5 "	(216 ")
" " piston rod	1.75 "	(44 ")
" " connecting rod (crank end)	2.5 "	(63 ")
Diameter of connecting rod (piston end)	2.25 "	(57 ")
Diameter of crank shaft	4 "	(102 ")
" " fly-wheels	66 "	(1680 ")
" " brake pulley	30 "	(762 ")
Length of brake arm	16.5 "	(420 ")
Weight of both fly-wheels	1650 lbs.	(750 kilos)

Clearance (compression chamber) 38% total cylinder volume.

The ground plan (Fig. 1) shows the general arrangement of the engine, piping, and apparatus employed in making the tests.

A sixty-light dry meter was used, connected directly with the street gas main, to measure the gas used by the engine, exclusive of the igniting flames. As the engine takes gas suddenly and at intervals, it is necessary to insert a flexible rubber bag in the gas supply pipe between the meter and engine, to act as a gas reservoir, and so relieve the meter of all strain.

The supply of air was also measured. A three-hundred light meter was used for this purpose, and a pair of large rubber bags was inserted in the air pipe for a purpose similar to that of the gas bag as just described. A small fan-blower, running at about 3,000 revolutions per minute, kept these bags constantly filled with air, the pressure of which was controlled by a check valve in the pipe near the blower. A three-way cock in the air pipe under the engine (not shown in the figure) allowed the air to be taken either through the large meter or directly from the room.

The water required for the water-jacket was measured by a Crown water meter placed near the gas meter, and its temperature, both before entering and immediately after leaving the water jacket, was measured by a standard thermometer.

The three meters used were tested just before they were put in place, and an allowance has been made for the readings of the air meter, the only one appreciably inaccurate.

A Bulkley pyrometer was placed in the exhaust pipe as near as possible to the engine, giving the temperature of the discharged gases.

For the purpose of measuring the useful work of the engine a Prony brake, consisting of two iron hoops with blocks of wood fastened at short intervals, was clamped around a thirty-inch pulley on the crank shaft of the engine. A strut transmitted the pressure derived from the brake directly to a Fairbanks' platform scale. This brake worked very smoothly without use of water.

The indicator used was of the Tabor pattern. It was placed directly upon the cover of the exhaust passage, and motion was taken from the cross-head by means of a cord running about a stepped pulley to reduce the stroke.

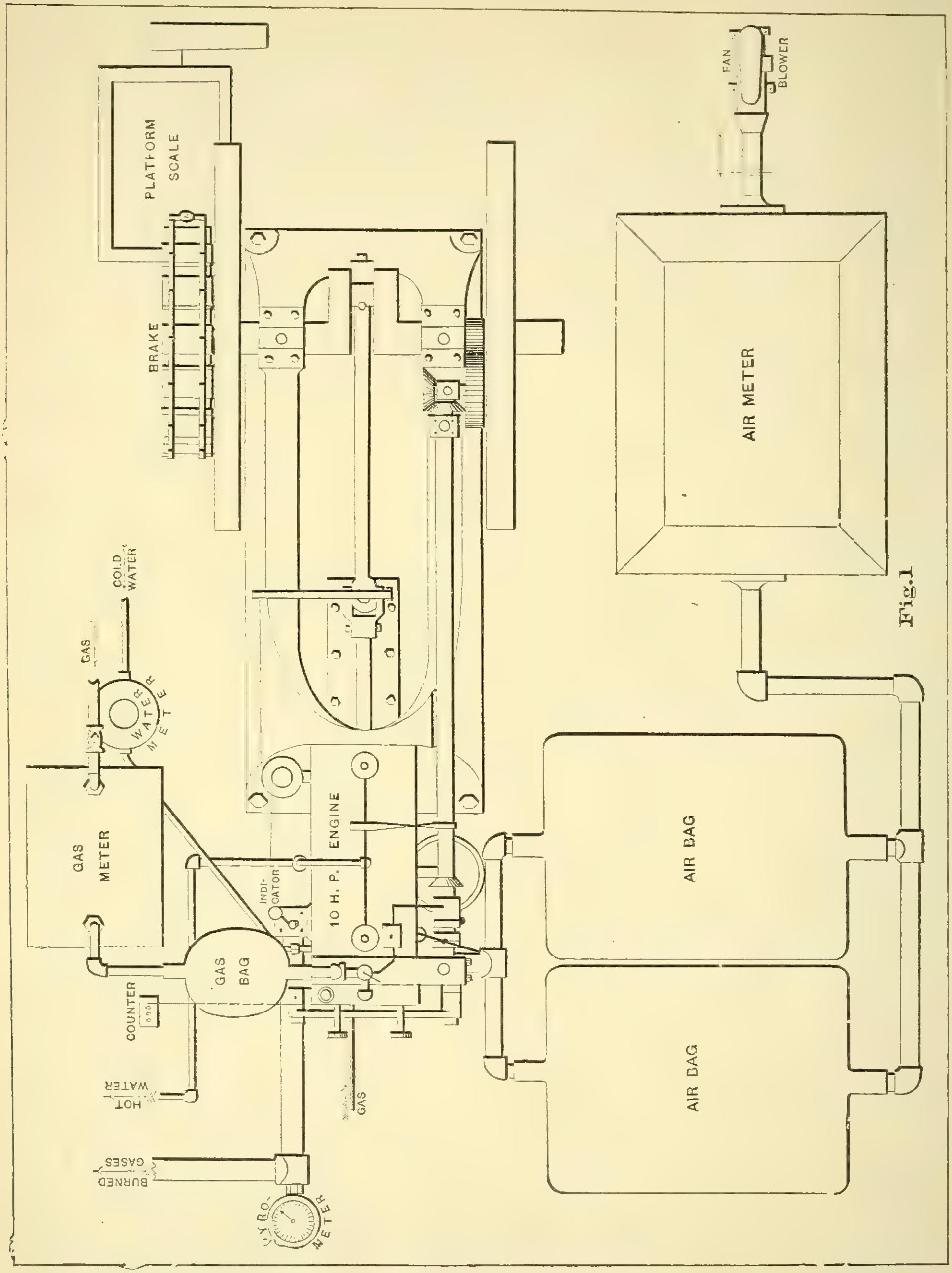
To measure the speed of the engine a speed counter was attached to the link moving the slide valve, thus recording the number of double revolutions.

2. Summary of Tests.

The observations from which the results given in the tables were obtained, were usually made at intervals of five minutes during the tests, but oftener when any marked variation was noticed. The gas pressure was about 30 millimeters ($1\frac{1}{4}$ inches) water column, and the air pressure, when the large meter and blower were used, averaged 50 millimeters (2 inches).

All results have been reduced to horse-power, as being the most convenient form for ready comparison. The "horse-power in gas burned" was calculated from the analysis of Hoboken gas, and is the dynamic equivalent of the heat capacity of the gas. The "horse-power lost by exhaust" and "by water jacket" were calculated from the specific heats of the discharged gases and of water. The indicated horse-power was computed in the ordinary way from the number of explosions and the mean effective pressure. The area of the cards between the compression and expansion lines was measured with an Amsler planimeter. An allowance was made for the area between the exhaust and admission lines, representing work done in expelling the burned gases, and in drawing in the fresh charge. This allowance is equivalent to a little more than one-tenth of an atmosphere mean effective pressure.

The figures given for gas consumed do not include the amount burned by the two igniting flames. An allowance of 7 cu. ft. (200 liters) per hour will cover this.



PLAN OF ARRANGEMENT FOR GAS ENGINE TRIAL.—STEVENS INSTITUTE OF TECHNOLOGY, 1883.

3. Friction of Engine.

The difference between the indicated and the actual work gives the amount of friction in the engine. In tests 10 and 17 this falls below 15 per cent. of the indicated work, a somewhat doubtful figure. The average of all the tests at full power is 18.6 per cent friction, a remarkably good result.

4. Gas Consumption.

The consumption of gas per horse-power per hour is larger than that usu-

RESULTS OF TESTS.

	At Varying Powers.									At Full Power.										
Number of Test.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Date (1883)	5/29	5/28	5/30	5/30	5/30	6/4	6/4	6/4	5/29	5/28	6/4	6/4	6/4	6/4	6/4	6/29	6/29	6/29	6/29	6/30
Duration in minutes	15	15	30	14.5	30	10	15	10	60	30	30	30	30	30	30	30	25	20	10	30
Gas, cubic feet	12.5	17.5	63	41.5	99	28.5	46.5	34.7	249	109	115	122	105	114	115	117 5	89	72	37	105
Gas, liters	355	495	1785	1175	2805	805	1310	980	7050	3090	3255	3460	2975	3227	3255	3320	2520	2040	1050	2975
Air, cubic feet	385	—	—	—	—	277	416	283	1538	732	806	765	796	831	816	806	622	496	245	809
Air, liters	10900	—	—	—	—	7850	11800	8000	43550	20740	22850	21670	22540	23510	23040	22850	17650	14050	6940	22910
Ratio of air to gas	—	—	—	—	—	—	—	—	—	6.6	6.9	6.2	7.4	7.1	7.0	6.86	7.0	6 9	6.63	7.7
Amount of water through jacket, cubic feet	.94	4.6	4.3	4.	10.8	2.5	3.75	2.45	24.3	7.8	11.5	6.0	6.65	4.45	8.75	11.8	2.9	2.1	4.2	9.75
Amount of water through jacket, liters	26.5	131	122	113	306	71	107	69 5	690	221	325	170	188	127	248	334	82	58	116	276
Temperature of water in °C., entering	22	22	21	21	21	22	22	22	22	22	22	22	22	22	22	23	23	23	23	24
Temperature of water in °C., leaving	49	29	51.7	44.5	42	49.5	48	51	45.7	59.7	48	80	76	80	57	49	89.4	88.2	53	48.1
Number of double revolutions	1252	1247	2445	1184	2426	809	1212	801	4634	2100	2325	2299	2291	2461	2404	2371	1859	1472	750	2396
Number of revolutions per minute	167	166	163	163	162	162	162	160	154.5	140	155	153	153	164	160	158	149	147	150	160
Number of explosions	235	352	1224	788	1820	607	970	716	4620	Same as the number of double revolutions.										
Ratio of explosions to double revs.	.19	.3	.5	.66	.75	.75	.8	.89	.997	1	1	1	1	1	1	1	1	1	1	1
Average weight on brake arm, lbs.	0	0	64	.106	125	107	125	150	183	197	188	191	178	178	176	195	190	190	188	150
Average weight on brake arm, kilos.	0	0	29	48	57	49	57	68	83	89	85	87	81	81	80	88	86	86	85	68
Mean effective pressure (atmosphere)	4.2	3.3	4.4	4.3	4.1	4.	3.9	4.	4.2	4.2	4.1	4.1	3 9	3.9	3.8	4.04	3 85	3.97	4.15	3.5
Temperature of exhaust gases (°C.)	163	121	299	349	371	371	366	402	416	399	429	432	427	427	432	430	422	407	410	421
Indicated horse power	2	2.55	5.4	7.06	7.6	7 3	7 6	8.6	9.4	8 4	9.6	9.5	8.9	9.7	9.6	9.6	8.7	8.7	9.3	8.5
Effective (brake) horse power	0	0	2.73	4.5	5 3	4.5	5.4	6.3	7.6	7.2	7.8	7.8	7.1	7.7	7 5	8.1	7.4	7.2	7.4	6.3
Horse power lost by exhaust gases	2.3	—	—	—	—	6 7	6.8	7.6	7.6	6.9	7.9	7.8	7.7	8.1	8.	8.	7.3	7.	7.	7.8
Horse power lost by water jacket	6.6	8.1	11.5	16.8	19.8	18	17	18.7	35.2	25.7	26.2	30.5	31.2	22 4	26.8	26.9	20.3	17.6*	33.4*	20.6
Horse power in gas burned	12.2	17.1	30.9	42.1	48.5	41.9	45 6	51.	61.	53.4	56.4	59.8	51.5	55.9	56.4	57.6	52.3	52.9	54.4	51.5
Gas per indicated horse power per hour, cubic feet	25	27	23.3	24.6	26	23.4	24.5	24.2	26.3	26.	24.	25.6	23.6	23 5	24.2	24.5	24 6	24.7	23.9	24.8
Gas per indicated horse power per hour, liters	708	765	660	697	736	663	694	685	745	735	680	725	668	665	685	694	697	700	677	703
Gas per effective horse power per hour, cubic feet	—	—	46.1	38.4	37.4	38	34.4	33	32.7	30	29.5	31.1	29.5	29.4	30.	29.1	29	30	30.1	33.4
Gas per effective horse power per hour, liters	—	—	1310	1090	1060	1080	970	940	930	850	835	892	835	832	850	825	822	850	853	945

* In tests 18 and 19 the amounts of heat recorded as transferred to the water jacket do not fairly represent the heat lost by the hot cylinder gases, for the following reason:—In test 18 but little water was used, the cylinder was becoming hotter, and thus part of the heat was consumed in heating the metal of the cylinder. In test 19, which followed 18 with only five minutes interval, more water was used, and the heat given out by the cooling of the previously heated cylinder is added to that derived directly from the hot gases. If the figures 23 horse power for test 18, and 28 horse power for 19, were substituted for those given in the table, they would probably be much closer to the results of a prolonged test.

ally obtained for the "Otto" engine. This is due chiefly to the poor quality of the gas used. The calculation given further on shows that only 5,495 calories can be obtained from the combustion of 1 cu. meter. For average quality illuminating gas, 6,000 calories is usually taken as a fair figure. Consequently, with $8\frac{1}{2}$ per cent. less gas of average quality an equal amount of heat would be obtained.

An allowance should also be made for the temperature of the gas. In these experiments the gas had a temperature of about 24°C. Now, in winter the temperature of the gas would be reduced nearly, or perhaps quite, to the freezing point as it passed through the meter. From the law that the volume of a perfect gas varies directly as its absolute temperature, pressure remaining constant, it is found that a unit of volume at 24° becomes $\frac{273}{273+24}=0.92$ volume at 0°C. That is, at 0°, 0.92 vol. has the same weight, and consequently the same heating power that 1 vol. has at 24°. It will be readily admitted that 5 per cent., or more, difference in the volume of gas consumed might easily result from the time of year at which the experiment was made.

The following table shows the effect of making these allowances. The first column gives the average of all the tests at full power, being the actual consumption of the gas used. The other columns give the calculated equivalent values. The temperature 0°C was taken as a convenient standard:

GAS CONSUMPTION (hourly).

ENGINE EXERTING ITS MAXIMUM POWER.

		Average with Hoboken gas.	This amount reduced to 0°C.	Equivalent quantity of ordinary gas.	This amount reduced to 0°C.
Gas per indicated H. P.	cubic ft.	24.5	22.5	22.4	20.6
	liters.	694	637	634	583
Gas per effective H.P.	cubic ft.	30.1	27.7	27.6	25.4
	liters.	853	784	781	719

It is safe to say that 21 cubic feet (600 liters) per hour of good quality illuminating gas at ordinary temperature is sufficient to develop one horse-power with the Otto engine. In fact, this result has frequently been obtained.

The pressure in the gas mains has no appreciable effect upon the volume of the gas, as it causes a contraction of not more than $\frac{1}{300}$.

The table of results at varying power gives the results of tests made when there was not sufficient resistance to make the engine take gas every time. This is the ordinary condition of running, for it is well to have a reserve of power for the governor to call upon. The gas consumption with varying power is seen to be nearly constant per indicated horse-power; but, per actual horse-power the amount of gas used necessarily increases, since the friction becomes greater in proportion to the useful work. These results display the merit of the governor. Whenever there is an excess of speed the supply of gas is cut off until the speed diminishes. In this way the governor insures the greatest possible economy when the engine is exerting less than its maximum power.

5. RATIO OF AIR TO GAS.

The ratio of air to gas was found, by actual measurement of both, to be about seven to one, when the engine was working most economically. Although with better gas the ratio would be slightly increased, yet it could not equal that usually given for the "Otto" engine —ten to one, or thereabouts. The ratio is commonly obtained from a measurement of the gas consumption alone, the air being reckoned as the volume of the piston displacement, less the measured amount of gas. This is not an accurate method, as is shown by the indicator diagram (Fig. 4). The pressure in the cylinder is sensibly below the atmospheric at the end of the out stroke, and it is manifestly unfair to compare volumes when the pressures are different. If, however, the volume represented on the indicator card between the points 1 and 3, (at which points the gases enclosed in the cylinder are at atmospheric press-

ure) be used in calculation, instead of the entire piston displacement, a much closer result will be obtained. The ratio determined by means of two meters is, however, much more satisfactory than a calculated result.

When the proportion of air is increased by partly closing the gas valve, the card obtained is like B or C, Fig. 6; the explosion line is much more inclined, the mean effective pressure is less, consequently also the indicated horse-power. The gas consumption per indicated horse-power is not much changed, but per effective horse-power it becomes considerably greater, showing the false economy of throttling the gas supply. A comparison of tests 16 and 20, in which the conditions other than the ratio of air to gas are nearly identical, shows very plainly the disadvantage of using too little gas.

6. Temperature of Water Jacket.

From the tables it appears that the amount of heat carried off by the water jacket is about half of the total heat of combustion of the gas burned. When the cylinder is kept cool by a plentiful supply of water, the quantity of heat carried away appears to be greater than when less water is used, and the cylinder allowed to become warmer.

From this, one is led to expect a greater per-centage of useful work with less water. But a careful comparison of the results fails to show that any marked difference in either the indicated or the actual work is caused by varying the temperature of the water jacket.

It is probable that much more heat is lost by direct radiation when the cylinder is warm than when it is cool, and that this accounts in part, at least, for the apparent difference in the quantity of heat carried away by the water jacket in the two cases.

7. Thermal Constants for the Gas used.

Analysis of Gas.

The analysis of the gas used in the tests, as determined by Thomas B. Stillman, Ph. D. of the Stevens Institute of Technology, is given on next column:

		By volume.
H	Hydrogen.......	.395
CH_4	Marsh gas......	.373
N	Nitrogen.......	.082
C_2H_6, &c.	Heavy hydrocarbons.........	.066
CO	Carbonic oxide..	.043
O	Oxygen........	.014
H_2O_1, CO_2, H_2S, &c.	Water-vapor, impurities, &c...	.027
		1.000

By weight its composition is found to be:

	Cu. meters.		Densities*		Kilos. per cu. m.	W't p. unit.
H	.395	×	.087	=	.035	.058
CH_4	.373	×	.694	=	.258	.426
N	.082	×	1.215	=	.099	.163
C_3H_6, &c.	.066	×	1.84	=	.121	.200
CO	.043	×	1.215	=	.052	.086
O	.014	×	1.388	=	.019	.031
H_2O_1, &c.	.027	×	~.8	=	.022	.036
	1.000	×	.606	=	.606	1.000

By "density" is meant the weight of one cubic meter in kilogrammes. As will be seen from the above, one cubic meter of the gas in question weighs 0.606 kilos.

8. Heating Power of the Gas.

Upon complete combustion the gas develops heat per cubic meter, as follows:

	Calories.†				Calories.
from H	29060	×	.035	=	1020
" CH_4	11710	×	.258	=	3020
" C_3H_6,&c.	11000	×	.121	=	1330
" CO	2400	×	.052	=	125
				per cu. m.	5495c

and per kilog. gas $\frac{5495}{.606}$ = 9070 calories.

Expressed in British measures, one cubic foot of gas develops 617.5 heat units.

9. Air necessary for complete Combustion and the Products of Combustion.

In order to determine the amount of air to be supplied for complete combustion, it is necessary to ascertain the quantity of oxygen that is taken into chemical com-

* Schöttler: Die Gasmaschine, p. 77.
† Same, p. 80.

bination by the several combustible constituents of the gas.

$$
\begin{array}{lrl}
 & 2H+O & =H_2O \\
\text{by volume} & 2+1 & =2 \\
\text{by weight} & 2+16 & =18 \\
 & CH_4+4O & =CO_2+2H_2O \\
\text{by volume} & 2+\ 4 & =2\ \ +4 \\
\text{by weight} & 16+64 & =44\ \ +36 \\
 & C_3H_6+9O & =3CO_2+3H_2O \\
\text{by volume} & 2+9 & =6\ \ +6 \\
\text{by weight} & 42+144 & =132+54 \\
 & CO+O & =CO_2 \\
\text{by volume} & 2+1 & =2 \\
\text{by weight} & 28+16 & =44
\end{array}
$$

The combining proportions per unit of the several constituents is:

By volume—

$$
\begin{array}{l}
1H\ \ +\ \tfrac{1}{2}O=1H_2O \\
1CH_4\ +\ 2O=1CO_2+2H_2O \\
1C_3H_6+4\tfrac{1}{2}O=3CO_2+3H_2O \\
1CO\ \ +\ \tfrac{1}{2}O=1CO_2
\end{array}
$$

By weight—

$$
\begin{array}{l}
1H\ \ +\ 8O=\qquad 9H_2O \\
1CH_4\ +\ 4O=\tfrac{11}{4}CO_2+\tfrac{9}{4}H_2O \\
1C_3H_6+\tfrac{24}{7}O=\tfrac{22}{7}CO_2+\tfrac{9}{7}H_2O \\
1CO\ \ +\ \tfrac{4}{7}O=\tfrac{11}{7}CO_2
\end{array}
$$

The volume of oxygen required for the combustion of 1 volume of gas is:

$$
\begin{array}{lr}
H\ \ .395\times\ \tfrac{1}{2}= & .197 \\
CH_4\ .373\times\ 2= & .746 \\
C_3H_6\ .066\times 4\tfrac{1}{2}= & .297 \\
CO\ \ .043\times\ \tfrac{1}{2}= & .022 \\
\hline
 & 1.262 \\
\text{less O in gas .014 } \ldots\ldots & .014 \\
\hline
 & 1.248
\end{array}
$$

Taking oxygen as 21 per cent. in atmospheric air the volume of air required is

$$\frac{1.248}{.21}=5.94 \text{ per volume gas.}$$

Since air weighs 1.251 kilos. per cu. meter the ratio by weight is

$$\frac{5.94\times1.251}{1\times.606}=12.26 \text{ air to gas 1.}$$

From the combustion of 1 unit weight of gas with 12.26 air there results 13.26 units weight of a mixture the composition of which will be:

$$
\begin{array}{lll}
CO_2 & \left\{\begin{array}{l} (CH_4)\ .426\times\tfrac{11}{4}=1.171 \\ (C_3H_6).200\times\tfrac{22}{7}=\ .629 \\ (CO)\ \ .086\times\tfrac{11}{7}=\ .135 \end{array}\right\} & 1.93 \\
H_2O & \left\{\begin{array}{l} (H)\ \ \ .058\times\ 9=\ .522 \\ (CH_4).426\times\ \tfrac{9}{4}=\ .958 \\ (C_3H_6).200\times\ \tfrac{9}{7}=\ .257 \end{array}\right\} & 1.74 \\
N & \left\{\begin{array}{l} \text{from the air.....}9.407 \\ \text{in gas itself......}\ .163 \end{array}\right\} & 9.57 \\
\text{Impurities in gas} \ldots & & 0.03 \\
\hline
 & & 13.27
\end{array}
$$

Per unit weight of mixture the composition will be:

CO_2	.146
H_2O	.131
N..........................	.721
Impurities	.002
	1.000

The volume which 13.27 kilos. of products of combustion will occupy is found from the known volumes of the constituent gases as follows:

	kilos.	cu. m. per kilo.	cu. m.
CO_2	1.93 ×	.524 =	1.011
H_2O	1.74 ×	1.28 =	2.227
N	9.57 ×	.823 =	7.876
Impurities	.03 ×	~.9 =	.027
			11.141

The products of combustion then occupy 11.141 cu. m. to every kilog. of gas. To find the ratio per cu. meter of gas we have simply to multiply by .606, the number of kilos in a cubic meter, and we get 6.751 as the result. As there is necessary 6.94 cu. m. of mixture of air and gas to every cu. m. gas, it is seen that by combustion a contraction of 2.7 per cent. takes place.

When there is an excess of air present, as is always the case in practice, the contraction becomes less in proportion, and may be considered to be about 2 per cent. In the following thermodynamic computations no account is taken of this contraction.

10. Specific Heats and their Ratio..

The specific heats of the products of combustion are determined from the specific heats of the several component gases as follows:

Specific heat at constant pressure (water=1).

$$C_p = \left\{ \begin{array}{ll} .2169 \times .146\ (CO_2) & =.0317 \\ .4805 \times .131\ (H_2O) & =.0629 \\ .2438 \times .721\ (N) & =.1758 \\ \sim .4 \times .002 (\text{impurities}) & =.0008 \end{array} \right\} \quad .2712$$

Specific heat at constant volume (water 1)

$$C_v \left\{ \begin{array}{ll} .1714 \times .146\ (CO_2) & =.0250 \\ .3694 \times .131\ (H_2O) & =.0484 \\ .1727 \times .721\ (N) & =.1245 \\ \sim .3 \times .002\ (\text{impurities}) & =.0006 \end{array} \right\} \quad .1985$$

The ratio of these specific heats is the exponent of adiabatic expansion and is found to be:

$$\gamma = \frac{C_p}{C_v} = \frac{.2712}{.1985} = 1.366.$$

$$C_v = \frac{(.1985 \times 13.26) + (.1684 \times 1.42)}{14.68} = .196$$

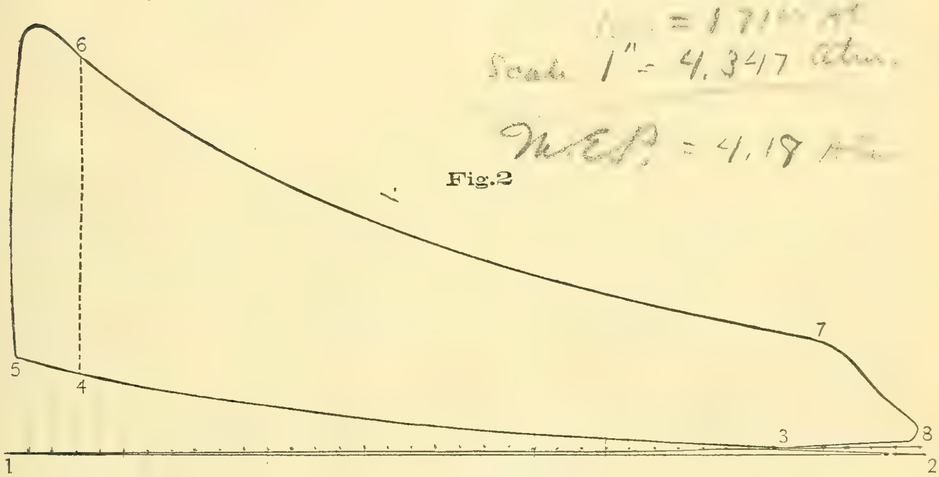
$$\gamma = \frac{C_p}{C_v} = \frac{.268}{.196} = 1.37$$

THE WORKING CYCLE.

11. THE INDICATOR DIAGRAM.

The indicator diagram (Fig. 2) is a fair sample of those taken during the tests at full power.

Beginning at the point 1, the lowest line of the diagram represents the pressure during the first forward stroke, while gas and air are entering the cylinder. This line lies at a nearly uniform distance below the atmospheric line, as is shown more clearly on the diagrams (Figs. 4 and 5) taken with a very light spring.



NORMAL INDICATOR DIAGRAM.

Since there is always an excess of air present, these values will be somewhat modified by that fact. From the meter records of test 19 the ratio of air to gas by volume was found to be 6.63 to 1; by weight the ratio is

$$\frac{6.63 \times 1.251}{1 \times .606} = 13.68.$$

Since for complete combustion only 12.26 parts of air by weight are needed, there are 1.42 parts in excess. The specific heats of air being $C_p = .2375$ and $C_v = .1684$; the effect of the excess of air will be to reduce the specific heat slightly.

$$C_p = \frac{(.2712 \times 13.26) + (.2375 \times 1.42)}{14.68} = .268$$

This line, 1, 2, shows a negative pressure of about 0.15 atmospheres. At 2 the inlet valve closes, and by the return stroke the gases are compressed into the clearance space at the back end of the cylinder. This compression is represented by the line 2, 3, 4, 5, which crosses the atmospheric line at 3, and shows a pressure of 2 atmospheres at 5.

One revolution of the engine is now completed, and the charge is ignited just as the crank is passing the center. The rapid burning of the gas liberates a large amount of heat, increasing the temperature and pressure, which reaches about 9 atmospheres as a maximum. The line, 5, 6, is called the explosion line, although the action is better described as rapid

combustion than as an explosion. The gases now expand during the second forward stroke, and exert upon the piston energy which, by means of the fly wheels, carries the engine through the remainder of the cycle. At 7 the exhaust valve opens, allowing the burned gases to escape.

The line 8, 1, shows the pressure while these gases are being expelled by the second return stroke of the piston.

When the governor prevents the admission of gas to the cylinder the cycle is somewhat modified. After compression of the air no explosion can take place since there is no combustible mixture present. The expansion line then follows closely the previous compression line, and the cycle is completed by expulsion of the air. Two revolutions are required to complete the cycle when the engine takes gas every charge, and four, six, eight, or sometimes ten revolutions may occur before the engine returns to its original state.

In Fig. 3 are given copies of two cards, one A, taken during test 1, when the engine took gas once in five times; the other, B, during test 14, when the engine took gas every time. Since in test 1 four charges of air passed through the cylinder after every explosion, the products of combustion were almost completely expelled, leaving the clearance space filled with nearly pure air at a temperature little above the atmospheric. A larger charge of gas could thus be drawn into the cylinder and still have sufficient oxygen for combustion. The result is shown by the greater area of diagram A. The mean effective pressure is not, however, proportionately greater, since the correction for work done in drawing in gases has to be applied five times to A, and only once to B.

It will be noticed, especially by the light spring diagram (Fig. 4), that the pressure between the points 9 and 10 falls below the atmospheric. This is undoubtedly due to the rapid cooling of the hot discharged gases in the exhaust vessel and pipe outside the engine. The contraction of these gases from this cooling is greater than the piston displacement during the early part of the return stroke, thus causing a partial vacuum within the cylinder. To show that this is the real cause, and that it is not due to any inaccuracy of the indicator, a diagram (Fig. 5), taken when the engine was running without taking gas every time, is inserted.

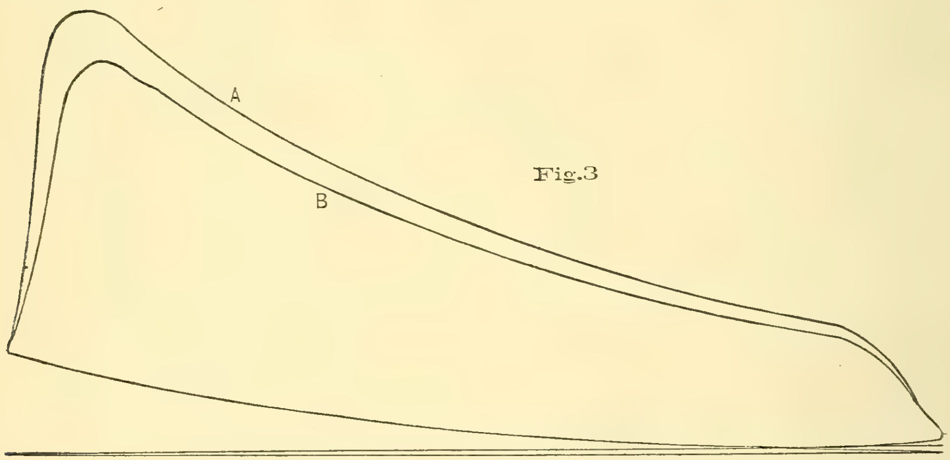


Diagram with Regular and with Intermitted Action.

After an explosion the pencil follows the line *a*, *b*, *c*, *d*, the same as 8, 9, 10, 1, on the other diagram. During the next forward stroke no gas is taken in, and the line *d*, *e*, corresponding to 1, 2, is drawn. Then follows the compression line *e*,*f*,*g*,*h*, the same as 2, 3, 4, 5. Since there is no explosion this time, the pencil returns along the wavy line *i*, *j*, *k*, *l*, nearly coincident with the compression line, At

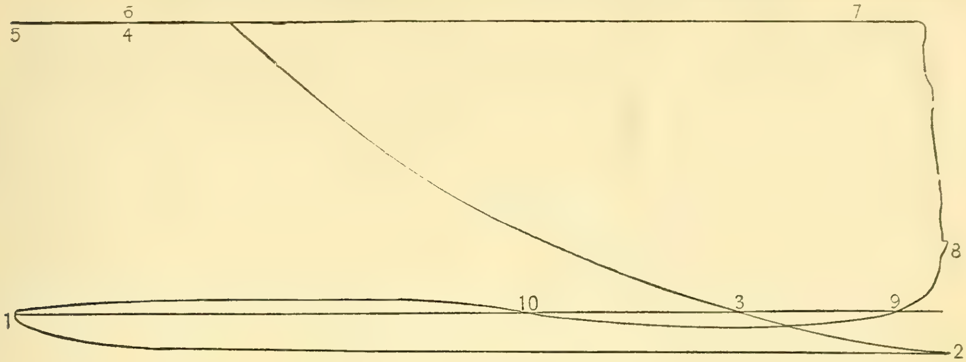
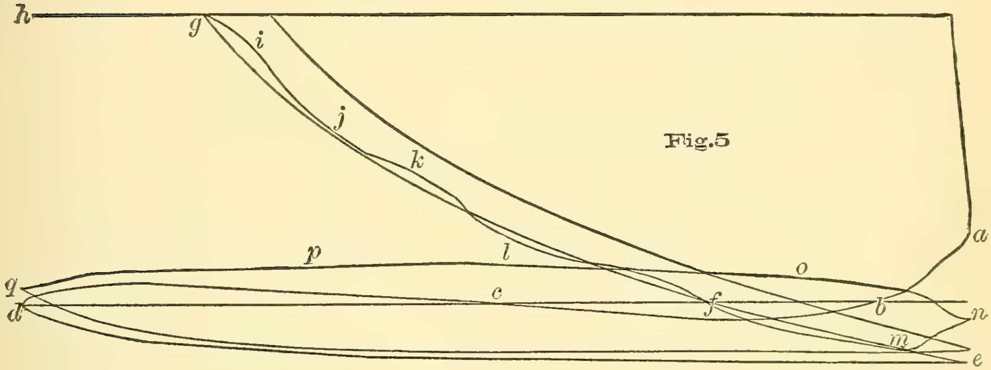
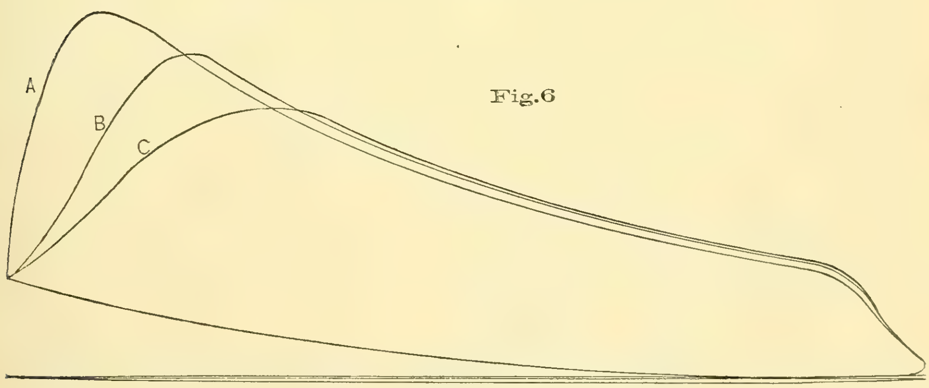


Fig. 4

DIAGRAM TAKEN WITH LIGHT SPRING.



LIGHT SPRING, INTERMITTED ACTION.



SERIES OF INDICATOR CARDS.

m the exhaust valve opens, allowing air to enter from without. The pencil does not return to *e*, but moves to *n*, nearly on the atmospheric line. Since the gases have not been heated, there can be no contraction to counteract the piston displacement, and on the return stroke the exhaust line *n*, *o*, *p*, *q*, immediately rises above the atmospheric, as would be inferred. That it keeps above the other exhaust line *b*, *c*, *d*, for the whole length of the stroke is a still further confirmation of this reasoning.

12. PRESSURES AND TEMPERATURES.

Before the indicator card can be used in thermodynamic calculations, it is necessary to determine the absolute volumetric relations represented on the diagram.

The volume of the clearance space was determined by weighing the water required to fill it. It was found to be 7.94 liters. The piston displacement is 13.015 liters. Consequently the clearance space is 38 per cent. of the total cylinder volume, and is represented on the scale of the diagram by 73 millimeters. The line of zero volume, therefore lies 73 mm. to the left of points 1 and 5 (Fig. 2).

The compression line 2, 3, 4, 5, crosses the atmospheric line at *3*, 170 mm. from the zero line, representing a volume 0.885 of the total cylinder space.

It is necessary first to determine the temperature of the contents of the cylinder at the point 3 before compression. The meters show that 1.40 liters gas and 9.25 l. air, at a temperature of 22° C, were admitted to the cylinder every charge during test 19, the one chosen for calculation. There were in the clearance space 7.94 l. products of combustion from the previous charge at a temperature of 410°, as given by the pyrometer.

Volumes at constant pressure are proportional to absolute temperature. These volumes reduced to 0° C become,

for entering gases $10.65 \times \frac{273}{296} = 9.82$ liters.

for product of combustion $7.94 \times \frac{273}{683} =$ 3.18 liters.

The weight of 1 liter of entering gases (air 6.63 vols., gas 1 vol.) at 0° is 1.16 grams. The weight of 1 liter of products of combustion (burned gases 9.6 vols., excess of air 1 vol.) at 0° is 1.19 grams.

∴ The entering gases weigh $9.82 \times 1.16 = 11.3$ grams., and the products of combustion weigh $3.18 \times 1.19 = 3.78$ grams. The temperature at constant pressure of a mixture of 11.3 grams entering gases at 296° absolute, and 3.78 grams burned gases at 683° absolute, considering the specific heats the same for the two components, will be,

$$\frac{(11.3 \times 296) + (3.78 \times 683)}{11.3 + 3.78} = 393° \text{ abs.}$$

From the thermodynamic formula $\frac{pv}{\tau} = \text{const.}$ for perfect gases, we find for the various points numbered on the card the values given below.

For the point 3,

$$p_3 = 1 \text{ at. } v_3 = 0.885 \text{ cylinder volume.}$$

$$\tau_3 = 393° \text{ abs.} \qquad T_3 = 120° \text{ C.}$$

$$\therefore \frac{p_3 v_3}{\tau_3} = 0.002252 = \delta a \text{ const.}$$

For the point 5,

$$p_5 = 3.09 \text{ at.} \qquad v_5 = 0.38 \text{ cyl. vol.}$$

$$\therefore \tau_5 = \frac{3.09 \times 0.38}{.002252} = 522° \text{ abs. } T_5 = 249°$$

For the point 6,

$$p_6 = 10 \text{ at.} \qquad v_6 = 0.44 \text{ cyl. vol.}$$

$$\therefore \tau_6 = \frac{10 \times 0.44}{.002252} = 1930° \text{ abs. } T_6 = 1657°$$

For the point 7,

$$p_7 = 3.5 \text{ at.} \qquad v_7 = 0.92 \text{ cyl. vol.}$$

$$\therefore \tau_7 = \frac{3.5 \times 0.92}{.002252} = 1432° \text{ abs. } T_7 = 1159°$$

Upon the opening of the exhaust valve at point 7 the gases begin to escape from the cylinder, and the absolute volumes are no longer shown by the indicator diagram. If, however, the expansion from 7 be considered adiabatic, values can be found for the point where the pressure becomes atmospheric. Designate such point as 9′ since it corresponds to the point 9 on the diagram. The exponent of adiabatic expansion γ, has been already determined.

$$pv^\gamma = \text{const.}$$

$$p_9' v_9'^\gamma = p_7 v_7^\gamma$$

$$p_9' = 1 \text{ at. } p_7 = 3.5 \text{ at.}$$

$$v_7 = 0.92 \text{ cyl.}$$

$$\therefore \quad v_9' = p_7^{\frac{1}{\gamma}} v_7 = 3.5^{0.73} \times 0.92 = 2.33 \text{ cyl.}$$

$$\therefore \tau_9 = \frac{1 \times 2.33}{.002252} = 1035° \text{ abs. } T_9' = 762°$$

For the case in which the engine does not take gas every time the values for pressure and temperature are quite different. This case is represented by diagram A. (Fig. 3). Taking the temperature of the charge before compres-

sion at 300° absolute, instead of 393° as found above, the maximum temperature reached is 1727° abs., instead 1930, although the pressure in A is one-half an atmosphere higher at the maximum.

13. Equations of Lines.

Since the compression and expansion lines are nearly adiabatic, the general formula, pv^x = constant, gives very accurate equations for those lines when the values of x are determined.

For the expansion curve x, is determined as follows,

$$p_6 v_6^x = p_7 v_7^x.$$

Using *Naperian* logarithms,

$$\log p_6 + x. \log v_6 = \log p_7 + x. \log v_7$$

$$x = \frac{\log p_6 - \log p_7}{\log v_7 - \log v_6}$$

$$x = \frac{\log 10 - \log 3.5}{\log 0.92 - \log 0.44} = 1.363$$

For the compression line,

$$x = \frac{\log p_5 - \log p_3}{\log v_3 - \log v_5} = \frac{\log 3.09 - \log 1}{\log 0.885 - \log 0.38} = 1.335$$

14. Indicated Heat.

"The mechanical equivalent of the heat absorbed or given out by a substance in passing from one given state, as to pressure and volume, to another given state, through a series of states represented by the coördinates of a curve on a diagram of energy, is represented by the area included between the given curve, and two curves of no transmission of heat drawn through its extremities, and indefinitely prolonged in the direction of increase of volume."*

The above theorem furnishes a method for finding the amount of heat received or rejected along the several lines of the diagram.

The heat received along the explosion line 5, 6, is the thermal equivalent of the area between the indefinitely extended adiabatics from 5 and 6. This area is, for convenience, divided by the vertical line 6, 4; the area 4, 5, 6 was measured on the card, and found to be 4.2 sq. centimeters.

Let A = the area under the adiabatic from point 6, indefinitely extended.

Let B = the area under the adiabatic from point 4, indefinitely extended.

Then the A − B + (area 4, 5, 6) will be the area sought.

The equation of the adiabatic from 6 is,

$$p_6 v_6^\gamma = pv^\gamma = \text{const.} \quad \therefore \quad p = p_6 v_6^\gamma \, v^{-\gamma}$$

$$A = \int p dv = \int_{v_6}^{v_\infty} p_6 v_6^\gamma \, v^{-\gamma} dv$$

$$= \frac{1}{1-\gamma} p_6 v_9^\gamma \left[v^{1-\gamma} \right]_{v_6}^{v_\infty} = \frac{1}{1-\gamma} p_6 v_6^\gamma \left(v_\infty^{1-\gamma} - v_6^{1-\gamma} \right)$$

$v_\infty^{1-\gamma} = 0$, since $1-\gamma$ is negative, hence,

$$A = \frac{1}{\gamma-1} p_6 v_6^\gamma v_6^1 \quad = \frac{1}{\gamma-1} p_6 v_6.$$

In the same way it can be shown that

$$B = \frac{1}{\gamma-1} p_4 v_4.$$

$$\therefore \; A - B = \frac{1}{\gamma-1}(p_6 v_6 - p_4 v_4),$$

and since $v_6 = v_4$,

$A - B = \frac{1}{\gamma-1}(p_6 - p_4)\, v_6 = 96.5$ sq. centimeters, taking γ as 1.37.

A − B + area 4, 5, 6 = 96.5 + 4.2 = 100.7 sq. cm.

Reduced to calories per charge, this becomes 4.67 calories.

With our present knowledge of the behavior of the permanent gases, it is not certain that the specific heats of these gases remain constant at the high temperatures attained in the gas engine. If, however, we consider their ratio constant, a comparison of γ with the exponent x for the expansion line 6, 7, shows that this line is almost exactly adiabatic, and that there is no appreciable transmission of heat along it.

The compression line 2, 3, 4, 5 is also nearly adiabatic, but a calculation similar to that given for the explosion line shows a rejection of 0.05 calories.

The total amount of heat received during that part of the cycle represented by the lines 3, 4, 5, 5, 6, and 6, 7 is 4.67 + 0 − 0.05 = 4.62 calories per charge.

This indicated heat disappears in two ways:

* Rankine's "Steam Engine," p. 303.

1. As indicated work.

2. As heat wasted after the exhaust valve opens.

1. The indicated work measured by a planimeter and reduced to calories gives 1.33.

2. When the exhaust valve begins to open at 7, $\tau_7=1432°$. If the contents of the cylinder be supposed to expand adiabatically, the absolute temperature at atmospheric pressure becomes 1035°. The temperature obtained from the pyrometer in the exhaust pipe was 683° abs. This difference in temperature shows that heat is lost by the gases cooling in the cylinder and exhaust pipe, and shows that the expansion beyond point 7 was not adiabatic.

The amount of heat included in (2) may, for convenience of calculation, be separated into three parts:

a. Work of adiabatic expansion of the cylinder contents to atmospheric pressure.

b. Heat given out by the cylinder contents cooling from 1035° to 683° abs. at constant pressure.

c. Heat given out by that part of the cylinder contents representing one charge in cooling still further from 683° to 296°.

a is found to be equivalent to 0.58 calories.

b. The contents of the cylinder weigh 11.3 grams for the charge of gas and air, and 3.78 grams for the gases remaining in the clearance space, making 15.08 grams in all. The specific heat of this mixture at constant pressure may be taken at .27

$$.01508 \times .27 \times (1035-683)=1.44 \text{ calories.}$$

$$c.\ .0113 \times .27 \times (683-296)=1.18 \text{ calories.}$$

Adding *a*, *b*, and *c*, we find for (2) $.58+1.44+1.18=3.2$ calories.

Adding (1) and (2) we find,

$1.33+3.2=4.53$ calories accounted for out of 4.62 calories indicated as heat received, as near an agreement as could be expected.

15. Discrepancy Explained—dissociation.

The total heat of combustion of one charge, .0014 cu. meters of the gas used, is 7.69 cal. The difference between this and the indicated heat, 4.62 cal., is 3.07 cal., or 40 per cent. of the total heat which is left unaccounted for by the indicator card.

As we find from the tests that about half of the total heat of the gas goes into the water jacket, a considerable quantity of heat must be so given out during that part of the cycle represented by the line 6, 7, where the temperature of the gases in the cylinder is high. Since this line is very nearly adiabatic, an amount of heat equal to that absorbed by the water jacket must be received from some source, necessarily from the gas itself.

It is a well established fact that heat decomposes chemical compounds. Carbonic acid is thus separated into carbonic oxide and oxygen at a comparatively low temperature. At the temperature attained after an explosion in the gas engine, it is almost certain that this action, known as *dissociation*, occurs and prevents the complete combustion of the gas taking place instantly. As the temperature gradually decreases from expansion, further burning ensues. It is quite possible that the gas is not fully burned until after the opening of the exhaust valve.

16. Efficiency.

As the cycle in which a gas engine works does not even approximate to the theoretical cycle of Carnot, it seems futile to apply his formula for efficiency.

The indicated work represents 18 per cent. of the total heat of combustion of the gas. The actual useful work is 14½ per cent.

An efficiency sometimes given for the gas engine, derived from the indicator diagram alone, is the indicated work divided by the indicated heat, giving about 30 per cent. This is manifestly incorrect, since the indicated heat is not the whole heat of the gas.

The best steam engines utilize only 10 per cent. of the total heat of combustion of the coal, and small engines rarely exceed 5 per cent., so that the gas engine is by far the more perfect heat engine.

17. Disposition of the Heat.

Heat is disposed of in a gas engine in these ways:

(1). As indicated work, including useful work and friction.
(2). In the hot expelled gases.
(3). In the water jacket.
(4). In radiation, etc.

Taking the figures directly from test 19, but making allowance for probable error in the figure for water jacket, it is found that

(1)=1.33	calories or	17 per cent.
(2)=1.18	"	$15\frac{1}{2}$ " "
(3)=4.00	"	52 " "
(4)=1.18	"	$15\frac{1}{2}$ " "

the sum 7.69 calories, being the total heat of combustion of the gas.

Commercial Efficiency of the Gas Engine.

18. Relative Economy of Gas, Steam and Hot-Air Engines.

In making a comparison of this kind it is necessary to consider,

(1). The cost of gas or coal consumed.
(2). The cost of water used.
(3). Lubrication.
(4). The cost of attendance.
(5). Depreciation and repairs.
(6). Interest on capital invested.

(1). The average consumption of gas in a gas engine per effective horse-power per hour, including igniting flames, is about 30 cubic feet.

The consumption of coal per effective horse-power per hour by small steam engines is about 7 lbs.

(2). The water used in the water jacket of a gas engine will not enter into the estimate, since by the use of tanks the same water may be used continuously.

The water supplied to the boiler of the steam engine here considered amounts to $\frac{5}{6}$ cu. ft per horse power per hour.

(4). A gas engine requires little or no attendance. A man can accomplish $\frac{5}{6}$ of a day's work and still take full charge.

Steam engines of this size require from $\frac{1}{2}$ to 1 day's attention, depending upon the proximity of the engine and boiler.

(5). As regards depreciation, it is safe to say that gas and steam engines have about equal terms of life; for, while gas engines have less complication of working parts than steam engines, yet they are subject to more severe and abrupt strains.

(6). The interest will necessarily be directly proportional to the amount of capital invested.

The following summary shows the relative cost of a day's running:

Gas Engine, 8 H. P. actual, 10 hours.

(1) 2,400 cu. ft. gas @ $2.50 per 1,000...	$6.00
(2) Water..........	0.00
(3) Lubrication..........	0.20
(4) $\frac{1}{6}$ day's labor @ $2.00..........	0.33
(5) Depreciation, &c., at 12% per year $\frac{12}{360}$% on $1,075..........	0.36
(6) Interest at 5% per year $\frac{5}{360}$% on $1,075	0.15
Daily expense..........	$7.04

Steam Engine, 8 H. P. actual, 10 hours.

(1) Coal $\frac{560}{2240}$ tons @ $5.00..........	$1.25
(2) Feed water 65 cu. ft. @ $1.25 per 1,000..........	0.08
(3) Lubrication..........	0.15
(4) $\frac{1}{2}$ day's labor at $2.00..........	1.00
(5) Depreciation, &c., at 12% per year $\frac{12}{360}$% on $800..........	0.27
(6) Interest at 5% per year $\frac{5}{360}$% on $800	0.11
Daily expense..........	$2.86

The following figures have been obtained for the expense of running a small hot-air engine. This engine has been running in a printing office in New York City for 18 years. It uses $4\frac{1}{2}$ lbs. of coal per H. P. per hour; every third year relining costs $100.

Hot-Air Engine, $2\frac{1}{2}$ H.-P. actual, 10 hours.

(1) Coal $\frac{115}{2240}$ tons @ $5..........	$0.25
(2) Water..........	0.00
(3) Lubrication..........	0.10
(4) Attendance same as for gas engine..	0.33
(5) Depreciation &c., @ 10% per year $\frac{10}{360}$% on $750..........	0.21
(6) Interest @ 5% per year $\frac{5}{360}$% on $750.	0.10
Daily expense..........	$0.99

The cost of one horse-power per hour is,

with gas engine......$8\frac{3}{4}$ cents.
with steam engine...$3\frac{1}{2}$ "
with hot-air engine..4 "

For intermittent work the gas engine is much more economical than the above figures indicate; and this fact, together with its safety, cleanliness and convenience, makes the gas engine very desirable where small powers are required.

19. Cheaper Gas.

With cheaper kinds of gas it becomes possible to reduce the figures for gas engines as low if not lower than those obtained for steam engines. In England and Germany, where the cost of illuminating gas varies from $0.50 to $0.75 per 1,000 cu. ft., the above figures would be reduced 60 per cent., making the daily expense of the gas engine about $2.75.

It should be rememberedt hat *illuminating* gas is not required for the gas engine. The manufacture and distribution in cities of some cheap gas especially adapted for use in gas engines may soon become a prominent industry; and, with economy added to its other merits, the gas engine may largely supplant steam for manufacturing and other purposes.

At the British Associatlon meeting a paper by Prof. J. A. Ewing was read, on the magnetic susceptibility and retentiveness of iron and steel. This paper was a preliminary notice of some results of an extended investigation which the author had been conducting for three years in Japan. Experiments with annealed rods and rings of soft iron wire showed that that material possesses the property of retentiveness in a very high degree. As much as 90 and even 93 per cent. of the induced magnetism survived the removal of the magnetizing force. The extraordinary spectacle was presented of pieces of soft iron entirely free from magnetic influence, nevertheless holding an amount of magnetism, per unit of volume, greatly exceeding what is ever held by permanent magnets of the best tempered steel. The magnetic character of the iron in this condition was, however, highly unstable. The application of a reverse magnetizing force quickly caused demagnetization, and the slightest mechanical disturbance had a similar effect. Gentle tapping removed the residual magnetism completely. Variations of temperature reduced it greatly, and so did any application of stress. On the other hand, the magnetism disappeared only very slowly, if at all, with the mere lapse of time. The residual magnetism in hardened iron and steel was much less than in soft annealed iron. The maximum ratio of intensity of magnetism to magnetizing force during the magnetization of soft iron was generally 200 or 300, and could be raised to the enormous figure of 1590 by tapping the iron while the magnetizing force was being gradually applied. A number of absolute measurements were made of the energy expended in carrying iron and steel through cyclic changes of magnetization; and the effects of stress on magnetic susceptibility and on existing magnetism were examined at great length. The whole subject was much complicated by the presence of the action which, in previous papers, the writer had named *Hysteresis*, the study of which, in reference both to magnetism and to thermo-electric quality, had formed a large part of his work.

An Asphalte Mortar.—The *Centralblatt der Bauverwaltung* describes a patented composition made at a factory in Stargard, Pomerania, which has for some years past been used with perfect success on the Berlin-Stettin railway for wall copings, water-tables, and similar purposes requiring a water-proof coating. The material is composed of coal tar, to which are added clay, asphalte, resin, litharge and sand. It is, in short, a kind of artificial asphalte, with the distinction that it is applied cold, like ordinary cement rendering. The tenacity of the material when properly laid, and its freedom from liability to damage by the weather, are proved by reference to an example in the coping of a retaining wall which has been exposed for four years to the drainage of a slope 33ft. high. This coping is still perfectly sound and has not required any repair since it was laid down. Other works have proved equally satisfactory. In applying this mortar, as it is termed, the space to be covered is first thoroughly dried, and after being well cleaned is primed with hot roofing varnish, the basis of which is also tar. The mortar is then laid on cold to thickness of about $\frac{3}{8}$ inch, with either wood or steel trowels, and is properly smoothed over. If the area covered is large, another coating of varnish is applied, and rough sand strewn over the whole. The water-proof surface thus made is perfectly impregnable to rain or frost, and practically indestructible. The cost of the material laid is estimated at 5d. per sq. ft.; and this price can be reduced at least 1d. for large quantities put down by experienced workmen.

COVERED SERVICE-RESERVOIRS.

By WILLIAM MORRIS, M. Inst. C. E.

From Proceedings of the Institution of Civil Engineers.

The author, in treating of this subject, has confined himself to those reservoirs of which he has some practical knowledge. For this reason he has not attempted a description of those ancient reservoirs, of which Puteoli and Constantinople are examples, which prove that the Roman and other ancient nations, who lived in warm climates, long ago appreciated the importance of keeping water pure and cool for potable purposes, and that although the use of covered reservoirs in connection with the waterworks of the present day is of somewhat recent date, it is by no means a modern refinement.

Another example of covered reservoirs, on a small scale, consists in underground tanks, for the storage of rain-water. These are largely used for keeping such water sweet and wholesome; indeed, the cities of Venice and Cadiz are almost entirely dependent on underground cisterns for their supply of water, and as there is practically no smoke in these cities, if the roofs, pipes and tanks are kept clean, the water is palatable.

Covered Service-reservoirs, as technically understood, are intended for the distribution rather than for the storage of water; but if attention be confined to the description of Covered Service-Reservoirs in connection with modern waterworks, the subject is still a wide one, and the author must apologise for his inadequate treatment of it.

Without attempting to trace the introduction of covered reservoirs into this country, it does not appear, from official returns, that any were used by the London Water Companies till after the year 1850; at which time filtration had been partially introduced. With the adoption of filtration, however, it soon became evident that covered reservoirs were necessary for the storage of filtered water, and accordingly the Metropolis Water Act, 1852, which required all water (except water pumped from wells) to be filtered, specially enacted that all reservoirs for filtered water within 5 miles of St. Paul's should be covered. It would appear that this enactment was more particularly intended to preserve the water from contact with the smoky and polluted atmosphere of London; but the objection to uncovered service reservoirs is by no means confined to those exposed to the smoky atmosphere of large towns, at least, such is the author's experience in the case of water from the Chalk. whether pumped directly from wells or from rivers, such as the Thames and its tributaries which are fed from chalk springs.

When comparatively shallow open reservoirs are filled with such water, vegetable and animal life is quickly developed, especially in summer; the lower forms of vegetation first make their appearance, consisting of myriads of zoospores of algæ. The following note relates to the effect of this growth on the color of water from the River Ravensbourne, from which the Kent Waterworks formerly took their supply. "August 11th, 1851. This day a remarkable change took place in the water in the old filter bed, it became of a greenish milk color; when mixed with alum, it precipitated a large quantity of white substance, which, being examined with the microscope, was found to contain zoospores, pulpy matter and crystals of alum. Boiling had no effect on the color. The next day the water in the filter turned green."

The zoospores subsequently develop into green blanket weed, which is apt to form stoppages in the house service pipes, if it should find its way into the mains; with such forms of vegetation are usually associated water fleas, cyclops, and other animalcules.

The following extract from a report of the author's father, the late Mr. W. R. Morris, M. Inst. C. E., bears on this question:

"The Service-Reservoirs of the Kent Company being without the limits of the Act of Parliament of last Session the

covering is not compulsory, but is not the less necessary. In the summer months the improvement from filtration is lost in the present uncovered reservoirs; just so much as the company's filters have raised the water in the public estimation, equally so will be the dissatisfaction of the tenants, particularly when wholly supplied from these reservoirs."

The reservoirs referred to in this report were the Greenwich Park and Woolwich Common reservoirs, constructed under an agreement between the Admiralty and Kent Waterworks Company, by which it was agreed that these two reservoirs should be constructed at the cost of the Admiralty, the former for the protection in case of fire of Greenwich Hospital and the Dockyard and Victualling Yard at Deptford, and the latter for the protection of the Dockyard and Marine Barracks at Woolwich, these establishments being connected with the reservoirs by special fire mains; under this agreement the Company are bound to maintain a given head of water in the reservoirs, but subject to this condition are at liberty to use them for the supply of their district.

These reservoirs, which contain 1,125,000 gallons and 1,750,000 gallons respectively, were uncovered service-reservoirs, only intended for the purpose of storing water for use in case of fire or accident to the pumping machinery; they hold sufficient water to keep the mains charged at night when the pumping engines are not working, they regulate the pressure in the mains and maintain it practically uniform. Thus, when the draught of water from the mains is in excess of the quantity pumped the reservoirs supply the deficiency; and, on the other hand, when the quantity of water pumped exceeds the draught the excess flows into the reservoirs.

Before these reservoirs were constructed there was no pressure in the mains by night, and it was necessary to start the pumping engines in case of fire, which involved considerable delay.

In some waterworks a special pumping main is laid down from the engines to reservoir, and the whole of the water supplied is pumped into the reservoir before it is distributed; this is not the case at the Kent Waterworks; the mains are all connected with the reservoirs, but a very small portion of the water supplied actually passes into them.

On account of the small quantity of water taken from it, the covering of the reservoir in Greenwich Park was postponed from 1853 till 1871; it was, however, determined to cover the reservoir on Woolwich Common, but, on tenders being invited, the cost was found to be greater than had been anticipated; the project was abandoned for the time, and the bottom of the reservoir was converted into a filter, which arrangement continued for eighteen years, till the draught of water from the reservoir increased beyond its power of filtration; the reservoir was ultimately handed over to the War Office, who granted the Company another piece of land on Woolwich Common for the construction of a covered reservoir.

The first covered reservoirs used by the Kent Waterworks were two, which had been built for the Woolwich, Plumstead and Charlton Pure Water Consumer's Company, from the designs and under the direction of Mr. S. C. Homersham, M. Inst. C. E.

In the reservoir excavated on Plumstead Common the water line is below the original surface of the ground. The structure is of concrete faced with brickwork in cement. The side walls are 2 feet 6 inches thick at the top and 4 feet thick at the bottom; the back of the wall is vertical, while the inside face is battered to a slope of 1 in 8. The reservoir is divided into two compartments by a center wall 5 feet wide at the top, and 7 feet at the base; the length of each compartment at the water line is 100 feet, the width of the one being 52 feet and of the other 50 feet. The floor of the reservoir is also of concrete about 18 inches thick, paved with brick: the corners of the reservoir are curved to a radius of 3 feet 6 inches; the covering arches, thirteen in number, are built with two rings of brickwork, having a span of 7 feet 2 inches and a rise of 1 foot; they spring from cast-iron girders which rest in the center on a wall 18 inches thick with arched openings built to a height of 6 feet above the division wall, and they slope down to and rest on the exterior walls; they are also supported by two cast-iron columns which rest on brick

piers built in the floor. The arches are covered with 9 inches of puddle, to carry off the rain water and prevent it soaking into the reservoir, and finally with gravelly earth laid to a regular slope and covered with grass.

The center wall is paved with York stone about 6 inches above the water line, and forms a gangway through the reservoir to which access is obtained by an entrance door and a flight of steps at the south end; the water enters through an inlet pipe in each compartment 16 inches in diameter, with sluice cocks inside the reservoir, over which York landings are fixed to form platforms for working the same. There are overflow pipes which discharge into a dry well sunk in the gravel. The water-level was indicated by floats, the gauge-rods connected with which are enclosed by brick columns; they are not at present in use, owing to the columns having settled out of the perpendicular.

The reservoir is built on the pebble bed of the Lower Eocene formation, consisting of small rounded pebbles and fine sand. It is perfectly watertight, and contains 650,000 gallons; when full the level of the water is 170 feet above ordnance datum. The cost of this reservoir, which was built in 1854, was £3,442, including boundary wall, &c.

The reservoir on Constitution Hill, Shooter's Hill, was built at the same time as that on Plumstead Common; it was never used by the Plumstead Company, but remained empty for some years till it was purchased by the Kent Waterworks. It is coffin-shaped, the full dimensions being 120 feet by 35 feet, the depth of water is 13 feet. The top-water level is 320 feet above ordnance datum. The roof is similar to that of the Plumstead reservoir, and consists of brick arches springing from fourteen cast-iron girders, supported in the center by cast-iron columns. The side walls are of brick, built with a batter, and stiffened by counterforts; the base is 2 feet 3 inches thick, which is reduced in steps to 1 foot 10½ inches, 1 foot 6 inches and 9 inches. The footings of the wall rest on concrete 18 inches thick. The floor consists of layers of tiles placed on 1 foot of concrete. The whole of the interior surface is rendered in Portland cement.

The reservoir is built on ground which slopes rapidly from south-east to north-west, so that whilst the south end of the embankment is level with the original surface, the north end is 20 feet above it; the slope of the embankment, 1½ to 1, is rather too steep for the soil, which is London clay, and it has slipped in places. For this reason it has not been considered desirable to retain a greater depth of water than 8 feet, to which height the overflow pipe has been adjusted. The reservoir has a capacity of 300.000 gallons, but has never been quite watertight owing to various cracks in the floor; these have been repaired from time to time. Its total cost has been £1,948.

The first covered reservoir constructed by the Kent Company was made from the designs and under the direction of the author's father in 1866, at Chiselhurst. It is of irregular shape, being built on a small corner plot of land in Bickley Park. The reservoir is excavated in ground, which consists of pebble bed, as at Plumstead Common. It is formed with four vaults, each 16 feet wide, of the following lengths: 113 feet, 103 feet, 93 feet and 55 feet; the depth of water is 11 feet; the exterior walls are 14 inches thick, stiffened by piers 2 feet 3 inches square. The piers in the center of the reservoir are 3 feet by 18 inches, supported by longitudinal and cross inverts; arched ribs spring from the top of the piers to support the side walls. The vaulting springs from cast-iron girders resting on the piers and side walls, and is formed of two rings of brickwork at the crown, and of three rings at the haunches. The girders are tied together by wrought-iron rods, 10 feet from center to center. The floor is formed of a layer of concrete, one course of bricks laid flat, and two layers of plain tiles in cement; the whole of the floor and side walls is rendered with cement. In the course of construction some heavy rain caused part of the gravel to fall in at the end marked B, buttresses were therefore added to stiffen the end walls. The top-water line is 315 feet above ordnance datum. The reservoir contains 450,000 gallons. The work, which was executed by Messrs. Aird, cost £2,246.

The Dartford reservoir formed part of the waterworks, constructed by the Dartford Local Board of Health in 1854. The

works comprised an artesian well, pumping engine, and a complete system of pipeage, but on starting the engine some mishap occurred to the well, and the opponents of the waterworks being successful at an election of the Local Board, they determined to spend no more money on the works, and allowed their creditors to seize, sell and remove the pumping engine and boilers; these were sold for £300. The works then remained dormant till 1868, when they were purchased by the Kent Company, the Local Board having obtained parliamentary powers to sell their waterworks to the Company.

The reservoir is circular on plan, 60 feet in diameter at the bottom and 64 feet at the top, with a depth of water of 20 feet. There was neither covering nor artificial bottom to the reservoir when the Company took possession; the bottom has been paved with one course of stock bricks and two courses of plain tiles laid in cement. The roof is formed of arches of hollow gault bricks 5 inches thick, springing from nine rolled joists 12 inches deep, radiating from the center, where they are supported on a cast-iron column; the outer ends rest on the walls of the reservoir. These girders have a slope of 4 feet from the center. The covering arches at the outer end next the wall have a span of 22 feet 9 inches, and a rise of 4 feet; the crown of the arch is level, whilst the span gradually diminishes to nothing in the center; the spandrels are strengthened by brick inverts, and the whole of the upper surface of the roofing is rendered in cement mortar. The cost of the roof was about £375.

The Greenwich Park reservoir was built as an uncovered reservoir from the designs of the late Mr. Thomas Wicksteed, M. Inst. C. E., in 1845, for the protection of the Government establishments at Greenwich and Deptford in case of fire. It is circular on plan, the diameter at the bottom being 154 feet, and at the top 184 feet, with a depth of 8 feet when full; it contains 1,125,000 gallons of water. The top-level of the water is 158 feet above ordnance datum.

It is excavated in the Blackheath gravel, which consists of rounded pebbles mixed with fine sand, forming an excellent foundation; the sides and bottom were of lime concrete 12 inches thick on 9 inches of puddle. They were afterwards paved with bricks laid flat in cement, in consequence of the decayed state of the lime concrete, which was partially destroyed by the action of the weather and the wash of the water against the sides.

The covering, which was completed in 1871, consists of five concentric rings of 9-in. arches, having a span of 16 feet and a rise of 4 feet 6 inches. The outer ring springs from a concrete foundation 3 feet wide and 3 feet deep half way down the sloping bank, and carried 18 inches into the solid ground; the other arches spring from rings of rolled wrought-iron girders of I-shaped section, 12 inches deep, bent to the proper curves, and resting between joggles on cast-iron caps, supported by brick piers 1 foot 10½ inches square, 8 feet high, and about 12 feet from center to center. The foundations of the piers are carried through the bottom of the reservoir to the solid earth, and are built with two double courses of footings; the piers are further supported and connected by rings of 18-inch inverts. The inner covering arch rests on a central well-hole 8 feet in diameter, domed over; a hole 3 feet in diameter in the center gives access to the interior; a backing of puddle 1 foot thick is carried up outside the exterior arches to render them watertight. The spandrels are filled in with a layer of 1 foot of concrete and 2 feet of earth, and together with the sloping bank are covered with turf, but the crown of the arches is bare, the outside ring being formed of hollow gault bricks.

The cost of covering the reservoir was £3,153, including £464 for the ironwork. An electric gauge, supplied by the General Post Office, indicates the level of the water in the reservoir on a dial fitted in the engine house at Deptford. The rent paid by the company for this gauge is £20 per annum.

The covered reservoir at Deptford was formerly a filter bed; the dimensions at the top of the slope being 250 feet by 140 feet, and at the bottom 230 feet by 120 feet, the depth is 9 feet in the center, the bottom has a rise of 6 inches towards the sides; the bottom and sides consist of a layer of 12 inches of puddle, and of 5 inches of concrete made with 9 parts of Thames ballast to 1 part of blue

lias lime. The bottom is paved with brick laid flat in cement, and the sides with brick on edge. The filtering material consisted of a layer of rubble stone, four layers of gravel, one layer of shells, and 18 inches of Hardwich sand; the total thickness was 4 feet 3 inches. It was formerly used for filtering the water from the river Ravensbourne; but on the company abandoning this source in favor of a supply from chalk wells, it was obviously desirable that the water should be pumped into a covered reservoir, rather than on to a filter bed, and put through a process of filtration, which exposed the water to the heat of the sun, depriving it of its freshness and coolness in summer, whilst frost rendered it unnecessarily cold in winter.

As it was necessary before converting these filters into covered reservoirs to remove the filtering material, it occurred to the author's father that a great saving in expense might be effected by using the gravel and sand for a concrete covering instead of brickwork. The idea was successfully carried out by the company's workmen, aided by a couple of bricklayers for building the piers and longitudinal arched wall which supports the concrete arches. This wall is 18 inches thick, and the piers are 18 inches by 22½ inches; the distance between the piers is 12 feet, and the rise of the brick arches is 2 feet 6 inches. The concrete arches have a similar span and rise; the thickness at the crown is 10 inches, and at the haunches 20 inches. The concrete was made from the gravel and sand of the filter bed mixed in the proportion of 7 parts of ballast to 1 part of Portland cement.

There was another filter bed 160 feet long and 100 feet wide, covered at the same time in a similar manner, the same centering being used for both. The cost of covering the two reservoirs, which have a superficial area of 51,000 square feet, including the cost of centering, was £2,669. These reservoirs, which contain about 2,000,000 gallons of water, were covered in 1872. Water from the wells is pumped into them, and is then pumped into the mains for the supply of the district.

One of the reservoirs attached to the Plumstead Works, formerly used in connection with the lime-softening process, is now used as an intermediate reservoir between the lift pumps and force pumps. The covering, Bunnett's flooring, consists of two flat arches, having a rise of only 3 inches in a span of 19 feet, formed of two layers of hollow arched bricks rebated to fit into each other; the thickness of the arch is 6 inches. The arches rest in the center on a rolled joist, supported by six cast-iron columns; the outer ends of the arches are supported by angle-iron abutments in the side walls; the angle-iron abutments are each bolted to the central joists by fourteen tie-rods. The reservoir at the top is 73 feet long by 38 feet wide; the cost of the roof was £242.

The Woolwich common reservoir was built as a substitute for the old uncovered circular reservoir, constructed at the cost of the Admiralty in 1845, as it was found impossible to cover the latter without seriously deranging the supply to the district. The Government, therefore, on the application of the company, granted a site adjacent for a covered reservoir in exchange for the old reservoir. It was one of the conditions on which the site was granted that the top of the reservoir should not project above the original surface of the ground.

The foundation was excellent, the soil consisting of solid London clay. A pond occupied part of the site, but no difficulty arose in connection with it, as a drain-pipe had, in the first instance been laid at a sufficient depth to carry off rain or surface water from the excavation. The reservoir is rectangular, 200 feet long by 100 feet wide, with a depth of 12 feet of water when full. It is covered by eight arches, consisting of two rings of brickwork, having a radius of 8 feet and a rise of 2 feet, springing from seven rolled iron I girders 12 inches deep with 6-inch flanges. The skew-backs are formed of gault bricks fitted between the flanges, and the two exterior arches of three rings of brickwork, which are carried down till the springing is level with the center from which the arch is struck. They rest with three courses of footings on the side walls, which are of concrete, 3 feet thick and 5 feet 6 inches high, faced with 4½ inches of brickwork.

The wrought-iron girders rest on one hundred and five brick piers, 1 foot 10½ inches square, 12 feet 6 inches from

center to center; they are built with four courses of footings, and are also supported by a series of brick inverts, running across the reservoir, which serve as struts for the side walls; the inverts are only 18 inches wide and rest on two courses of footings. The end walls are formed of concrete, faced with $4\frac{1}{2}$ inches of brickwork in 8 bays, having a radius of 7 feet, and a batter of 3 inches in 10 feet, and are 3 feet thick; the apices of the curves consist of brick piers 3 feet square, set diagonally; the four corners of the reservoir are supported by T-shaped brick piers 5 feet 3 inches by 3 feet, with projections 2 feet $7\frac{1}{2}$ inches by 9 inches. The floor consists of 6 inches of concrete laid on 6 inches of puddle. The surface is curved to correspond with the sweep of the cross inverts, and has a fall of three inches towards the western end. The whole of the brickwork is of the best hard stocks, laid in cement mortar of 3 parts of sand to one part of Portland cement. The concrete is formed of six parts of Thames ballast to one part of Portland cement.

The reservoir is connected with the company's mains by two 18-inch pipes built in the wall of the western end; the sluice cocks are in a pit outside the reservoir; a 12-inch overflow pipe is built into the wall at high-water level. There is also a drain pipe with a 4-inch scouring cock for use in cleaning out the reservoir.

The interior is reached by two manhole doors with an iron ladder; small air shafts are fixed for ventilation, and a Richmond's float-gauge indicates the height of water.

The reservoir contains 1,500,000 gallons; it was designed by the author's father, and was executed by Messrs. J. Aird & Son, at a total cost of £5,500.

In 1874 a covered reservoir was built on Telegraph Hill, New Cross, under the author's direction. It is 200 feet long by 100 feet wide; the depth of water is 14 feet, and the top-water line is 163 feet above ordnance datum. The construction is almost identical with that of the Woolwich Common reservoir, but in this instance the wrought-iron girders were omitted, and their place supplied by brick arches springing from the piers, similar to the Farnborough reservoir.

The reservoir is not entirely below the level of the ground, the top of the embankment being from 6 feet to 9 feet above the original surface of the land. The foundation is excellent, the excavation being in the London clay, which, however, when exposed, forms a bad embankment. The banks have, therefore, a slope of $2\frac{1}{2}$ to 1. The concrete was made of 1 part of Portland cement, to 4 parts of river ballast and 2 parts of burnt ballast.

In constructing this reservoir it was found that the thrust of the longitudinal arches tended to force back the top of the end walls, which were only supported by the made earth; at the same time the wall turned on the edge of the solid ground as a center, and the toe was thrust forward into the reservoir. The movement was so slight as to be barely perceptible, and was promptly arrested by the introduction of inverts to support the toe of the wall. The reservoir holds about 1,750,000 gallons. The cost, including the boundary wall, was £5,280.

In 1877, the Kent Waterworks Company, at the invitation of the Bromley Rural Sanitary Authority, applied to Parliament for powers to supply water to the Cray Valley and to other parishes, namely, the parishes of Stone, Swanscombe, Darenth, Wilmington, Sutton at Hone, Farningham, Eynsford, Foot's Cray, North Cray, St. Paul's Cray, St. Mary's Cray, Orpington, Chelsfield, Farnborough, Keston, Hayes and West Wickham; the Company also applied for parliamentary power over a portion of the parish of Beckenham already supplied by them, in which their Shortlands pumping station is situated, it being a few yards beyond their former water limit.

The author was directed to prepare plans showing the works required for the supply of the district. These included a service-reservoir, for which he selected a site on Cowlass Hill, in the parish of Farnborough, about $\frac{1}{2}$ mile south-west of the high road to Sevenoaks and Tunbridge, having an elevation of 445 above ordnance datum, sufficiently high to command all but a few isolated points.

The plot of land is an irregular oblong, approximately 400 feet long by 220 feet wide; the levels vary from 428 feet above ordnance datum in the north-west

corner to 445 feet, the highest point towards the south-east. The geological formation consists of the Lower Eocene Beds of the Tertiary Series, which rise up from the London Basin and terminate at this point in a steep escarpment overlying the Chalk.

The present reservoir occupies about one-half the land, leaving sufficient for another reservoir should it eventually be required; it is square in plan, the length of each side being 125 feet; the depth of water is 15 feet, the highest water-level is 440 feet above ordnance datum, and it contains about 1,400,000 gallons.

One of the principal difficulties attending the construction of the reservoir was the necessity of making a proper road up to the site, which is 900 yards from the high road; the existing road over heavy clay land being little more than a cart track; while the ascent about 90 feet, is in some parts rather steep. This, and the fact that all the materials had to be carted a distance of over 2 miles from the Orpington Station on the South-Eastern Railway, added considerably to the cost of construction, which was £8,207.

Before commencing the reservoir a 14-inch pipe was laid along the line of pipe No. 1 in communication with the Shortlands pumping station, in order that water should be on the spot for the use of the contractor; and at the same time a 12-inch stoneware pipe was laid in the same track, as far as a brook which crosses the line of pipe, to act as a drain pipe whilst the work was in process, and as an overflow and emptying pipe for the reservoir. The laying of these pipes had been completed and did not form part of the contract.

The contractors, Messrs. J. Aird and Son, at once proceeded to lay down a line of tramway from the high road to the reservoir for the purpose of bringing the materials on the ground. The reservoir was staked out and the work commenced on the 6th of May, 1879. A trench was in the first instance excavated, in which the side walls of the reservoir were to be built; it was 8 feet 3 inches wide with an average depth of 11 feet 6 inches, and was closely timbered. The nature of the soil was remarkable; that in the eastern trench consisted of bright Thanet Sand, which extended for a short distance along the north and south trenches; to the west of this succeeded gravelly clay, with shelly clay and good yellow clay above, blue clay with veins of lignite below; the bottom of the trench on the western side was entirely in peat or lignite, which also returned about half way round the northern and southern trenches, the whole of the stratification being of a most irregular character. As the lignite was too thick to be dug out, and was fairly solid when not disturbed or exposed to the weather, it was determined to build the wall upon it, but to make the foundation somewhat wider than was at first intended. The whole of the bottom of the trench was therefore covered with puddle varying from 9 to 12 inches thick, made of the clay excavated from the reservoir. When tempered and passed through a pug mill it made first-rate puddle. A space of 12 inches was left on the external side of the trench for puddle backing, and the remainder of the trench was filled to a depth of 18 inches with concrete formed of 6 parts of clean Thames ballast to 1 part of Portland cement. The concreting was commenced on the 26th of June.

On this footing, which was 7 feet 3 inches wide, the side walls were built of 4½-inch brickwork, 1 foot high, to form the inside face of the reservoir, with a layer of concrete behind, 4 feet 1½ inch wide and 1 foot thick, thus making the thickness of the wall 4 feet 6 inches. The construction was thus proceeded with in layers 1 foot at a time, with a header for every fourth course of brickwork to tie it into the concrete. The top of the wall was reached at 6 feet 6 inches from the footing or 8 feet from the puddle.

Meantime, three cast-iron pipes were laid in separate trenches under the western wall. One pipe in connection with the 14-inch pipe, line No. 1, already inserted, the second in connection with the overflow and emptying pipe, the third in reserve ready to be connected with the line of main No. 2, if it should eventually be required. These trenches were filled with well-rammed puddle, and covered with York landings, 6 feet square and 6 inches thick, where the pipes passed under the side wall.

The outside wall having been brought to springing height, the timber at the back of the wall was withdrawn, and the

puddle backing rammed in. It was intended by the author that the wall should be temporarily strutted against the dumpling of earth in the interior, till permanently supported by the cross inverts from east to west, and by the foundations and inverts of the bull's-eyes at the northern and southern ends; but as it would have entailed some extra excavations and timbering, the contractors delayed this part of the work and proceeded with the excavation of the dumpling, some of the earth being used to form the banks of the reservoir, and the remainder being wheeled to spoil on the Company's surplus land.

The earth had been removed down to the level of the 18-inch concrete footing all along the eastern side and northern end of the reservoir without any ill effect, and the cross inverts were being commenced, when the western side wall, from which the earth was being removed, cracked, on the 26th of August, and bulged forward about $2\frac{1}{2}$ inches in the center. Struts were at once put in, and about half of the cross inverts were completed, when, after heavy rain on the 14th of September, the western wall, with the exception of about 30 feet at the north end, slid bodily forward some 5 feet into the reservoir, doubling up three of the cross inverts which should have supported it. The contractors thereupon proceeded to break up the concrete wall, which proved to be of very good quality, and to excavate the earth which had fallen in behind it. When the debris had been removed, a small land-spring was found at the back of the wall; this spring and the trenches which had been cut through the bank for the purpose of laying the pipes under the wall, no doubt, contributed to its destruction.

Although the puddle under the wall had not shifted, it was thought desirable to remove it, and to excavate the ground to an additional depth of 18 inches. The soil was still peat and lignite, on which a layer of 9 inches of puddle was placed, the thickness of the footing of the concrete wall being increased from 18 inches to 3 feet. The foundations of the inverts which had been upset were similarly increased, and the inverts throughout were strengthened by making them of three rings instead of two rings of brickwork.

Before filling in at the back of the reinstated wall, a stoneware drain pipe was laid to collect the land-spring, and lead it into the 12-inch drain-pipe. Seeing that this new piece of wall was without a backing of solid earth, the author thought it desirable to strut the wall by seven cast-iron columns 12 feet long, which happened to be in hand, with one end against the solid earth, and the other against the back of the wall behind each invert, to prevent the wall being forced outward by the internal pressure of the water, and the thrust of the covering arches.

The footings for the end bull's-eyes and cross inverts are built on concrete foundations, 4 feet wide and 9 inches thick, on a 9-inch layer of puddle deposited in trenches 18 inches below the floor of the reservoir, to prevent the puddle being squeezed out from under the concrete. The central cross inverts, which are 12 feet 6 inches from center to center, are 1 foot $10\frac{1}{2}$ inches wide, formed of three rings of brickwork supporting brick piers 1 foot $10\frac{1}{2}$ inches square, 12 feet 6 inches from center to center, and 10 feet high. From the top of these spring the arches of the longitudinal wall carrying the covering arches; this wall is 18 inches thick; the arches are formed of two rings of brickwork, having a radius of 8 feet and a rise of 2 feet; 18 inches above the soffit of these arches spring the covering arches, resting on concrete skew-backs. These are built of two rings of brickwork tied together by the insertion of headers at convenient distances; they have a radius of 8 feet and a rise of 2 feet, the spandrels being filled in with a bed of 15 inches of concrete. The external arches, which carry the thrust of the covering arches down to the concrete side walls, consist of three rings of brickwork, and have a radius of 9 feet 6 inches. These arches have been backed up on the outside with concrete, which not only stiffens them but forms a better surface for the puddle to rest against. The puddle backing 1 foot thick is carried 1 foot above the top water-line.

The north and south walls also have covering arches resting on the concrete walls, which the author believes to be an unusual form of construction. To support these end arches and strengthen the work, he has interposed an arched cross

wall of 2 feet 3 inches between them and the covering arches, and has further stiffened the structure by bull's eyes to take the thrust of the longitudinal arches, and at the same time to act as struts to prevent the concrete wall being forced inward by external pressure. The bull's-eyes are 12 feet 6 inches in diameter, 2 feet 3 inches thick, and are formed of three rings of brickwork, which is toothed into the brickwork of the covering arch, the whole being tied together by two stout hoop-iron bands which extend along each of the longitudinal walls from one end of the reservoir to the other. The floor of the reservoir, the surface of which is level with the inverts, is slightly undulating, and has a fall to the center for drainage. It is formed of 9 inches of concrete laid on 9 inches of puddle; a 9-inch trench was dug down to the puddle beneath the foundations, and a proper connection made between the two layers of puddle The surface of the reservoir is covered with earth to the depth of 1 foot above the crown of the arches, and is sown with grass. The side slopes have an inclination of $2\frac{1}{2}$ to 1; at the north-east corner, the top of the bank is 14 feet above the foot of the slope; an ordinary post and rail fence round the Company's land was included in the contract.

The sluice cocks, on the 14-inch main and on the 12-inch emptying pipes, are fixed in circular pits inside the reservoir; at the back of the emptying cock a branch pipe is carried up, terminating in a bell mouth at the top water-line, which acts as an overflow and allows surplus water to escape. The bricks used were red, wire-cut gault bricks from Dunton Green, laid in Portland cement mortar, 3 parts of sand to 1 part of cement; the sand was partly Thames sand, but chiefly pit sand from Dartford Heath. The reservoir is fitted with twenty-four air pipes for ventilation, access to the interior is by two manholes formed in the covering arches, closed by two heavy cast-iron doors and frames resting on York landings. The descent to the bottom of the reservoir is by a wrought-iron ladder. Severe frosts, during the months of December and January, considerably delayed the completion of the work, which was finished on the 7th of February, 1880.

The Hornsey Wood Reservoir was built for the East London Waterworks in 1868, from the designs and under the direction of Mr. Charles Greaves, M. Inst. C. E., by Messrs. Aird & Son. The internal dimensions are 454 feet 6 inches by 186 feet; with the maximum depth of water, 11 feet 6 inches, it contains about 5,000,000 gallons. The site is on the London clay, the earthwork being for the most part excavation, the pot-water-line is wholly below the original surface of the ground.

The side and end walls are of brickwork in cement 18 feet high, built on a foundation of concrete 5 feet 6 inches wide and one foot thick; the internal faces of the walls are vertical, but the thickness diminishes at the back in half-brick steps from 3 feet 9 inches at the base to 1 foot $10\frac{1}{2}$ inches at the top; the walls have a backing of concrete parallel with the interior face of the wall and 4 feet 6 inches from it.

The roof consists of fifteen vaults springing from the sides, and running longitudinally from end to end of the reservoir; the span of the arches is 11 feet 4 inches, and the rise 2 feet 10 inches; they are formed of two rings of brickwork, and the spandrels between the arches are filled with concrete to the height of 2 feet 6 inches. The vaulting is supported by fourteen longitudinal walls 18 inches thick, with footings 3 feet wide resting on the solid earth; these walls are pierced with twenty-seven arched openings, 13 feet 6 inches wide, with semicircular soffits; the arches are formed of three rings of brickwork, the inverts have a versed sine of 18 inches, and the height of the openings from the bottom of the invert to the soffit of the arches is 9 feet 9 inches; the piers between these openings, which are 3 feet $4\frac{1}{2}$ inches wide, are stiffened by 18-inch buttresses, which diminish from one and a-half brick at the base to one-half brick at the top.

The floor consists of brick inverts 9 inches thick, having an internal radius of 11 feet 1 inch, resting on concrete; there is an extra ring of brickwork 18 inches wide between each of the piers. The height from the springing of the inverts to the springing covering arches is 12 feet 6 inches. The reservoir is covered

with earth to the depth of 2 feet 6 inches above the crown of the arches.

The Haggar Lane Reservoir was recently constructed for the East London Waterworks Company, from the designs and under the direction of the late Mr. George Seaton, M. Inst. C. E.

The earthwork consists one-half of excavation and the other half of embankment. The top water-line, which is 167.50 feet above ordnance datum, ranges from 8 feet 6 inches to 6 feet 6 inches above the original surface of the land. The dimensions of the work, to the outside of the puddle walls, are 191 feet 6 inches by 121 feet 6 inches, the inside dimensions being 182 feet by 112 feet. The exterior walls are built in curved bays, 18 inches thick, but the footings, which rest on 9 inches of concrete, are straight on plan. The bays have a versed sine of 18 inches, and are carried to the top in the end walls, but in the side walls the brickwork is corbelled over to form a straight skew-back for the covering arches. The brickwork is backed with concrete, which fills in the spandrels, and is 3 inches thick at the thinnest place; the concrete has a straight exterior face, between which and the earthwork there is a layer of 18 inches of puddle. The floor is formed of brick inverts 9 inches thick; the inverts have a span of 15 feet, a rise of 2 feet, and rest on concrete, which has a minimum thickness of 6 inches; beneath the concrete there is a layer of puddle 18 inches thick. The vaulting is supported by six 14-inch longitudinal walls, strutted and stiffened by six 14-inch cross walls; these walls divide the reservoir into forty-nine sections, each measuring 25 feet by 15 feet. In order to permit the water to flow through these sections, the side walls of each section are pierced with circular openings 8 feet 3 inches in diameter, and the cross walls with circular openings 6 feet in diameter. The longitudinal walls rest with three courses of footings on 9 inches of concrete; at the top they are thickened to 18 inches to form skew-backs for the springing of the vaulting; the cross walls are built on the inverts to the height of 7 feet 6 inches; for 7 feet in the center the top of the wall is level, whence the brickwork slopes up to the springing of the covering arches. The vaulting, which has a span of 14 feet 3 inches and a rise of 3 feet 9 inches, is formed of two rings of brickwork; the spandrels are filled up with 18 inches of brickwork, and 18 inches of concrete; the arches are covered with 2 feet of earth. The banks of the reservoir have a slope of $2\frac{1}{2}$ to 1.

The vault, next the side of the reservoir from which the water flows through the pipe to the engine, is an exception to the other longitudinal walls; it has only one circular opening at the further end, the object being to induce a circulation of water through the reservoir. This wall is lightened by 9-inch panelling. There is a recess in the floor 2 feet 6 inches wide, with openings under the longitudinal walls to collect the drainage, and lead it to the pipe at the corner of the reservoir. The maximum depth of water is 14 feet from the bottom of the invert; the reservoir contains about 1,500,000 gallons.

The author has been favored by Mr. Alexander Fraser, M. Inst. C. E., the Engineer of the Grand Junction Waterworks Company, with drawings of the Kilburn and Hampton reservoirs.

The Kilburn reservoir is 300 feet long by 175 feet wide; with the depth of water of 20 feet it contains 6,000,000 gallons. It is excavated to a depth of about 14 feet in the London clay; the embankment rises about 13 feet above the original surface of the ground, which is nearly level; the side walls rest on a concrete foundation 5 feet wide and 2 feet thick, and are built of brickwork 1 foot $10\frac{1}{2}$ inches thick, in curved bays 20 feet across, having a radius of 18 feet and a versed sine of 18 inches. The spandrels are filled in with concrete so that the back of the wall is straight; behind the wall is puddle backing 3 feet thick. The brick piers carrying the roof are cruciform in plan, 4 feet 6 inches across by 18 inches thick; they are built with four double courses of footings, and rest on blocks of concrete 6 feet square and 2 feet thick, sunk 4 feet below the level of the floor; the piers are 21 feet 6 inches from center to center. At 15 feet from the floor-line the arches which support the cross walls spring from these piers; they have a span of 17 feet, a rise of 3 feet, and are 18 inches thick. The covering arches spring from the cross walls at 20 feet above the floor-line; they have a span of 20 feet, with a rise

of 4 feet, and are formed of two rings of brickwork. They are stiffened internally by an arched rib of 18 inches square, springing from each of the cruciform piers. Where the arches rest on the end walls the brickwork of the curved bays is corbelled out to a straight skew-back for the arches to spring from.

The outer wall is supported against the pressure of the external earth by buttresses between each of the curved bays, which extend as far as the foot of the first of the cruciform piers; they are 1 foot 10½ inches thick, and are built with four double courses of footings on a concrete base 6 feet wide and 2 feet thick. The floor is concrete 12 inches thick. The reservoir is covered with earth to the depth of 1 foot 6 inches above the crown of the arches. The cost of the reservoir, which was built in 1872 by Messrs Aird and Son, was about £23,000. The brickwork is very similar to that of the Nunhead reservoirs, except that in the latter, bull's-eyes are used for the support of the external walls instead of buttresses. The earthwork, however, is of an entirely different character, the Nunhead reservoirs being built on sloping ground.

The reservoir at Hampton is 240 feet long and 155 feet wide; the maximum depth of water is 12 feet, and it contains 2,750,000 gallons. It is built entirely of concrete; the foundation rests on blue clay, above which the soil consists of excellent clean sharp ballast, admirably suited for concrete. The side walls are 6 feet thick at the base, 3 feet thick at the top, and 14 feet high; the back of the walls is vertical, the whole of the latter being on the internal face.

The walls are backed with 18 inches of puddle. The piers are 12 feet from center to center, and 13 feet high; they are taper, being 2 feet square at the base and 18 inches square at the top; they are capped with York stone, on which rest wrought-iron rolled joists 8 inches deep, with 4-inch flanges; the joists are tied with ¾-inch galvanized wrought-iron rods 6 feet apart, which fasten the whole of the roof together, and relieve the side walls from any outward thrust.

The roof consists of arches, with a span of 12 feet and a rise of 2 feet 6 inches, springing from the wrought-iron joists; the concrete is 9 inches thick at the crown and 18 inches thick at the haunches. The concrete is covered with 6 inches of earth. The floor is formed of concrete 1 foot thick. The reservoir is not rendered internally; but the surface of the concrete was smoothed over with cement mortar as the moulds were removed. The concrete was made from ballast dug out of the excavation, from which all stones that would not pass through a 1½-inch sieve were removed before mixing, or broken to a proper size. The proportion was 6 measures of ballast to 1 measure of Portland cement. This reservoir is entirely below the original surface of the ground. The concrete in the piers takes some time to set sufficiently hard to be loaded, and on this account, if time is an object, it is in Mr. Fraser's opinion, questionable whether brickwork would not be preferable for the piers, although concrete is somewhat cheaper. The reservoir was built in 1880, at a cost of about £8,000.

The Burton-on-Trent reservoir has been constructed for the South Staffordshire Waterworks Company from the designs and under the direction of Mr. W. Vawdrey, M. Inst. C. E.

It is built on a hill to the south of the town on stiff clay soil; about one-half of the earthwork consists of excavation and one-half of embankment. It is rectangular on plan, covered by brick arches, springing from cast-iron girders, supported on cast-iron columns. The dimensions at the floor-line are 224 feet 6 inches by 145 feet 10 inches, and at the top 229 feet 6 inches by 150 feet 10 inches; the depth of water is 21 feet.

The external walls are of concrete faced with brindled Stafford brickwork built in Portland cement. On plan the brick walls are formed of a series of brick piers with curved bays between them; the distance between the centers of the piers in the side walls is 13 feet 6 inches, and in the end wall 15 feet. The versed sine of the curves is 1 foot; the brickwork, which is built with every alternate four courses, 14 inches and 9 inches thick, has a batter of 1 in 8; it is carried down 3 feet 6 inches below the floor, and rests with four courses of footings on a foundation of 18 inches of concrete. The back of the concrete wall is straight in plan, and is carried down vertically with the exception of a 6-inch set-off at the junc-

tion of the solid ground and the embankment. The total height of the wall is 25 feet 6 inches; the minimum thickness of the concrete and brick in the center of the bays is 3 feet $1\frac{1}{2}$ inch at the top, and 6 feet $7\frac{1}{2}$ inches at the bottom; the base of the concrete, which extends under the footings of the brickwork, is 11 feet wide; the concrete wall is backed with, and rests on, puddle 12 inches thick; the floor is a bed of 10 inches of concrete, on 18 inches of puddle. There are one hundred and forty-four hollow columns, which rest on stone blocks 3 feet square by 15 inches thick. These stones project 3 inches above the level of the floor, and are supported on blocks of concrete 5 feet square by two feet thick, below which there is a 12-inch layer of puddle.

The columns are cylindrical, of cast iron, 10 inches in external diameter, and 1 inch thick; they are made in two pieces, each piece being 10 feet long, bolted together in the center. The girders, which are bolted to the top flanges of the columns, are 15 feet long, I shaped in section, the depth at the ends being 10 inches, and in the center 20 inches; the bottom and top flanges are of equal width throughout, viz., 8 inches and 16 inches; the thickness of the top flange is $1\frac{1}{4}$ inch, of the bottom flange $1\frac{1}{2}$ inch, and of the web 1 inch. Each girder is bolted at the end to the one next it; the girders are tied together by two $1\frac{1}{8}$-inch galvanized tie-bars to each length of girder, and the sidewalls are thus relieved of any thrust from the roofing.

The covering arches have a span of 13 feet 6 inches and a rise of 18 inches, and are formed of two rings of brickwork in cement. The spandrels of the arches are filled in with concrete and the concrete is covered with asphalt to prevent the percolation of rain-water through the roof. The rain-water is collected in drains which discharge at each side of the reservoir. The roof is covered with about 1 foot of soil and turfed over. The parapet which retains this covering is curved to a height just level with the soil over the arches; it rises 3 feet above the top of the earthwork, and is neatly formed of blue bricks with panels of blue and red chequer work. The top of the bank is about 10 feet wide, with a slope of 4 to 1 to the original surface. The reservoir contains about 4,000,000 gallons, and cost about £15,000.

Covered Reservoirs in Germany and Austria.

The author had an opportunity, during a short tour with Mr. Alexander Aird, Assoc. Inst. C. E., of Berlin, of seeing something of the waterworks of the principal cities of Germany. He therefore proposes to give some description of their reservoirs, together with a few incidental notes on other points.

The reservoirs of the Berlin Waterworks at Charlottenburg are built of brick, and are very similar in construction to the Kilburn reservoir, cruciform piers being used; but the sidewalls are straight on plan, and wholly of brick, 10 feet thick at the base, and battering to 4 feet at the top. The span of the arches is 20 feet, the depth of the water 14 feet 9 inches, and the contents of each reservoir are 2,541,000 gallons.

Instead of buttresses, bulls'-eyes are built between the side walls and the first row of piers, the spaces between the bulls'-eyes being arched over; consequently, the arches in front of the end walls are at right angles to the main covering arches across the reservoir, and form groynes where vaults intersect. By this arrangement only four abutments, one abutment for each of the four corner 20-feet compartments, are necessary.

The site of the reservoirs is very fine, loose sand, so absorbent that it was necessary to spread bitumenized paper over the bottom of the excavation before concrete could be laid on it. The floor consists, first, of bitumenized paper; secondly, of a layer of 15 inches of concrete; thirdly, of 15 inches of puddle; fourthly, of 6 inches of concrete, the surface of which is rendered. The reservoirs are fitted with an electric water-level indicator, with an automatic graphic recorder, by Messrs. Hipp & Co., Neuchatel, Switzerland, of which Mr. Henry Gill, M. Inst. C. E., the chief engineer, has favored the author with the following description:

"The Charlottenburg reservoirs are distant from Tegel about 7,500 yards, and two wires are laid between the respective engine houses; that at Charlottenburg contains the battery. The float in the reservoir at each rise or fall of 4 inches completes the circuit, and actuates the

registers at the engine-houses at Charlottenburg and Tegel. At Tegel an electric clock drives a drum which revolves once in twenty-four hours; on this drum an endless roll of paper is wound; in front of this horizontal paper-covered drum runs a small trolly carrying an ink-fed style or pointer. The trolly is caused to advance or recede by the currents produced by each contact made by the rise and fall of the float in the reservoir, and as the style is in contact with the paper on the revolving drum, a diagram is traced which records the height of water in the reservoir at any given time. Its accuracy is checked once a day by Morse's dot and dash telegraph. The apparatus is found to work very satisfactorily. The water which is pumped into the reservoir from Tegel is derived from a series of wells, sunk near the margin of the Tegel lake, which yields an abundant supply of water, (10,000,000 gals. per diem); but unfortunately the water contains filaments of an alga (*Crenothrix Polyspora*), which, on passing through the iron water mains, is thrown down in the form of a brown deposit. This accumulates in the reservoirs and water mains to such an extent that the water as delivered to the consumers is frequently much discolored. It is remarkable that it was not till the works had been in action for six months that any sign of the alga made its appearance."

Mr. Gill has favored the author with drawings of the Berlin covered filters; they are covered reservoirs, which only differ from others in containing filtering material.

Mr. Gill makes the following observations on the climate of Berlin: "Frost usually sets in here about the 15th of November; if it lasts about ten days, the average duration of the first attack, the earth is frozen to the depth of 12 inches, and open reservoirs and filters are covered with ice from 4 inches to 6 inches thick; then there comes a relaxation of the grip of winter for three or four weeks, so that during the day the thermometer rises to 45° or 50° Fahrenheit, but falls to freezing point at night. The days are too short, however, for the mid-day temperature to have much effect upon the frozen ground or the sheets of ice on the reservoirs, and these remain increasing in thickness to about the middle or end of February; but even when, say about the end of February, the ice has become thin, and early in March has disappeared, the night frosts prevailing till the end of March prevent the laying dry and cleansing of the filters."

It is in Berlin seldom possible to cleanse an uncovered filter in the period from the 15th of November to the 15th of March. It is therefore indispensable, if the necessity of giving unfiltered water is to be avoided, to construct filters so that they can be cleansed even during the prevalence of the severest frost. Hence the vaulted filters of the Berlin Waterworks, which have been copied in the Magdeburg Waterworks. Each division of the filter measures 175 feet by 125 feet, and is divided into sections 11 feet 6 inches square by the piers, which are two bricks or 20 inches square. These sections are domed over with Bohemian vaulting (*Bomische Kappen*), in the apex of which there is an opening for a window. Each dome which is a segment of a hollow sphere, rests on longitudinal and transverse arched ribs springing from the piers referred to above. The arched ribs are kept low, so that there is only just room for a man to wheel a barrow under the center, which prevents the barrow striking against the piers. The size of the window is so regulated that the surface of sand in every separate section gets a direct ray of light. This is necessary in cleansing the filter. The window is covered loosely with a sheet of glass $\frac{1}{2}$ inch thick, and during severe frosts a wooden shutter is laid over the glass; in summer the glass and shutter lie off at the side. The barrow road, for which a special inclined archway is built, consists simply of movable planks resting on supports; when not in use the planks are laid on brackets above the water-line.

The foundation of this filter is a layer of 18 inches of puddle; but the base of the side walls, and the inverts which support the piers, rest on gravel puddle 6 feet 6 inches wide under the side walls, and 5 feet 4 inches wide under the inverts. It is made as follows: First, a layer of 6 inches of puddle, then of 3 inches of metal or broken stone as used for macadamized roads; this is rammed into the clay till the clay rises to the surface; similar layers succeed, each of puddle and metal, till the required thickness

is obtained, the stone being thoroughly rammed into the clay. The floor consists of 9 inches of concrete rendered. The walls are of brick 3 feet 3 inches thick and 11 feet high. The water-line is 8 feet above the floor, which is also the springing-line for the arched ribs. The roof of the filter is covered with 2 feet of earth.

These filters are rather expensive, the cost at Berlin, including valve-chambers, valve-regulators, &c., is about 90*s*. per square meter, but the covering is indispensable.

The reservoir at Breslau consists of two wrought-iron tanks fixed inside a water-tower 130 feet above the ground. The tower is built of brick, square on plan with cross walls to stiffen the structure. These divide the building into four compartments, which are utilized as engine houses, and contain a pair of single-acting Cornish pumping engines and a vertical direct-acting double cylinder fly-wheel pumping engine; there is still vacant space for another engine: The wrought-iron tanks are each 84 feet long by 41 feet wide and 20 feet deep, containing about 900,000 gallons. They rest on expansion plates, so that they are free to expand or contract without affecting the brickwork. The tower which entirely encloses the tanks is covered with a slate roof, and presents a bold and massive appearance. It stands on the bank of the river Oder, from which the city is supplied with filtered water.

The reservoirs of the Vienna Waterworks are computed to contain about 21,000,000 gallons, which at the time when the author visited the work (October) was equal to three days' consumption. This is sufficient to permit of repairs being made to the aqueduct without interfering with the water-supply to the city; the emptying and filling of the aqueduct occupies about thirty-six hours, leaving thirty-six hours for the actual repairs. The aqueduct is about 55 miles in length, and eight hours are occupied in the water flowing from the source at Kaiserbrunnen to Vienna. The quantity of water supplied by the two springs which feed the aqueduct, the Kaiserbrunnen and the Stixenstein, varies from 145,000 cubic meters per diem in summer to less than 29,000 cubic meters in winter, when the snow and other sources from which these springs are fed are frost bound. This quantity is less than half the estimated water-supply. In order to supplement the supply, wells have been sunk in the gravel beds of the valley of the Schwarza at Pottschach, from which 17,000 cubic meters per diem are pumped into the aqueduct during winter; these supplemental works were designed and executed by Baron Schwarza and Mr. Aird's firm.

The author is indebted to the Oberingenieur Mihatsch for the particulars of the Schmelz reservoir. It is 347 feet square, with a depth of 12 feet of water, and contains about 8,000,000 gallons. The foundation is cut out of the solid rock. The side walls are of brick 5 feet thick; the piers are of masonry 3 feet 6 inches square on brick footings; the cross vaulted roofs rest on these piers, the span of the vaulting being about 22 feet. The floor consists of one course of brickwork laid flat on the surface of the rock, over which is a layer of concrete. The bottom and sides are rendered with Portland cement. Great care is taken to ensure circulation of water through the reservoir, it being considered very undesirable that any portion of the water should be allowed to become stagnant; hence there are division walls connecting the piers.

The new waterworks at Munich, which were in course of construction by Mr. A. Aird's firm, comprise a high-level reservoir about 7 miles from that city, and an aqueduct or conduit about 19 miles in length from the springs in the Mangfallthal to the reservoir. These springs issue from the west side of the valley about half way up the slope, where an extensive bed of porous limestone-gravel rests on a bed of impervious clay, and are at a sufficient elevation to supply Munich by gravitation.

The reservoir is divided into two compartments by a division wall; each compartment is 272 feet square, with a depth of water, when full, of 10 feet, the contents altogether being about 8,800,000 gallons. The bottom is 10 feet 6 inches below the original surface of the ground; the soil is limestone gravel, consisting for the most part of smooth water-washed pebbles. The side walls are of concrete 3 feet 8 inches thick and 7 feet 6 inches high, built in a trench 6 feet 6 inches

wide; these walls have a stepped toe or footing on the inside, which not only gives a wider foundation, but is useful in breaking the joint with the concrete floor. The floor itself has a uniform thickness of 2 feet over the whole area of the excavation, the piers and division wall being built upon it. The end walls on which the covering arches rest are carried up in brickwork to the height of 3 feet 10 inches above the concrete, diminishing from 30 inches at the base to 15 inches at the top, which corresponds with the level of the water in the reservoir when full. The piers supporting the cross arches are also of brick 26 inches by 20 inches (German bricks are somewhat longer and wider than English bricks). The height from the floor to the springing is 4 feet 3 inches; the cross arches for supporting the vaulting are formed of three rows of brickwork, with a span of 10 feet 2 inches and a rise of 30 inches. The vaulting is semi-circular and has a span of 10 feet 3 inches, the springing being 7 feet 8 inches above the floor; it is formed with one ring of brickwork about 5 inches thick. The spandrels are filled in with concrete to the height of 3 feet, and the ends of the vaults are closed with brickwork, battering from 3 feet to 15 inches, built on the concrete wall. The division wall between the two compartments of the reservoir is of brickwork to the level of the springing of the vaulting, being 4 feet 10 inches thick at the base and 3 feet at the top; the spandrel between the two vaults which rest on it is filled in with concrete to the height of 2 feet 8 inches. The crowns of the arches are covered with earth to the depth of 3 feet 9 inches; and the banks of the reservoir have a slope of $1\frac{1}{2}$ to 1.

The concrete used in the construction of the above is made of 5 parts of washed and screened gravel from the excavation (all large stones being broken), 1 part of washed siliceous sand, brought from a distance, and 1 part of Portland cement. The sides and bottom of the reservoir are rendered in the following manner: As soon as the moulds have been withdrawn from the concrete after it has set, the surface is rendered with cement mortar made of 1 part of Portland cement to 1 of sand, only just sufficient mortar being used to give a smooth face to the work; it is then coated with a film of neat cement, and rubbed with irons till it takes a high polish. The finished work rings like solid marble.

The inlet and outlet pipes are built into the concrete walls. The conduit from the Mangfallthal terminates in a $29\frac{1}{2}$-inch pipe. There is a double line of mains from the reservoir to the city, each line of pipes being $27\frac{1}{2}$ inches in diameter.

Frankfort-on-the-Maine is supplied with water from springs in the Vogelsberg and Spessart mountains, distant about 40 miles from the city. The water from the springs flows through an aqueduct by gravitation to a high-level reservoir in the Friedberger warte, which contains 37,500 cubic meters or 8,500,000 gallons.

The reservoir consists of three compartments, the longest of which is a recent addition; an arched gangway is formed on the center division wall, which extends from one end of the reservoir to the other. The two old compartments consist of sixteen vaults, with alternate openings in the cross walls, which cause the water to flow in a zigzag manner from one end of the compartment to the other, the object being, as at Vienna, to ensure thorough circulation of the water. The vaulting has a span of about 15 feet, and is formed with four rings of brickwork. The depth of water in the reservoir, when full, is about 12 feet.

The water from these mountain springs is excellent in quality, but the quantity is far below the requirements of the city, especially in summer, when it is necessary to shut off the water from the city at noon to accumulate sufficient in the reservoir for the next morning's consumption.

The reservoir at Darmstadt is built of brick. The foundations only are below the original surface of the ground. It consists of two compartments, each 80 feet by 72 feet, with a depth of water of 13 feet. The bottom is concrete, 20 inches thick, overlaid by 9 inches of brickwork, the surface of which is rendered with cement mortar. The side walls are 8 feet thick at the base and 4 feet thick at the top. The piers of the cross arches for supporting the vaulting are 6 feet 6 inches wide and 25 inches thick; the arches have a span of 13 feet

$1\frac{1}{2}$ inch, and a radius of 10 feet. The vaulting is semicircular, with a span of 13 feet $1\frac{1}{2}$ inch; the springing is 11 feet 8 inches from the floor, and is formed of two rings of brickwork; the division wall between the two compartments is 6 feet 6 inches thick; the spandrels of the vaulting are filled up with concrete. The reservoir is covered with earth to the depth of 3 feet 3 inches above the crown of the arches; the banks, which are of sandy soil, have a slope of $1\frac{1}{2}$ to 1. The inlet and outlet pipes are so arranged that the water flows in at the top of the reservoir, but passes out through self-acting valves from the bottom. The reservoir is fitted with a ball-float, connected by a wire to the pumping station; when the reservoir is full an electric bell rings in the engine-house; there is also a communication between the reservoir and engine-house by means of a telephone. The cost of this reservoir has been 168,000 marks. It holds about 900,000 gallons.

The water with which Darmstadt is supplied is obtained frem six tube wells, about 16 inches in diameter, driven to a depth of from 150 feet to 200 feet in the sandy plain between the river Rhine and the Odenwald Mountains at Griesheim, about 5 miles from Darmstadt. The general character of the sand is too fine to yield much water, but it is traversed by permeable veins of coarse sand and fine gravel, which afford an ample supply. The normal water-level is 10 feet below the surface of the ground. The engine pumps direct from the wells into the mains, the suction-pipe from the pump having branches which dip into each of the six wells; when pumping the maximum quantity required for the town, 500,000 gallons per diem, the water in the tube-wells is depressed 11 feet, but in testing the yield of the wells double the quantity has been obtained.

The project for the supply of Darmstadt was submitted to the Town Council by Mr. A. Aird's firm, in 1874, who called in an engineer to supervise the execution of the work. The latter, however, instead of so doing, persuaded the Town Council to carry out a project of his own, and instead of tube-wells, substituted a well 26 feet in diameter, which he sank to a depth of 105 feet, by the pneumatic system; but the large well only yielded one-tenth of the required supply. Mr. A. Aird's firm were thereupon applied to, and successfully carried out their original scheme.

The covered reservoir at Cologne is erected on a tower in the center of the city, about 100 feet above the ground. It is a circular cast-iron tank, containing about 800,000 gallons, divided into two concentric compartments; the inner one is 75 feet in diameter, and the outer, which is annular, has an outside diameter of 105 feet; there is an opening in the center for the staircase. The depth of the tank is 15 feet; it is covered with a wrought-iron trussed roof, which rests on the tank itself, and is boarded over and covered with zinc.

The tank is supported on a series of concentric and radiating brick walls. The author is informed by the engineer, Director Hegener, that when he took charge of the works there were leaks in the bottom of the tank, and that, in consequence of the above walls being built without openings, holes had to be cut through them to ascertain where the defects were. Eventually the whole of the bottom was covered with Portland cement concrete of sufficient depth to cover the flanges, which has been effectual in preventing further leakage. Only a fraction of the water consumed passes into this tank, which holds about one-seventh of the daily supply.

Cologne derives its water supply from two brick wells sunk within 28 yards of the river Rhine; they are 18 feet in diameter, and have a depth of 60 feet, or 36 feet below the lowest water level of the Rhine. The strata consists of sand and gravel mixed with boulders. At the depth of 46 feet, under a layer of large boulders, a bed of gravel 6 feet thick was met with, which was extremely hard, and took the divers many months to sink through; under this came clean, coarse gravel, from which the water is pumped. The daily yield is 5,000,000 gallons, with a depression of 21 feet in the wells. Although the wells are so close to the Rhine, the spring water obtained from the gravel is supposed to be without any admixture from the river, as the temperature of the spring water varies only 5° Fahrenheit compared with a variation of 38 degrees in the Rhine water, viz., from 32° to 70°. The spring water is also

much harder, being from 17° to 18°, whereas the Rhine water is only 7°.

The author has ascertained the particulars of the Dresden reservoir from "Das Wasserwork der Stadt Dresden," published by the engineer, Mr. B. Salbach.

It is built in the form of a parallelogram, divided by a wall into two compartments, each 172 feet long by 122 feet wide; the depth of water is 16 feet 6 inches, and the reservoir contains 4,400,000 gallons. The side walls, piers, cross arches and division walls are built in coarse masonry of sandstone, with cement mortar; the vaulting is formed of two rings of hard burnt porous bricks in cement, which is rendered externally to prevent the percolation of rain water into the reservoir. The side walls are 6 feet 9 inches, and the division wall 7 feet 6 inches thick; the piers are 40 inches wide by 20 inches thick; the span of the cross arches is 16 feet; the vaulting, which springs from top water-level, has a span of 16 feet, and a rise of 47 inches.

The excavation is in compact fine sand, over the bottom of which, including the foundations of the walls and piers, &c., a uniform thickness of 2 feet of concrete was laid. This consisted, in the first place of a layer of rough pieces of granite, with the flat side on the sand; above this a thin bed of concrete was spread and lightly rammed to fill the interstices betweeen the stones, and over this the concrete was deposited in the usual manner in 6-inch layers, and solidly rammed.

The concrete under the foundations of the walls and piers was laid first, and the rest, which forms the bottom of the reservoir, subsequently, and the surface was protected during the progress of the work by a course of brick-on-edge in cement; on the completion of the work two courses of bricks were laid flat, and the suface rendered in cement. The whole of the internal surfaces of the reservoir, including the piers, are rendered over with cement, with a hard, polished surface, partly for the purpose of making them watertight, partly to prevent the deposit of mud or slime in the interstices of the porous sandstone.

The Hanover reservoir, is situate on the Lindener Berg. In order to maintain a pressure in the city due to a height of 33 meters (108 feet) it was built entirely above ground, only sufficient soil being excavated to obtain a good foundation on the solid limestone.

The reservoir is completely exposed, without any embankment to support it, principally on account of the great expense which would have been involved in carting earth from a distance none being available near the site. Moreover, an embankment has its disadvantages, as it may possible cover up defects or leakages in the brickwork, which may do considerable damage before discovery; whereas, in a reservoir with the walls exposed the slightest leakage is at once detected. The reservoir consists of two divisions, each 106 feet long by 98 feet wide; the depth of water is 19 feet 6 inches, and it contains altogether about 2,400,000 gallons. It is built of hard burnt bricks in Portland cement mortar. The side walls have a trapezoidal profile, and are strengthened by buttresses: the inner side is vertical; the thickness at the base is 14 feet 9½ inches, and at the level of the springing of the vaulting, which corresponds with the top-waterline, it is 7 feet 6 inches thick. The division wall, which has vertical faces and is stiffened by piers, is 9 feet 6 inches thick. The interior surface of these walls is rendered with Portland cement.

The bottom of the reservoir is formed of a layer of concrete from 3 feet 3 inches to 4 feet 6 inches thick, on which piers are built for supporting the arched cross wall to carry the vaulting. The piers are 4 feet 6 inches by 2 feet 6 inches, 15 feet high, and stand about 16 feet apart from center to center, transversely dividing the breadth of the reservoir into six equal parts, and about 15 feet from center to center longitudinally, so that each division of the reservoir is covered by seven vaults, which have a span of 12 feet, and a rise of 3 feet 3 inches. The upper surface of the vaulting is covered with sloping brickwork in cement, and rendered, above which there is a layer of 3 feet 3 inches of sand, over which succeed 6 inches of mould turfed over. The rain water percolating through this covering is conveyed into the reservoir through drainpipes. The total height of the reservoir above the ground-level is 30 feet.

That part of the brickwork above the vaulting which merely serves to support the covering earth, is built with Roman

cement. The reservoir is fitted with fourteen ventilators. The floor is finished with four layers of Hanoverian clinker bricks laid in Portland cement mortar, and rendered in Portland cement hardened under the trowel, *i.e.*, the cement is worked under the trowel and pressed until it becomes hard and smooth.

The description and drawings of this reservoir are taken from "Das Neue Wasserwork der Koniglichen Residenzstadt Hanover," by Oberbaurath Berg.

The author was struck by the marked preference of the Germans for spring water as compared with lake or river water. In England it is generally assumed that the water from mountain lakes, such as Loch Katrine or Thirlmere, is the purest and most wholesome. The Germans object to such waters as being stagnant and dead; they prefer water as it gushes fresh and sparkling from the mountain side, and which has a constant temperature; whereas lake water is too cold in winter and warm and vapid in summer. Where natural springs are not available they prefer to tap the subterranean streams of water which, in many parts of Germany, flow either parallel with the rivers or down towards them from the surrounding country, rather than to use the water from the rivers, however well filtered or soft it may be. In fact, an equable temperature is deemed of the first importance in choosing potable water, a moderate degree of hardness not being objected to.

The following towns, the whole of which have populations exceeding 50,000, are supplied with water pumped from underground springs: Aix-la-Chapelle, Augsburg, Berlin, Cologne, Crefeld, Dantzig, Dresden, Dusselldorf, Elberfeld, Essen, Gratz, Halle, Hanover, Cassell, Königsberg, Leipzig. Nürnberg, Strasburg.

As compared with similar towns supplied from rivers: Altona, Berlin, Brunswick, Bremen, Breslau, Brunn, Hamburg, Magdeburg, Posen, Stettin.

These two lists comprise all the large towns of Germany and Austria, except such as are supplied from natural springs, as in Vienna and Frankfort, and those which have no properly organized system of water supply.

Design for Covered Service-Reservoir.

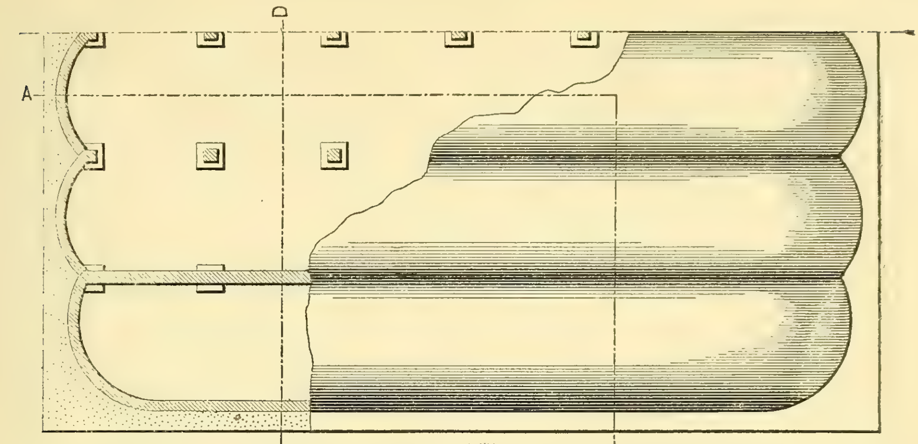
Having laid the foregoing examples of covered reservoirs before the Institution, the author proposes to conclude the paper with a sketch of his idea of a model Covered Service-Reservoir.

It should, if possible, be built on a level piece of ground, and the soil should be perfectly solid and homogeneous. These conditions are assumed to exist. The reservoir as designed is 100 feet square on plan, with a maximum depth of 17 feet, and would contain 1,000,000 gallons of water.

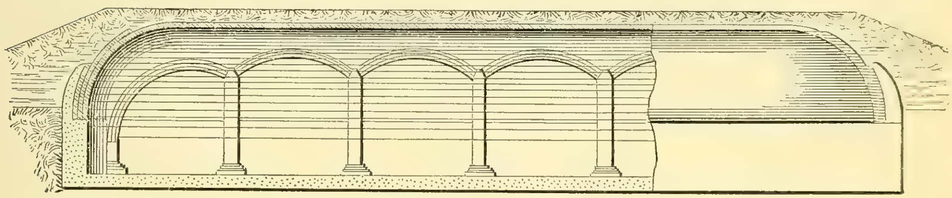
The design is so arranged that the earth dug from the excavation is utilized in covering the reservoir and forming the necessary embankments; the depth of the excavation will therefore depend on the slope given to the embankment. In this case, a slope of 2½ to 1 is adopted; and it is calculated that an excavation 11 feet deep will furnish sufficient earth to form an embankment 13 feet above the ground-line, including a layer of soil 2 feet thick over the roof of the reservoir.

The author has adopted a similar construction to that of the Munich reservoir for the lower part of the structure, it being assumed that there will be no difficulty in obtaining good ballast for concrete in the neighborhood of the site.

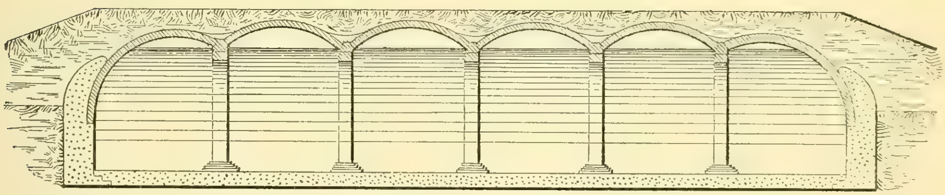
The work should be commenced by digging trenches 7 feet wide and 11 feet deep for the reception of the outside walls (it will be convenient to designate the outer walls, which are parallel with the vaulting, the side walls, and those at right angles to the vaulting, the end walls). The bottom of these trenches should be covered with 18 inches of concrete, on which concrete side walls, 4 feet thick, and 7 feet 6 inches high, should be constructed, the concrete being rammed between the moulds for forming the internal face and the external earth. The end walls should be similar, except as regards the internal face which should have six curved bays, the radius of the curve being 11 feet 6 inches. As soon as the concrete in these walls has set, the moulds should be taken down, and the surface rendered with Portland cement worked with the trowel, till it becomes hard and smooth, and then finished with a film of neat cement worked to a fine polish.



HALF PLAN



LONGITUDINAL SECTION ON A.B.



CROSS SECTION ON C.D.

DESIGN FOR RESERVOIR

If the ground be firm, the toe of the wall, supported by the internal earth, will be sufficient to sustain the wall against external pressure without strutting; but care should be taken not to load the ground outside the reservoir by the premature construction of the embankment. The internal earth or dumpling should now be removed down to the level of the toe of the wall, but in removing the earth below this level, which should be done piece-meal, the layer of concrete forming the floor of the reservoir should be at once substituted, the thickness of which, 2 feet, would when set be ample to resist any ordinary external pressure tending to thrust forward the toe of the wall.

The floor having been laid, the brick piers for supporting the vaulted roof should be built on it. These are 1 foot $10\frac{1}{2}$ inches square on plan, and 16 feet 8 inches from center to center in each direction. From these piers, at 13 feet from the floor, spring the arches which support the vaulting; the arches in the center have a radius of 11 feet 6 inches, but the two end arches, which have a radius of 9 feet 6 inches, are brought down, so that the thrust of each row of arches is carried below the ground-line.

The vaulting, which, is built with a

radius of 11 feet 6 inches, is similar to that of the Farnborough reservoir, but the ends of the covering arches are brought down with a curve of 12 feet 6 inches radius, till they rest on the correspondingly curved bays of the concrete end walls already described. The two exterior arches, being brought down at each end in a similar manner, form the rounded corners of the reservoir.

On the completion of the brickwork, the 2-feet space between the top of the concrete wall and the surface of the soil should be filled with concrete backing, which should be carried up to the height of 6 feet above the ground line. The earthwork of the embankment should now be proceeded with, and the floor and internal faces of the side arches rendered with Portland cement, to make the structure water-tight.

By the above arrangement, the thrust of the roof is carried down to an abutment, resting against undisturbed ground. The pressure of the water is not only supported by the same abutment, but it is also counterbalanced by the pressure of the made earth forming the embankment which rests on the exterior arches of the reservoir, in addition to the pressure of the earth, which would have to resist the internal pressure if the walls were vertical.

The author believes the above system of construction to be secure and economical, and that, by its adoption, the water-line may be raised higher above the original surface than could safely be done with vertical walls, unless such walls be made sufficiently thick to resist the pressure of the water without the aid of earthwork, as in the Hanover reservoir, which would add considerably to the cost.

Where favorable conditions of site do not exist, some modification of the above model would be necessary; such, for instance, as the use of puddle, should any settlement of the foundations be apprehended which might cause cracks in the structure.

THE GENERAL THEORY OF THERMODYNAMICS.

By Prof. Osborne Reynolds, M.A., F.R.S.

From "The Engineer."

Thermodynamics is a very difficult subject. The reasoning involved is such as could only be expressed in mathematical language; but this alone would not prevent the leading facts and features of the subject being expressed in popular language. The physical theories of astronomy, light and sound involve even more mathematical complexities than thermodynamics; but these subjects had been rendered popular, and this to the great improvement of the theories. What rendered the subject of thermodynamics so obscure was, that it dealt with a thing or entity (heat), which, although its effects could be recognized and measured, was yet of such a nature that its mode of operation could not be perceived by any of our senses. Had clocks been a work of nature, and had the mechanism been so small that it was absolutely imperceptible, Galileo, instead of having to invent a machine to perform a definite function, would have had, from the observed motion of the hands, to have discovered the mechanical principles and actions involved. Such an effort would have been strictly parallel to that required for the discovery of the mechanical principles of which the phenomena of heat were the result. In the imagined case of the clock, the discovery might have been made in two ways. By the scientific method; from the observed motion of the hands the fact that the clock depended on a uniform intermittent motion would have led to the discovery of the principle of the uniformity of the period of vibrating bodies; and on this principle the whole theory of dynamics might have been founded. Such a theory of mechanics would have been as obscure, but not more obscure than the theory of thermodynamics based on its two laws. But there was another method, and it was by this that the

theory of dynamics was brought to light—to invent an artificial clock, the action of which could be seen. It was from the actual pendulum that the principles of the constancy of the periods of oscillating and revolving bodies were discovered, whence followed the dynamical theories of astronomy, of light, and of sound.

As regarded the action of heat, no visible mechanical contrivance was discovered which would afford an example of the mechanical principle and motions involved, so that the only apparent method was to discover by experiment the laws of the action of heat, and to accept these as axiomatic laws without forming any mental image of their dynamical origin. This was what the present theory of thermodynamics purported to be. In this form the theory was purely mathematical, and not fit for the subject of a lecture. But as no one who had studied the subject doubted for one moment the mechanical origin of these laws, Professor Reynolds would be following the spirit if not the letter of his subject if he introduced a conception of the mechanical actions from which these laws sprang. This he should do, although he doubted if he should have so ventured, had it not been that while considering this lecture he hit upon certain mechanical contrivances, which he would call kinetic-engines, which afforded visible examples of the mechanical action of heat, in the same sense that the pendulum was a visible example of the same principles as those involved in the phenomena of light and sound. Such machines, thanks to the ready help of Mr. Foster, his assistant in constructing the apparatus, he should show, and he could not but hope that these kinetic engines might remove the source of the obscurity of thermodynamics on which he had dwelt. The general action of heat to cause matter to expand was sufficiently obvious and popularly known; also that the expanding matter could do work was sufficiently obvious. But the part which the heat played in doing this work was very obscure. It was known that heat played two, or it might be said three, distinct mechanical parts in doing this work. These parts were: (1) To supply the energy necessary to the performance of work. (2) To give to the matter the elasticity which enabled it to expand—to convert the inert matter into an acting machine. (3) To convey itself, *i. e.*, heat, in and out of the matter. This third function was generally taken for granted in the theory of thermodynamics, although it had an important place in all applications of this theory. The idea of making a kinetic engine which should be an example of action such as heat had no sooner occurred to him than various very simple means presented themselves. Heat was transformed by the expansion of the matter caused by heat. At first he tried to invent some mechanical arrangement which would expand when promiscuous agitation was imparted to its parts, but contraction seemed easier—this was as good. All that was wanted was a mechanism which would change its shape, doing work when its parts were thrown into a state of agitation. In order to raise a bucket from a well either the rope was pulled or the windlass wound—such a machine did not act by promiscuous agitation; but if the rope was a heavy one—a chain was better—and it was made fast at the top of the well so that it just suspended the bucket, then if it was shaken from the top waves or wriggles would run down the rope until the whole chain had assumed a continually changing sinuous form. And since the rope could not stretch, it could not reach so far down the well with its sinuosities as when straight, so that the bucket would be somewhat raised and work done by promiscuous agitation. The chain would have changed its mechanical character, and from being a rigid tie in a vertical direction would possess kinetic elasticity, *i. e.*, elasticity in virtue of the motion of its parts, causing it to contract its vertical length against the weight of the bucket. Now, it was easy to see in this case that to perform this operation the work spent in shaking the rope performed the two parts of imparting energy of motion to the chain and raising the bucket. A certain amount of agitation in the chain would be necessary to cause it to raise a bucket of a certain weight through a certain distance, and the relation which the energy of agitation bore to the work done in raising the bucket followed a law which, if expressed, would coincide exactly with the second law of thermody-

namics. The energy of agitation imparted to the chain was virtually as much spent as the actual work in raising the bucket, that was to say, neither of these energies could be used over again. If it was wanted to do further work, the raised bucket was taken off, and then to get the chain down again it must be allowed to cool, *i. e.*, the agitation must be allowed to die out; then attaching another bucket, it would be necessary to supply the same energy over again. He had other methods besides the simple chain, which served better to illustrate the lecture, but the principle was the same. In one there was a complete engine with a working pump. By mere agitation the bucket of the pump rose, lifting 5 lb. of water 1 foot high; before it would make another stroke the agitated medium must be cooled, *i. e.*, the energy which caused the elasticity must be taken out, then the bucket descended, and, being agitated again, made another stroke. He felt that there was a childish simplicity about these kinetic engines, which might at first raise the feeling of "Abana and Pharpar" in the minds of some of his hearers. But this would be only till they realized that it was not now attempted to make the best machine to raise the bucket, but a machine that would raise the bucket by shaking. These kinetic engines were no mere illustrations or analogy of the action of heat, but were instances of the action of the same principles. The sensible energy in the shaking rope only differed from the energy of heat in the scale from the energy of heat in a metal bar. The temperature of the bar, ascertained from absolute zero, measured the mean square of the velocity of its parts multiplied by some constant depending on the mass of these parts. So the mean square of the velocity of the links of the chain multiplied by the weight per foot of the chain, really represented the energy of visible agitation in the chain. The waves of the sea constituted a source of energy in the form of sensible agitation; but this energy could not be used to work continuously one of these kinetic machines, for exactly the same reason as the heat in the bodies at the mean temperature of the earth's surface could not be used to work heat engines. A chain attached to a ship's mast in a rough sea would become elastic with agitation, but this elasticity could not be used to raise cargo out of the hold, because it would be a constant quantity as long as the roughness of the sea lasted. Besides the waves of the sea, there was no other source of sensible agitation, so there had been no demand for kinetic engines. Had it been otherwise, they would not have been left for him to discover—or had they been, he might have been tempted to patent the inventions. But there had been a demand for what might be called sensible kinetic elasticity, to perform for sensible motion the part which heat elasticity performed in the thermometer. And it had not been left for him to invent kinetic mechanism for this purpose, although it might be that its semblance to the thermometer had not been recognized. The principle was long ago applied by Watt. The common form of governors of a steam engine acted by kinetic elasticity, which elasticity, depending on the speed at which the governors were driven, caused them to contract as the speed increased. The governor measured by contraction the velocity of the engine, while the thermometer measured by expansion the velocity in the particles of matter which surrounded it; so that it could now be seen that having to perform two operations, the one on a visible scale, the other on a molecular scale, the same class of mechanism had been unconsciously adopted in performing both operations. The purpose for which these kinetic engines was put forward was not that they might be expected to simplify the theory of thermodynamics, but that they might show what was being done. The theory of thermodynamics could be deduced from the laws of motion from any one of these kinetic engines, just as Rankine deduced it from the hypothesis of molecular vortices. Nothing had yet been said of the third part which heat played in performing work, namely, in conveying heat in and out of matter. It was an innovation to introduce such considerations into the subject of thermodynamics, but it properly had a place in the theory of heat engines. It was on this part that the speed at which an engine would perform work, depended. The kinetic machines showed this. If one end of a chain was shaken the wriggle ran along with a definite speed,

so that a definite interval must elapse before sufficient agitation was established to raise the bucket; further, an interval must elapse before the agitation could be withdrawn, so that the bucket might be lowered for another stroke. The kinetic machine, with the pump, could only work at a given rate. He could increase this rate by shaking harder, but then he expended more energy in proportion to the work done. This exactly corresponded with what went on in the steam engine, only owing to the use of separate vessels, the boiler, cylinder, and condensers, the connection was much confused. But it was clear that for every horse-power (2,000,000 foot-pounds per hour), 15,000,000 foot-pounds had to be passed from the furnace into the boiler, as out of the 15,000,000 no more than 2,000,000 could be used for work, the remaining 13,000,000 were available for forcing the heat into the boiler and out of the steam in the condenser, and they were usefully employed for this purpose. The boilers were made as small as sufficed to produce steam, and this size was determined by the internal temperatures of the gases in the furnaces, and the water in the boiler; and whatever diminished this difference would necessarily increase the size of the heating surface required, *i. e.*, the weight of the engine. The power which this difference of temperature represented could not be used in the steam engine, so it was usefully employed in diminishing the size of the engine. Most of this power, which in the steam engine was at least eight times the power used, was spent in getting the heat from the gases into the metal plates, for gas acted the part of conveyance far less readily than boiling water or condensing steam. If air had to be heated inside the boiler and cooled in the condenser with the same difference of temperature, there would be required thirty or forty times the heating surface—a conclusion which sufficiently explained why attempts to substitute hot air for steam had failed. In one respect the hot air engines had an advantage over the steam engine. During the operation in the cylinder the heat was wanted to be kept in the acting substance; this was easy with the air, for it was such a bad conductor of heat that unless it was in a violent state of internal agitation it would lose heat but slowly, although at a temperature of 1000 deg. and the cylinder cold. Steam, on the other hand, condensed so readily that the temperature of the cylinder must be kept above that of the steam. It was this fact which limited the temperature at which steam could be used. Thus, while hot air failed on account of true economy, the practical limit of the economy of steam was fixed by that which a cylinder could bear. These facts were mentioned because at the present time there appeared to be the dawn of substituting combustion engines in place of steam engines. Combustion engines, in the shape of guns, were the oldest form of heat engine. In these, the time required for heating the expansive agent was zero, while they had the advantage of incondensible gas in the cylinder, so that if the cylinder was kept cool it cooled the gas but slightly, although this was some 3000 deg. in temperature. The disadvantage of these engines was that the hot gas was not sufficiently cooled by expansion, but a considerable amount of the heat carried away might be used again could it be extracted and put into the fresh charge; to do this, however, would introduce the difficulty of heating surface in an aggravated form. However, supposing the cannon to have been tamed and coal and oxygen from the air to be used instead of gunpowder. Thermodynamics showed that such engines should still have a wide margin of economy over steam engines, besides the advantage of working with a cold cylinder and at an unlimited speed. The present achievement of the gas engine, stated to be some 2,000,000 foot-pounds per ton of coke, looked very promising, and it was thus not unimportant to notice that whatever the art difficulties might be, thermodynamics showed no barrier to further economy in this direction, such as that which appeared not far ahead of what was already accomplished with steam engines. But however this might be, he protested against the view, which seemed somewhat largely held, that the steam engine was only a semi-barbarous machine, which wasted ten times as much heat as it used—very well for those who knew no science, but only waiting until those better educated had time to turn their attention to practical matters, and then to give place

to something better. Thermodynamics showed the perfections, not the faults of the steam engine, in which all the heat was used, and could only enhance the admiration in which the work of those must be held who gave us, not only the steam engine, but the embodiment of the science of heat.

ON THE COMPARATIVE ENDURANCE OF IRON AND MILD STEEL WHEN EXPOSED TO CORROSIVE INFLUENCES.

By DAVID PHILLIPS, M. Inst. C. E.

From Proceedings of the Institution of Civil Engineers.

I.

Iron and so-called mild steel have been subjected from time to time to such elaborate mechanical tests, at the hands of so many practical men, as to render further investigation into their comparative strength and ductility almost unnecessary; but the endurance of these metals when exposed to corrosive influences, a subject scarcely second in importance to that of their strength, has been almost entirely neglected. Excepting the work of the late and the present Boiler Committees of the Admiralty, and of some government officers, there are, the author believes, the results of only a very few experiments on record.

Having regard to the numerous discussions which have taken place during the last three years respecting the materials best suited for the construction of ships, boilers, and bridges, and remembering the efforts that have been made to perfect mild steel for those purposes, it is extraordinary that the subject should have received so little attention. Even the opinions advanced by experts are conspicuous by their lack of reliable information, opinions emanating nevertheless from those who would doubtless be able to give valuable information were the question fairly treated. But recent discussions, the author thinks, have afforded evidence of partiality, mild steel finding advocates in abundance, while the question of its endurance when exposed to corrosive influences, as compared with iron, appears, from the consumer's point of view, to have been purposely neglected.

The author had the honor to serve on the committee appointed by the Admiralty in June, 1874, to inquire into the causes of corrosion in boilers, and since its dissolution he has made further experiments with the same objects in view. He trusts that his efforts to throw light on the matter may be of advantage to the profession. It will be necessary for the purpose he has in view to refer to several of the experiments made by the late Boiler Committee, as well as to his own; and it may be remarked in passing that it would have been satisfactory to all interested in engineering matters, had the results of the last twelve months of the labors of the committee, and of the experiments continued at Devonport after its dissolution, been published before this.

In this paper it is proposed to treat chiefly of the comparative durability of iron and mild steel when exposed to similar influences. The action on these metals of various kinds of water in the presence or absence of air, alkalies, &c., is too wide a subject to include, even if it had not already been treated by the Chairman of the late Boiler Committee, Rear-Admiral C. Murray Aynsley, C. B.

The first experiment to be mentioned is that referred to in the third report of the late committee, p. xi. Six sets of tubes of different brands of iron and steel, prepared for the purpose, were tested in a special apparatus in Sheerness dockyard, with the view of ascertaining their comparative durability. The sets were numbered from 1 to 6, but as some of the tubes became unfit for testing, sets 1 and 6 will be only incidentally referred to. The remaining four sets con-

sisted severally of tubes of one of each of the following brands, viz.:

A.I. Lloyd and Lloyd's improved metal (iron.)
B.W. Whitworth's compressed steel.
B.F. Fagersta Bessemer steel.
C. Bolton Iron and Steel Co.'s Bessemer steel.
D. Crampton's thrice-hammered iron.
E. Lloyd and Lloyd's improved homogeneous metal (iron.)
F.F. Firth and Sons' crucible steel.
F.S. Ordinary iron (not specially prepared.)
G. Cammell and Co.'s special steel.

Sets 2 and 3 had each in addition a Yorkshire iron tube (A.Y. 15 and 19) thus making the total number of tubes thirty-eight, eighteen being of iron and twenty of steel. The Whitworth, Fagersta, and Firth's steel, and the Crampton iron tubes, were cold-drawn, the others were welded. The cold-drawn tubes had originally a smooth and clean surface, with only a film of oxide, the result of annealing, whilst the welded tubes, especially those of iron, were rather rough, with the usual coating of oxide. This would be in favor of the steels in calculating the losses of weight. All the tubes were carefully weighed before and after each experiment, and were 8 feet in length and $2\frac{1}{2}$ inches in diameter, representing an exposed surface inside and out of 9.58 square feet, with the exception of the ordinary iron tubes (Fs), which were 2 inches in diameter, and had 7.68 square feet of exposed surface.

The testing apparatus consisted of two cylinders, 3 feet 7 inches in diameter, and 6 feet in height, the tubes being screwed tightly through the top ends.

TABLE I.

	Kind of Water, Alkali, &c., in Tubes,				Loss of Weight.			
	Set 2. $\frac{2}{32}$ Density and 500 grs. of Lime; air admitted weekly.	Set 3. Sheerness Well, no Alkali; Air admitted weekly.	Set 4. Rain, and 500 grs. of Soda; Air excluded.	Set 5. Distilled from Sea Water, no Alkali, Air excluded.				
Tubes.	Loss of Weight.	Loss of Weight.	Loss of Weight.	Loss of Weight.	Total Loss of Weight.	Mean Loss per Tube.	Average Loss per Sq. Ft. of Surface.	Average Loss per Square Foot of Surface.
	Oz.	Oz.	Oz.	Oz.	Oz.	Oz.	Oz.	Grs.
Steel B.W...	21.25	28.00	20.25	11.25	80.75	20.19	2.1072	921.90
" B.F..	24.00	27.25	15.75	12.50	79.50	19.87	2.0746	907.63
" C......	23.75	23.00	22.00	13.25	82.00	20.50	2.1398	936.16
" F.F...	21.25	16.50	17.00	18.25	73.00	18.25	1.9050	833.43
" G.....	20.25	16.25	18.75	16.50	71.75	17.94	1.8723	819.13
Total loss...	110.50	111.00	93.75	71.75	387.00	..	..	169,312.50
Mean loss...	22.10	22.20	18.75	14.33	19.35	..	2.0197	883.66
Iron A.I....	12.75	11.75	14.50	13.25	52.25	13.06	1.3635	596.53
" D.....	15.50	16.00	17.25	12.25	61.00	15.25	1.5918	696.41
" E.	14.00	17.25	25.25	12.00	68.50	17.12	1.7876	782.07
" F.S....	6.00	7.00	5.00	3.75	22.25	5.56	0.7113	311.19
" A.Y....	14.25	11.50	..	..	25.75	12.87	1.3440	588.00
Total loss...	62.50	63.50	62 50	41.25	229.75	..	..	100,515.62
Mean loss...	12.50	12.70	15.62	10.31	12.77	..	1.3890	607.71

The lower parts of the tubes, 5 feet 8 inches in length, thus confined in the cylinders and filled with various kinds of water, were exposed during the working hours in the yard to an average steam pressure of 50 lbs. per square inch, the steam being supplied from one of the factory boilers close by; whilst the upper parts, representing, as it were, the steam spaces of boilers, were inclosed in a chamber open to the air, and supplied with sufficient cold water to insure partial condensation of the steam within. The average steam pressure in the tubes was thus kept at about 38 lbs. per square inch.

In table I were the conditions of working, and the losses of weight of the tubes after a little more than two years' trial.

Taking all the four sets, representing 191.6 square feet of steel, and 165.4 square feet of iron, the results are as follows:

Table II.

No. of tubes.	Metal.	Total loss of weight.	Average loss per sq. ft. of surface.	Percentage in favor of irons.
		Oz.	Grs.	
20	Steels	387.00	883.66	45.4
18	Irons	229.75	607.71	

Taking sets 2 and 3 only, representing 95.8 square feet of steel, and 92.28 square feet of iron, the following is the result:

Table III.

No. of tubes.	Metal.	Total loss of weight.	Average loss per sq. ft. of surface.	Percentage in favor of irons.
		Oz.	Grs.	
10	Steels	221.5	1,010.50	69.2
10	Irons	126.0	597.36	

The tubes in each set were under precisely similar treatment; consequently the loss of weight of each of the different brands is a fair criterion of their comparative durability.

Illustrations are exhibited of gutta-percha impressions taken from parts of some of the tubes at different levels. They give a fair idea of the nature, as well as of the severity, of the corrosion. The specimens are arranged in order, according to the severity of the corrosion, as deduced from the examination of their interior surfaces, but in weight the ordinary iron lost the least, and the Fagersta and the Whitworth steels the most; and they represent respectively set 2; the exterior suface at about the water-level in the condensing chamber, the interior surface in the steam space, at the water-level, and the bottom ends of the tubes.

The Barrow steel tubes, H 2 and 3, were tested only half as long as the others; taking this into account, it will be seen by the impressions that the corrosion was exceptionally severe. The group marked A and G represents set 4: and that marked Fs and Bw, set 5. The A and Fs were the least, and the G and Bw the most affected of the two last sets, which compare favorably with sets 2 and 3. The impressions in the center of the top row were taken from the interior surfaces of Ay and I tubes at the water-line and at the bottom ends. Only two of these tubes, 15 and 19, were under similar conditions to sets 2 and 3, and are included in the four sets before mentioned.

Small disks of iron and steel of various brands were also tested in another group of tubes in the apparatus. Impressions taken from these, and from some pieces of the same metals, which were tried in boilers at Devonport, are likewise illustrated. Full particulars of these experiments will be found in the third report of the committee, appendices G and H, pp. 267-268, and 281-284.

All the specimens, tried in boilers, from which the impressions were taken, had originally bright surfaces. The top row represents the disks in contact with zinc; the second row those with no zinc present.

There was a striking difference between the iron and steel disks, and also between the several steels as regards the nature and severity of the corrosion. While the disk O (steel) was very uni-

formly affected, the disk V was deeply pitted; yet the loss of weight in the latter was 19 grains less than in the former. The O 2 from the same plate as the O disk was much rougher, while the loss of weight was 18.8 grains less. The A disk (steel) was peculiarly affected, being pitted in small deep holes, whilst the intervening surface was scarcely touched. Again, the edges of some of the disks were corroded uniformly, whilst others were affected in a most extraordinary manner, especially the R, A, and Y steel disks. The edges of the Y iron disks were corroded in ridges and grooves, showing plainly that the bloom had been made up by ordinary piling, and that the piles could not have been of uniform quality.

The metals in this experiment were doubtless exposed to a severe test. They were suspended in copper tubes, filled with water of $\frac{2}{32}$ density, with tallow as a lubricant, air being admitted weekly. The duration of the experiment was six months only.

Impressions taken from pieces of iron and steel of various brands, which were suspended for twelve months in one of the boilers of the Trusty, a tug having a jet condenser, and worked at a low pressure, are also shown. The bottom row represents the set that was in the water, and the other the duplicate set that was in the steam space. These pieces were cut from plates which were subjected to cold bending and other tests, as described in the Reports of the Boiler Committee. Excepting the edges, the surfaces were rough. The Whitworth compressed and the Siemens' steels were not included in this group, on account of the delay in their production. In this experiment again, the loss of weight in the steels, and the nature of the oxidation were remarkable. The percentage in favor of the irons was 32.7; in the disk experiment it was 56.7.

The same set of plates, with others of Whitworth's, Simens', and Attwood's steels added, were tried for another twelve months in a feed-water heater supplied with fresh water, and a duplicate set in one of the boilers of the Perseverance, a tug worked at a low pressure and fitted with surface condensers. The surfaces of the specimens had all been made bright. The percentage in favor of the irons was 27.5 in the tug, and 11.8 in the feed-water heater. The pieces of metal tested in these experiments were very small, but the results are not without significance.

Plates of Bolton steel and Lowmoor iron, 10 inches long by 8 inches wide, by $\frac{9}{16}$ and $\frac{7}{16}$ inch thick respectively, were also placed in the boilers of the two tugs before mentioned. They were suspended with the view of ascertaining the comparative effects of fastening together plates of steel and iron with brass and with iron stay; and also the action of zinc on two plates, steel and iron, connected together, when attached to only one of them. Eight plates of each metal, representing 9.9 square feet of steel, and 9.68 square feet of iron, were tested. The result from one-half of the specimens after thirteen months' trial was as follows:

Table IV.

No. of plates.	Metal.	Total loss of weight.	Average loss per sq. ft. of surface.	Percentage in favor of irons.
		Oz. Grs.	Grs.	
4	Steels	7 209.3	659.63	32.7
4	Irons	5 218.7	497.14	

The corrosion in the steel plates, in the form of "pitting," was so marked, and the difference between the plates from the Trusty and from the Perseverance so great, that all these eight plates were forwarded to the Admiralty for inspection. The result of the trial of the other eight, four for twenty-one and four for twenty-two months, was as follows:

Table V.

No. of plates.	Metal.	Total loss of weight.	Average loss per sq. ft. of surface.	Percentage in favor of irons.
		Oz. Grs.	Grs.	
4	Steels	10 382.9	959.25	28.6
4	Irons	8 109.4	745.74	

The impressions taken from one-half of these plates, though slightly distorted, give a fair idea of the corrosion sustained by the two metals. N 5 was taken from one of the steel, and D D 6 from one of the iron plates.

Plates of the same metals, 15 inches long by 8 inches wide by ½ inch and ⅜ inch thick respectively, was also tested in the feed-water heater before mentioned.

The result after thirteen months was:

TABLE VI.

No. of plates.	Metal.	Total loss of weight.	Average loss per sq. ft. of surface.	Percentage in favor of irons.
		Oz. Grs.	Grs.	
2	Steels	19 327.9	2,453.9	10.9
2	Irons	17 218.7	2,212.2	

The impressions were only taken from a part of each of the plates in consequence of their size; but they show fairly well the nature of the extraordinary corrosion which occurred. The corrosion in the steel plates was only a little more marked and irregular than in the iron. Impressions 5 and 6 were taken from the tieplate, and 7 and 8 from the shell of a small steel boiler doing harbor duty at Chatham, which show very deep pitting. One group of impressions represents plates of Lowmoor iron and of Landore steel, 4 inches square by ⅜ inch thick, which were suspended in pairs in vessels under slightly different conditions for a period of six months. Those marked 3 were in the same feed-water heater as the last-named plates. The result was:

TABLE VII.

No. of plates.	Metal.	Total loss of weight.	Average loss per sq. ft. of surface.	Percentage in favor of irons.
		Oz. Grs.	Grs.	
4	Steels	1 85.0	506.24	4.8 nearly
4	Irons	1 73.5	483 17	

The only peculiarity worth noticing in this experiment is that, while the two plates in the feed-water heater lost 381.8 and 394.2 grains, the two in the boiler fed from the heater lost only 8.0 and 3.4 grains respectively. In a former experiment, when iron only was placed in the feed-heater and boiler in question, the plate in the heater lost 405 grains, whilst that in the boiler lost during thirteen months only 20 grains. The heater was an open vessel, and the boiler was worked at a pressure of about 55 lbs. to the square inch.

The next experiments to be mentioned are a series made with iron and steel plates, 4 inches square by ⅜ inch thick, suspended in the boilers of different ocean and coast-going steam vessels belonging to various ship owners.

The results are interesting from more points of view than one; but in this paper they will be treated chiefly as affecting the subject under consideration, the effects of the different treatments they experienced having been already dealt with by Admiral Aynsley. However different the treatments, and whatever the effects, it should be remembered that the five plates, all of different metals, of which each set consisted, were always under precisely similar conditions, whether in or out of the boiler. The exposed surface of each plate was 37.89 square inches, and the aggregate surface of each kind of metal in the fifty-six sets which have come under the author's notice was 14.74 square feet, or 44.22 square feet of steels, and 29.48 square feet of irons. The five metals were crucible, Bessemer, and Siemens steels, and B B Staffordshire and best Yorkshire irons. The periods of trial varied from five months to two and a half years.

The gross results are given in table VIII.

The impressions taken from a few of these sets are shown, and are interesting as illustrating the character of the corrosion caused by a change of conditions, such as the waters used, different practices of blowing off, &c., as well as the comparative effects on the different metals.

To venture a little beyond the subject of the paper, it may be observed that the difference in the condition of some

TABLE VIII.

Number of plates.	Metal.		Total loss of weight.		Average loss per square foot of surface.	Percentage in favor of the irons.
			Oz.	Grs.	Grs.	
56	J.	Crucible Steel......	97	70.8	2,885.83	14.3 good.
56	N.	Bessemer "	118	229.4	3,520.32	Or over.
56	Y.	Siemens "	116	166.8	3,456.67	N and Y only (mild steels).
56	B.B.	Staffordshire Iron..	92	163.0	2,743.58	21.3 good.
56	D.D.	Yorkshire " ..	101	115.7	3,007.68	..

of the sets is remarkable. For example, in sets 2 and 43 the character of the corrosion was quite different; and whilst the mean loss of weight of set 43, after being in the boiler two hundred and eighty-five days, was 347.7 grains, that of 2, after being in the boiler three hundred and eleven days, was 825.4 grains, or per day as 1 to 2.1.

The boiler in which set 43 was suspended was filled fourteen times with fresh water, 2 inches of water being blown off daily, whilst that in which set 2 was hung was filled eight times with fresh and five times with sea water, 3 inches of water being blown off daily at sea, and 12 inches at intermediate ports.

A limited quantity of zinc was certainly present, though only suspended loosely, in the boiler with set 43, and to this its better condition may perhaps be attributed; but on comparing set 62 with 43, it would appear that the zinc had no influence on the plates.

Boiler 62 was employed in the same trade as boiler 2, namely, India and China. The plates were in the boiler two hundred and ninety-eight days, during which period it was filled five times with fresh water, 1 inch being blown off daily, and no zinc present, yet the mean loss of weight was only 364.9 grains as compared with 825.4 grains in set 2 ; or per day as 1 to 2.1.

Again, comparing set 84 with 62, the bad effects of much blowing off are manifested. Set 84 was only forty-three days in the boiler, which was filled during that period four times with sea and once with fresh water, whilst, extraordinary as it may seem, an average of 73 inches of water ws blown off daily. This, with the waste from other sources, was made up from the sea. The mean loss of weight in this set was 575.7 grains, as against 364.9 grains in set 62, being at the rate, per month of thirty days, of 401.7 grains in the former against 36.7 grains in the latter, or nearly twelve times as much.

As the boilers in these cases were fed from surface condensers and worked at a high pressure, it may be of interest to compare the results with those obtained from two jet-condensing low-pressure boilers, though this type is almost a thing of the past. Referring then to set 9, and set 63, the mean loss in each was practically the same, viz., 1,332 and 1,333.8 grains. Set 63 was one hundred and seventy-four days in the boiler; there is no record of the time that set 9 was in the boiler, but the total time was nearly the same as for the former set.

Comparing these results with those obtained from set 2, which was subject to the old, or perhaps the ordinary surface-condensing treatment, the loss from corrosion was nearly three times as much in the jet-condensing boilers, or per day as 2.8 to 1. Again, comparing these two sets, 9 and 63, with set 62, which was subjected to something like the most improved method of working, the loss in jet-condensing boilers was about six times that in the boiler with surface-condensers, namely a mean loss of 1,332.9 grains as compared with 364.9 grains, or per day as 6.2 to 1.

These results clearly prove that the conclusions arrived at by many experienced engineers and chemists as to the causes of corrosion in boilers, previous to the appointment of the Boiler Committee, were erroneous; but the conclusions are nevertheless still believed in to

TABLE

Number of Set.	Metal.	Water.	Loss of weight. First twelve months.	Second twelve months.	Total.	Average pr. sq. ft. of surf'ce	Percentage in favor of the—
			Grs.	Grs.	Grs.	Grs.	
98	N. Bessemer steel.....	Rain water.	186.7	141.4	328.1	1,246.9	Irons = 7.2, including J,† 9.4 nearly
	Y. Siemens steel.....		174.1	147.0	321.1	1,220 3	
	B.B. Staffordshire iron		165.3	119.0	284.3	1,080.5	
	D.D. Yorkshire iron. ...		185.1	136.2	321.3	1,221.1	
99	N. Bessemer steel.....	Sea water.	42.4	36.9	79.3	301.4	Iron = 5.7
	Y. Siemens steel.....		33.5	34.7	68.2	259.2	
	B.B. Staffordshire iron		35.4	35.6	71.0	269.8	
	D.D. Yorkshire iron.....		36.9	31.6	68.5	260.3	
100	N. Bessemer steel.....	Exposed to weather and dipped in sea water daily.	1,044.7	501.6	1,545.6	5,874.0	Iron = 128.3
	B.B. Staffordshire iron		417.9	259.1	677.0	2,572.9	
	Y. Siemens steel.....	Exposed to weather only	234.4	135.9	370 3	1,407.3	Iron = 84.8
	D.D. Yorkshire iron		147.6	52.7	200.0	761.2	
101	N. Bessemer steel.....	Exposed to weather and dip'd in fresh water daily.	227.9	..	..	866.1	Iron=169.7
	B.B. Staffordshire iron		84.5	..	..	321.1	
	Y. Siemens steel.....	In kitchen tank along with J. 99.	134.5	..	..	511.2	Iron=7.4 good
	D.D. Yorkshire iron.....		125.2	..	..	475.8	
102	N. Bessemer steel.....	Rain water direct from the clouds filtered.	28.5	..	..	108.3	Steels=20 0
	Y. Siemens steel.....		28 8	..	..	109.5	
	B.B. Staffordshire iron		30.7	..	..	116.7	
	D.D. Yorkshire iron*....		38.1	..	..	144.8	
†98	J. Crucible steel†....	Rain in water butt.	198.4	146.2	344.6	1,309.6	99 over 98 =20.4
99	J. Crucible steel.....	Same water in kitchen tank.	166.3	119.8	286.1	1,087 3	100 over 98 =52.1 good
100	J. Crucible steel.....	Same water in kitchen boiler.	118.9	107.6	226.5	860.8	100 over 99 =26.3 good

* Cinder removed after the plate was weighed and included in the loss given.

some extent, and the mode of working consequent thereon followed.

The author will now describe the experiments he made with five sets of

IX.

Remarks on the appearance of the surfaces before testing.	Remarks on the appearance of the surfaces after testing.
Clean and bright all over......	Slightly pitted in small spots, and corroded in large patches, the Y. being the least and more generally corroded of the two, but not very much discolored.
Very dirty on one face........	Less affected and more generally of any of the set.
Slightly dirty on both faces (cinder)..................	Much the same as Y., but more generally affected of the two, and only slightly discolored.
Clean and bright all over......	Scarcely at all affected, except at top corners, which are black; the Y. is slightly touched under the hole on one face; both are nearly bright otherwise.
Very dirty on one face........	Top corners of both black; one face of the B.B. slightly touched under the hole; crystalline appearance, but finer than D.D.; otherwise both appear dirty.
Slightly dirty on both faces (cinder)......	
Clean all over................	Very rough and deeply marked all over.
Slightly dirty.................	Very much less marked; no comparison between the two.
Clean all over................	Slightly marked all over, the Y. being more affected under the hole on both sides; generally the difference is not much.
Slightly dirty all over.........	
Clean all over................	Marked all over; slight at top, increasing in severity downwards in both; but much more severe in N. than B.B.; the latter has a few very minute bright specks on one face after cleaning (something like minute blisters).
One face very dirty; the other better..............	
Clean all over................	Corroded in patches, which are very local in both; nearly half the surface scarcely at all affected. Slightly more locally affected and marked in the Y., but very similar to the J. 99.
One face very dirty; the other one half only.......	
Perfect all over.....	Very slightly affected along the top edges, the corners are black, the rest being nearly bright.
Very dirty over; laminated in one corner. One face slightly dirty; the other very dirty. Cinder picked out and impression taken of it..............	These are a little more marked, due no doubt to cinder; surface generally crystalline, the B.B. having bright spots much more prominent than 99, projecting in the form of minute blisters above the surface.
Clean all over..	Corroded similarly to N. and Y. 98, but slightly more marked.
	Similarly affected to the last, but a little more local.
	Generally unaffected, but black, with small patches of pitting, and pin-holes here and there. Bottom edge on both sides severely affected for three-eight inch up, with a few deep pit-holes on edges.

† Comparable with the other 98 specimens.

plates, similar to those tested in ocean steamers. The conditions to which some of these plates were subjected, and the results obtained, are scarcely, if at all,

less important than those of the experiments last described. They are given in table 9 (see pages 134 and 135). To avoid even a suspicion that galvanic action had any influence in these cases, all the plates were suspended on glass rods, and each plate was separated from its neighbors by glass ferrules. Sets 98, 99, and 100 were under test for two years; sets 101 and 102 for only half that period. During the first twelve months set 98 lost considerably more than 99, and as this might have been due to the action of soot brought down from the roof, or of the lead of the water-butt, or both, set 102 was suspended in an earthenware pan similar to that containing 99, but filled with rain water direct from the clouds, and filtered.

Taking the aggregate losses of the irons and steel, omitting the crucible steels, the result was as follows:

TABLE X.

No. of plates.	Metal.	Total loss of weight.	Average loss per square foot of surface.	Percentage in favor of irons.
		Oz. Grs.	Grs.	
10	Steels	7 69.8	1190 41	64 8 nearly
10	Irons	4 150.6	722.42	

Impressions taken from these sets of plates, after twelve months' exposure, and at the end of two years, are shown. There is little to remark concerning the results, except that the corrosion is strikingly local and severe in set 98, which was placed in rain water; also that wetting the metals daily, especially with sea water, and exposing them to the weather, causes very severe corrosion, which the irons, as shown by the table, resisted much better than the steels.

The crucible steels illustrate further the action on metals of the same sort of water under slightly different conditions: The specimen J 98 was in a butt which received water direct from the roof, 89 was in a small tank in the kitchen, supplied by hand with water from the butt, and 100 in a small boiler supplied with water from the tank, for culinary purposes.

In years gone by the rapid and sometimes sudden deterioration of boilers was attributed to galvanic action, supposed to be set up between the copper or brass tubes of the condensers and the iron of the boilers. Although this theory may be regarded as obsolete, others equally speculative have taken its place. Galvanic action is now said to be set up between steel and steel, or iron and iron, if they differ in the slightest degree in composition; between metals and the cinders too frequently found unfortunately pressed into them by the rolls; but especially between iron and steel, and between the metals composing boilers and the oxides with which they are coated. The galvanometer is a very sensitive instrument, and would, no doubt, show whether one metal were negative to another, or whether such differences existed between metals and their oxides as would produce galvanic action; but practically these theories are in the author's opinion unworthy of much consideration. They are advanced by the ardent advocates of mild steel, and, as the author thinks, are only a cloak to cover the too indiscriminate advocacy of that material.

It is easy to say that the purer classes of metals ought to offer greater resistance to corrosion than the impure kinds. Accepting the theories advanced by the advocates of mild steel, this is undoubtedly a necessary conclusion; for all classes of iron, and especially the commoner brands, have invariably a thicker coating of oxide than the steels, and moreover have the great drawbacks, cinders and laminations, which it is professed are unknown in steel. But in spite of these reasonings, it is undoubtedly a fact, that under almost all circumstances iron, and particularly the harder classes, is far superior to the finer steels in its resistance to corrosion, and this the experiments described by the author incontestably prove.

As regards uniformity of composition, and temper also, steel has probably more than its fair share of praise. Laminations and cinders are undoubtedly the great objection to iron as now made, and this is well illustrated in the Cʏ group of impressions taken from one of the tubes tested. The center impression shows the inner layer of a blister corroded

through, and a large cavity thus formed in the tube; the one just above shows a blister punctured by a sharp-pointed instrument, while that below shows blisters untouched. It will be seen from these how easily original defects in the manufacture of tubes or plates, such as cinder, may be mistaken for what is called "pitting."

In these respects mild steel is no doubt much superior to iron, but that it is not without original defects, such as want of homogeneity and uniformity of temper, will be seen from another series of impressions. Here in some of the Y and J plates, there was to all appearance a small hard spot, which seems like a small pit surrounded by a slight ring. It should be borne in mind that these marks in the impressions appear the reverse of reality, the pits being spots much less acted upon, and the rings surrounding them grooves much more acted upon, than the rest of the surface of the plates.

Another plate (also Y), placed in the feed-water heater before referred to, had a spot, about $\frac{3}{8}$ inch in diameter, untouched by corrosion. After the plate had been six months under water the spot was quite bright, had its edge well defined, and was surrounded by a very slight groove; but the corrosion generally was trifling in this plate.

Some of the steel disks and tubes, especially the Whitworth's and Firth's, presented after testing a damaskeen appearance, being marked, very similarly to gun-barrels, with slightly and beautifully formed ridges and grooves.

Want of uniformity on the part of the steels will also be seen in Table 1 in the appendix, giving the results of the cold-bending test of the metals tested in the tube apparatus. Although three out of the five brands of steel had stood the severe test of being drawn cold from blooms, with only a small hole drilled through the center, there were surprising differences of temper, not only between the various brands, but also between the different tubes of each brand. Some of them, especially the Firth's, Barrow and Whitworth's, split up in various directions, and broke in several pieces whilst going through the ordeal of cutting open longitudinally, for the inspection of their interior surfaces, whilst others of the same brands showed scarcely any distress. It was on account of this that strips were cut from the tubes and subjected to the tempering test.

On comparing also the behavior of the tempered with that of the annealed specimens under the cold-bending test, the results are unsatisfactory. For whilst three of the metals after annealing were bent, doubled and then hammered flat, without exhibing any but the slightest signs of distress, after tempering the same metals only stood bending to the following angles before showing distress, viz., four strips from the Bf tubes to 35°, 34°, 11°, and 5°, the last two then breaking; three strips from the C tubes to 74°, 72° and 36°; and four from the Ff tubes to 12°, 11°, 9°, and 0°, the last breaking suddenly.

The other three steels stood the test as follows. after annealing. The G steel was bent to a semicircle at the crown of $\frac{1}{2}$-inch in diameter, when half of it broke through, the other half remaining perfect. The Bw was bent to an angle of 147°, when it began to yield in the center of the crown, the edges being perfect. The H was bent to a semicircle $\frac{5}{16}$ inch in diameter at the crown, when it broke half through. After tempering, the same metals only stood bending as follows: Five Bw strips to 11°, 8°, 6°, 4°, and 0°, all being broken through; four G strips to 40°, 16°, 0°, 0°, the last two breaking suddenly. The two H strips also broke without bending.

Of all the six brands, the Bw were the most uniform in temper and in grain of fracture, though hard; the C being the softest, while the Bf, Ff and G showed the greatest contrasts, especially in the appearance of their fractures. The Bf and Ff varied from fine to very coarse in grain, one edge of each being much coarser than the other. One of the G's was ductile and the fracture rather silky in appearance, the next was hard and fine in grain, the third hard but coarse in grain, and the fourth fine and silky at both edges, but laminated and dirty in the center. The annealed strip of the G was also remarkable; for, while it broke through halfway across long before the crown was hammered flat, the other half showed no signs of distress. In another case a strip of Bf broke after bending

to 34°, when it exhibited a very coarse fracture; a second strip, cut from the same tube, bent only to 18°, and showed a fine steely fracture. It is certainly possible that these great contrasts may to some extent be due to want of care in the manipulation of the metals, and not entirely to want of uniformity in their composition; but it must be admitted, on the other hand, that such metals as will harden considerably when only moderately heated and plunged into water of ordinary temperature, are unfit for boilers, especially for furnaces and combustion chambers. It should also be remembered, that the tubes in question were made specially for testing, and it is therefore fair to suppose that more than ordinary care was taken to supply good material. In that case, what could be expected from tubes or plates supplied wholesale?

It is important to ascertain how far want of uniformity in composition has to do with local corrosion in metals, and particularly in steel; also how far the presence in a medium degree, or absence in a minimum degree, of impurities in iron and steel can affect their durability.

It may be observed that local corrosion cannot have been caused in any of the specimens of metals dealt with in this paper by the imperfect adhesion of their oxides, as, with the exception of the small pieces of plate tried in the Trusty, and the welded tubes, all the specimens were either planed, filed, or ground bright all over.

It has often struck the author that the manufacturer and the chemist, in their anxiety to produce a metal containing the least possible amount of impurities, and thus to attain a high standard of ductility, in depriving it, perhaps to a greater degree than necessary, of elements such as phosphorus, carbon, &c., or adding to it manganese, probably thus render it more liable to corrosion. If the metals tested in sea-going boilers be compared (the results are given in Table VIII.), it will be found that the ordinary BB Staffordshire iron has a percentage in its favor of 9.6 over the best Yorkshire iron, and that the harder steel, J, is 20.9 per cent. better than the two mild steels, N and Y. Comparing the two irons with the two mild steels there is a difference of 21.3 per cent. in favor of the irons. On the other hand, it should be mentioned that in one or two cases the harder metals suffered considerably more than the softer metals in sea water; but generally the reverse was the case. In the irons laminations and cinder, no doubt, caused these exceptions, but in the steels they were in all likelihood due to want of homogeneity.

It will not be out of place to give the results of the trial of three of these sets of plates to show how great the differences were between the loss sustained by one or more of the metals compared with others of the same set (see Table XI.)

The thirty-first set of plates was in one of the boilers of the Duke of Sutherland, a Holyhead boat belonging to the London and North-Western Railway Company; the time occupied was the longest of all the ocean experiments, namely, two and a half years. This vessel has two iron and two steel boilers; and as they are about five years old, and as the steel was manufactured at the company's works at Crewe, it would be interesting to know how the steel boilers now compare with the iron boilers.

In set 78 the Yorkshire iron DD shows a loss a little over twice that of the hard steel J; but in this instance the iron indicated plainly that a thin layer at the edge of one side, no doubt the result of lamination, had got disengaged, or corroded through. Again, in set 91 the hard steel J lost nearly 35 per cent. more than the mild steel N, and the soft iron DD lost 26.3 per cent. more than the hard iron BB.

The tensile test of the irons and steels supplied by the firms from whom these plates were obtained are given in the appendix, Table 2. Though the pieces tested were not from the same plates as some of those tried in the boilers, the results, coupled with those since supplied, furnish a fair idea of the softness or ductility of those metals, and may bear some relation to the losses sustained by them.

Turning again to the results of the tube experiments given in Tables I., II., and III., it will be observed that the four Fs ordinary tubes only lost 311.19 grains per square foot of surface, whilst the twelve, consisting of A_1, D and E

TABLE XI.

Number of sets.	Letter marked with, and kind of metal.	Total loss of weight.	Average loss per square foot of surface.	Percentage in favor of the—
		Grs.	Grs.	
31	J. Crucible steel......	2,027.5	7,704.5	
31	N. Bessemer "	2,630.2	9,994.7	
31	Y. Siemens "	2,437.3	9,261.7	Irons = 104.3
31	B.B. Staffordshire iron..	1,085 0	4,123.0	
31	D.D. Yorkshire " ..	1,230.1	4,674.3	
78	J. Crucible steel......	657.9	2,500.0	
78	N. Bessemer "	839.4	3,189.7	
78	Y. Siemens "	718.1	2,728.7	Steels = 51.4
78	B.B. Staffordshire iron..	891.6	3,388.0	
78	D.D. Yorkshire " ..	1,344.6	5,109.4	
91	J. Crucible steel......	124.5	473.1	
91	N. Bessemer "	92.3	350.7	
91	Y. Siemens "	111.6	424.0	Irons = 18.5
91	B.B. Staffordshire iron..	81.6	310.1	
91	D.D. Yorkshire " ..	103 1	391.7	

tubes, specially prepared, lost 691.68 grains, the difference being 122.2 per cent. in favor of the former. Again, whilst the four A 1 tubes named "Improved metal" lost 596.53 grains per square foot of surface, the E tubes called "Improved Homogeneous Metal," by far the most ductile and expensive of the two, and made by the same firm, lost 782.07 grains per square foot of surface; the difference in favor of the coarser metal being 31.1 per cent., the conditions of working being the same. On looking at the results of the cold-bending test of the "Improved Metal"—the coarser of the two last-named—it appears that while two of the pieces were doubled and hammered together flat without showing any signs of distress, the third began to give way when bent to 26° only. So great was the difference that it was hard to believe that the tubes were manufactured by the same firm and as of one brand.

Recent analyses of some of the brands of metal under consideration confirm the conclusions the author has drawn from the results of these experiments, viz., that the commoner sorts of iron, containing the most phosphorus, resist corrosion far better than the superior kinds; and also that the harder steels, containing the greatest amount of carbon and phosphorus, are better in this respect than the softer and finer sorts.

In the cruder classes of iron the percentage of phosphorus appears to range from 0.20 to 0.21, while in the better sorts it ranges from 0.07 to 0.14 In the milder steels it varies from 0.16 to 0.04 only. The percentage of carbon appears to be much about the same in all irons, varying only from 0.0545 to 0.074, while in the mild steels it ranges from 0.131 to 0.273. From 0.0649 to 0.1080 per cent. of manganese is found in the irons, and from 0.238 to as much as 0.75 per cent. in the steels.

Taking three brands of iron and three of steel, it appears that while the total amount of carbon in 3 tons of the irons, 1 ton of each brand, amounts to only 4 lbs. 1½ oz., in a similar quantity of the steels it amounts to 14 lbs.; and that while 3 tons of the irons contain 10 lbs. 1½ oz. of phosphorus, the same quantity of the steels contains only 2 lbs. 0¾ oz.

It would seem, therefore, that much yet remains to be done, both by the manufacturer and the chemist, in order to produce a metal possessing strength and ductility, but at the same time much better able than the present mild steel to resist corrosion. On the other hand,

the treatment of boilers might be so modified, especially with the aid of zinc properly applied, that the purer metals might be used for their construction without suffering the severe corrosion to which they are now liable.

With regard to the experiments quoted in this paper, it may be said that more definite results would have been obtained from more extensive trials; but to this it may be replied, that all experiments in which the conditions are not precisely similar cannot be considered satisfactory. Even when a steel boiler is worked side by side with an iron boiler, and is treated as nearly as possible in the same manner, causes arise, and will arise, to make the conditions somewhat different and the results unsatisfactory. And again, there is the difficulty in such matters of obtaining reliable evidence from mere observations, however carefully and honestly they may be made. Therefore it is contended that for cases such as have here been dealt with, and especially for short periods, the weighing scale is by far the most truthful means of ascertaining results.

The author has made a point in this paper of giving facts, and not indulging in assumptions and theories; in discussions on this important subject, there has hitherto been too great a tendency to follow up fanciful opinions, and to disregard ascertained facts.

Before concluding, mention may be made of the composite boilers constructed by the Admiralty in accordance with the recommendations of the late Boiler Committee. The shells of these boilers are of alternate rings of iron and steel, and in each there is a steel and an iron furnace. In one boiler the front end and the front tube-plate are of iron, and the back tube-plate of steel; in the other boiler the reverse is the case. Two sets of two each were made, but unfortunately much delay ensued before they were sent to sea. It will be admitted, setting aside the galvanic action theory, that this is a thoroughly practical experiment, and well worthy of careful attention. Perhaps it would have been still better if the iron shell-plates had been of a brand in ordinary use, and not of the best Yorkshire iron. The furnaces and combustion chambers would have been quite sufficient to test the latter metal against steel. Two of these boilers have now been about three years in commission, and it would be interesting to know their present condition.

For the courtesy always shown by the Engineer-in-Chief of Her Majesty's Navy, and the assistance rendered by the Secretary to the Boiler Committee to the author in the preparation of this paper, especially with regard to the ocean-plate experiments, he takes this opportunity of tendering his best thanks.

SANITARY DEFECTS IN HOUSES.

From "The Building News."

At the Parkes Museum of Hygiene, Dr. Charles Kelly, of Worthing, delivered an address upon the connection between zymotic disease and the sanitary defects in houses. He began by saying that the sanitary defects in houses which caused disease were those which permitted sewer gas to pollute the air or water of a house. Taking his instance from a south coast town, the author traced outbreaks of various zymotic diseases that had occurred during the last few years, and showed that although many of them were due to the milk or water supply some could undoubtedly be attributed to sewer gas, which found its way into houses where the sewers were unventilated. In some cases the ventilation of sewers had proved the cause of evil by the sewer gas escaping from the street ventilators, especially where the outfall of the sewer was tide-locked. This was shown by outbreaks occurring directly a new system of ventilated sewers had been laid down in a place where they had previously been without either regular house drains or sewers. The lecturer gave instances of enteric fever arising from polluted water, which had received the pollution from one person. In these

cases the houses were well ventilated, had a good soil, and the drainage was good; the water supply was the sole cause of nearly 30 persons having the fever. Another outbreak of fever occurred in a place with all sanitary requirements, with the exception that there were openings from the sewers, and those exposed to the air from these openings had enteric fever. This fever, he said, apeared sometimes to arise without imported cases. Some persons thought that there must be an imported case when the fever broke out, but careful inquiry would show that it frequently broke out without there being any prior case. It did not come so much to cottages as to the first-class houses, where the drain pipes were brought into the houses, and hence it was that the people who lived in cottages did not have this fever so much as those who lived in pretentious houses. Though not thought to be catching, yet, treated in close houses, it undoubtedly did spread. Outbreaks of scarlet fever in connection with defective houses exposed to sewer gas had frequently occurred. Diphtheria, again, was a disease which seemed to be in some cases due to sewer gas; but he had little doubt that cold and damp were predisposing causes in many cases, if not the origin. Besides the more severe forms of disease, there were other disorders due to sanitary defects which made little or no impression upon the death rate, but yet which affected the general health. Persons exposed to sewer gas in a dilute form were pale and anæmic; they often suffered from headache, sore throats, and from diarrhœa; the appetite was impaired, and they fell into a feeble state of health. Women and children were more liable than men, because they lived more at home, and are therefore more exposed.

To prevent the occurrence of these diseases, of which enteric fever may be taken as the type, it is essential: (1) That the drinking water should be pure; (2) That no sewer gas should enter the dwelling. Water, though delivered pure by a company, might become polluted in various ways after it has entered a dwelling. To prevent such pollution regulations were now laid down by most companies. If the constant system was in use all the drinking water should be drawn from a tap direct from the main; if the intermittent system was in use there should be a separate cistern for drinking water, and this should have no connection with any drains whatever. An overflow or wastepipe should not be attached to any cistern; but, as a precaution against overfilling, a tell-tale or warning pipe should be fixed, discharging in the open air. Every closet supplied with water should only be so supplied by means of a cistern, by which about two gallons of water could flow at each discharge. The supply pipe should not be less than $1\frac{1}{4}$ in. in diameter, so that the water must descend as rapidly and in as straight line as possible. To prevent the entrance of sewer gas it was needful that all sink and bath pipes should deliver into the open air outside the dwelling; their course should be as short as possible, and in many cases the pipe would only have to pass through the wall-sink or bath. The contents would then pass into a trap or gully about $1\frac{1}{2}$ ft. distant from the house-wall, so that if any gas happened to be forced through the trap it would pass into the atmosphere, and not into the house. The soil pipe should be outside the house, and from its upper end there should be a ventilating pipe carried up above the eaves of the dwelling. Some recommended that between the house drain and the main sewer there should be an intercepting trap so as to prevent the foul air, as from public sewers, from making its way into house drains. Two untrapped openings should be made in the drains so as to provide for a free circulation of air. In the model by-laws two methods were given for ventilation of house drains, but in all cases a trap was placed between the house and the sewers. Dr. Kelly said he was not sure that this plan was a good one. It made no provision for the ventilation of the main sewer other than by openings at the street levels. There was no statutory power to erect ventilating shafts by the side of houses without the leave of the owners, and such leave was not often given. In sea-side towns where the sewer was tidelocked twice a day, or in places where the sewer was laid nearly level, the means of ventilation by openings in the road was not sufficient. In some towns where these ventilators

were very offensive enteric fever had been found to be on the increase during the last two or three years.

Every house drain should be directly connected with the sewer so as to do away with any disconnecting chamber or trap. Every soil-pipe being carried up above the house in its full diameter, would then act as a ventilator to the main sewer. In a street half a mile long with houses on each side there might be as many as 150 or 200 ventilators, instead of only having eight or nine on the road level. Sewer gas was often less injurious than the air which was met with in the house drains if they were not well flushed. Any traps or contrivances which hindered the rapid flow of sewage were dangerous to health. That house could alone be considered fit to live in which within its four walls or beneath its basement contained no drains whatever. In many a village enteric fever was unknown, unless it happened to be imported. But in houses where the sanitary arrangements were very complex illness was frequently met with. In most places it was far healthier to live in a clean, well-built cottage, with no drains whatever, than in more pretentious dwellings where the drains were too often defective. In the simplicity of their dwellings the poor enjoyed an immunity often denied to the rich.

FRENCH METHODS OF CALCULATING THE PRESSURES IN AN ARCH.

By E. SHERMAN GOULD, C. E.

Written for VAN NOSTRAND'S MAGAZINE.

In a previous paper (December No. of *Van Nostrand's Magazine*) I gave some of the routine methods employed by French constructors in building arches. These methods give results which, as far as design is concerned, can be guaranteed, for they are, in fact, nothing more nor less than precedent reduced to formulas. But this is by no means to say that arches built upon lines other than those therein laid down are necessarily defective.

In order to establish a test by which any given design for an arch may be tried as regards its strength, many able and well qualified men have labored to investigate the *theory of the arch*, for the purpose of deducing rules by which the strains in the various parts of an arch may be calculated, and the proper proportions of the structure determined.

The calculation of the stability of masonry arches is frequently spoken of as if the subject were one admitting of the application of rigorous mathematical principles. Such, however, is not the case. When dealing with the strains in girders, trusses, and the like structures, composed of elastic materials, we know with a fair degree of determinateness not only the amount and distribution of the weights, but also the lines along which their strains are transmitted to the abutments; while in the reaction of the abutments, given in degree, direction and position, we possess a sure starting point from which all the other reactions may, step by step, be traced.

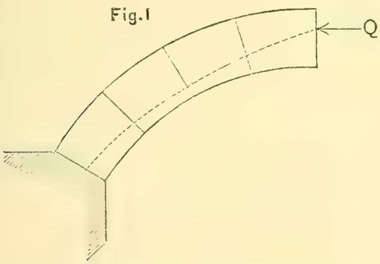
In the case of the vousssoir arch, the problem loses its determinate character. We know, indeed, the amount, position and vertical line of action of the weights of each voussoir, but this is all. We can not even tell how the surcharge is distributed, and it is often difficult to say, in this respect, which is the side of safety upon which to err, for the stability of an arch depends upon its *equilibrium;* and if we assume a system of distribution, or a degree of weight, which does not actually obtain, we may thereby be led to reckon upon a balance of pressures which does not exist.

Some idea of the difficulties which beset this subject may be gained from the fact that the efforts of the many distinguished men, who for the last 150 years have lent their talents to the task, have so far failed in eliminating the indeterminate character from the problem, and

so establishing the true theory of the arch. Their labors have not, however, been wholly fruitless, for many important principles have been developed. Among the most valuable contributions to our knowledge of the matter are the experimental researches of Boistard, who demonstrated the fact that the tendency of all arches, with their abutments, is to yield by breaking into four principal masses, and rotating around the five ensuing points of contact. Unless the arch be in perfect equilibrium, in which case the pressure is uniformly distributed over the faces of the joints, there is always a shifting of the center of pressure at these points, even when the joints do not in the least open, and it is principally owing to the impossibility of accurately locating the position of the center of pressure that the problem of the arch lacks determinate character.

Of all the practical methods proposed as approximate solutions, that of Méry, known as the *curve of pressure*, is the one which, in France, has been received with the most favor, for, since its announcement in 1840 to the present day, it has, with various modifications, been almost exclusively employed in the designing and testing of arches in that country.

Fig. 1

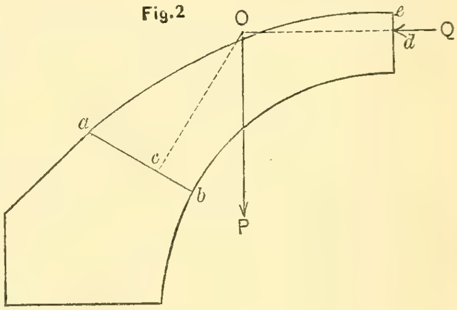


Before describing this important method in detail, it will be well to glance at its underlying principles. Let Fig. 1 represent the half of a segmental arch, kept from falling forward by the horizontal thrust Q. If the arch were complete this thrust would be furnished by the reaction of the other half, which we now supposed to be removed. If we knew the *amount* and *point of application* of the thrust Q, the simple graphic process of combining the same with the weights of the successive voussoirs, would enable us to obtain at once the *curve of pressure*, shown by the dotted line.

As regards the *amount* of Q, this element might be determined by the formula of Navier, to be described further on, but, as we have just seen, unless the arch be in perfect equilibrium, there is a tendency, slight or marked, according as the arch approaches or recedes from a state of perfect equilibrium, to open at the crown, and this causes the *point of application* of Q to rise or fall vertically along the face of the (imaginary) joint at the center.

Again, let Fig. 2 represent a half arch, and let *ab* be the joint of rupture (see article in December No. of magazine). Let P represent the weight of the mass situated above the joint of rupture, passing through its center of gravity, and Q the horizontal thrust. The mass above *ab* is held in equilibrium by Q, P, and the

Fig. 2



reaction of the joint of rupture along *o c*. Of these three forces we can easily determine P in degree and position. If we knew the points of application *c*, *d* of the other two, a simple calculation or graphical construction would give us at once the value of Q in terms of P, and the curve of pressure could be accurately described.

But the same tendency exhibited by the thrust Q to concentrate itself at some point other than the center of the joint at the crown, is repeated by the component *o c* at the joint of rupture *a b*, so we are equally unable to fix the position of the point *c*.

In order to give determinateness to the problem, it is necessary to fix, arbitrarily, the position of the points *d* and *c*. M. Méry places the first at the distance *e d* from the extrados, equal to one-third of the depth at the crown, and the second at the distance *c b* from the intrados, equal to one-third of the joint *a b*. He

does so, from the following considerations: He assumes that the horizontal pressure at the crown is *nil* at the intrados, and that it increases in arithmetical progression, reaching a maximum at the extrados. The total pressure is therefore represented by the area of a triangle constructed upon the vertical joint, with its apex at the intrados. The resultant passes through the center of gravity of the triangle; hence the point d is at $\frac{1}{3}$ of the altitude of the triangle, measured from the base. Reasoning in the same way for the joint of rupture, the maximum pressure being here at the intrados, the point c falls at the distance $cb=\frac{ab}{3}$ from b. To ascertain if the thickness at the crown is sufficient to resist the pressure of the thrust, he assumes that $\frac{2}{3}$ of the total pressure is sustained by $\frac{1}{3}$ the depth at the crown. In other words, he assumes the keystone to be subject to a strain double of what would be produced if the pressure or thrust was evenly distributed over the whole depth. And the same way for the pressure upon the joint of rupture. As this assumption magnifies the actual strain—provided the points c and d fall where Mons. Méry places them—we may accept it with safety.

Of this method and its use the shortest criticism is the best. It is founded wholly upon undemonstrable assumptions, but as it invariably leads to dimensions which practice proves to be sufficient, and is moreover of an extreme simplicity in its application, the wisdom of the French engineers in adopting its use is fully justified.

In order to make this important method quite clear, I will work out an example by it. For convenience I take an example from Dubosque, in which the measures are given in French units. Fig. 3 shows half a full-centered arch, span 10 meters, rise, consequently, 5 meters. As it is not necessary that the real voussoirs enter into the calculation, we divide it so that a joint occurs at the center. The weights of the fictitious voussoirs above

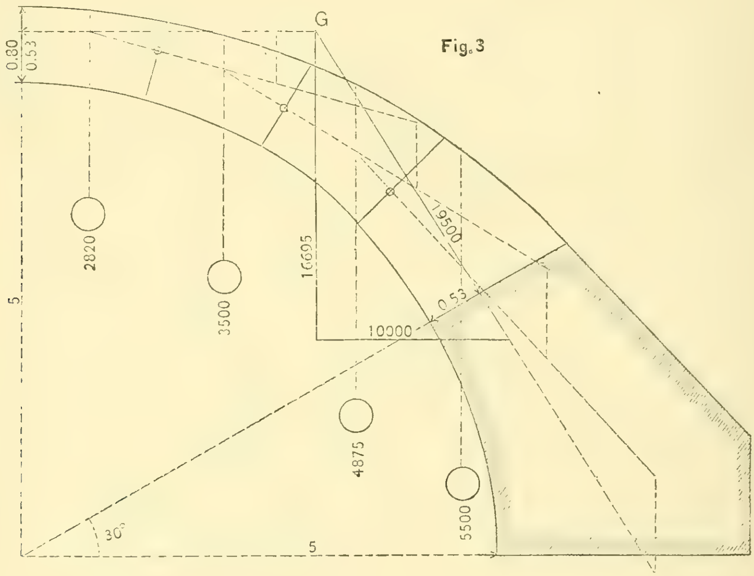


Fig. 3

the joint of rupture are 2,820 kilos., 3,500 kilos., 4,875 kilos., and 5,500 kilos., as shown in the figure passing through their respective centers of gravity. Their total weight is 16,695 kilos., which passes through the center of gravity of the whole mass above the joint of rupture. It must be borne in mind that, the French unit being the meter, we are now dealing with a slice 3.28 ft. thick, instead of 1 ft. only, as would be the case if we were working in English measures. Hence the greater weights.

The depth of keystone is 0.80 meters (Dubosque uses Desjardin's formula).

Length of joint of rupture, 1.60 meters. Passing the horizontal thrust through a point 0.53 meters ($=\frac{2}{3}$ of 0.80) from the intrados at the crown, and producing it till it cuts the vertical line in G, and joining G with a point in the joint of rupture distant 0.53 meters ($=\frac{1}{3}$ of 1.60) from the intrados, we may readily construct the triangle of forces shown in the figure, having sides proportional to the weights 16,695, 10,000, and 19,500 kilos. All the rest of the work is sufficiently apparent from the figure, where the points at which the curves, or rather the broken lines, cuts the joints are marked by little circles. It is usual to commence at the crown, combining the horizontal thrust with the weight of the first voussoir at the line passing through its center of gravity. We could, however, just as well commence at the joint of rupture, and combine the oblique resultant with the weight of the voussoir next above the joint of rupture. Also the curve may be continued below this joint, down through the entire abutment, if desired.

This example is certainly that of an exceedingly well-conditioned arch, and there is no need to test the dimensions, either of the key or of the joint of rupture. If, however, in verifying the proportions of any given arch, we should find the curve of pressure to approach very closely to the extrados or intrados at any point, we should be obliged to see if the area comprised between the curve and the nearest bounding line of the arch was sufficient to bear the strain of $\frac{2}{3}$ of the entire resultant pressure at that point. Thus supposing, merely as an illustration, that we feared the curve approached too near the intrados at the joint of rupture, the resultant passing through this point is 19,500 kilos., two-thirds of which are 13,000 kilos. The area sustaining this pressure is $100\times53=53{,}000$ square centimeters, or 2.5 kilos., nearly, per square centimeter, or not quite $35\frac{3}{4}$ lbs. per square inch.

There is another, quicker way of getting the horizontal thrust. This is by the rule of Navier, already referred to, which reads that, in an inch *in equilibrium*, the horizontal thrust at the crown is given by the product of the radius of the curve of the intrados *at the crown* into the weight upon the unit of surface at the crown. The thrust thus obtained can be placed at the upper third of the depth of keystone, as before, and the curve worked out, as already shown.

This rule is often used by French engineers. At first sight it would seem a very bold jump at a conclusion, for it sets out by assuming the arch *to be* what we want to ascertain *if it is.* But it must be borne in mind that the whole investigation rests upon assumptions, more or less plausible, and that the tendency of a loaded arch, or of one in which the thickness is progressively augmented from the crown to the haunches, is to approach the form of equilibrium, so the assumption is not so audacious as would at first appear. It is certainly a very convenient rule, and is probably a very near guess at the truth, in a large majority of cases.

Let us apply it, for instance, to the example just worked. Taking the average weight of a cubic meter of masonry at 2,500 kilos. we would have for the weight per unit of surface at crown,

$$0.80\times2{,}500=2{,}000 \text{ kilos.}$$

Multiplying this by the radius, 5 meters, we have,

$$5\times2{,}000=10{,}000 \text{ kilos.}$$

Exactly what we found by Méry's method.

It would be a very interesting and instructive exercise to test a number of arches of different forms by the two methods. It must be borne in mind that the *radius at the crown* is what is used. In full-centered and segmental arches, this is the radius with which the intrados is struck. In basket-handled arches it would be the radius of the central arc. In elliptical arches the proper radius can be found by trial.

It will be observed that all of the above applies only to the arch proper, without extrinsic load or surcharge. When the surcharge is to be taken account of, it is necessary to combine it with the weights of the voussoirs, individually and collectively, which leads to a somewhat troublesome research of the centers of gravity.

A surcharge introduces an additional confusing element into the calculation, at least in many cases. When it consists of a bank of earth it is customary to consider such bank as devoid of cohesion, and pressing vertically downward over the whole extent of the horizontal projection of the arch. If this assumption

were correct there would certainly be in the case of a very high bank, an immense crushing strain upon the masonry, but on the other hand, the arch could not yield except by direct crushing, for one part, the crown for instance, could not sink without a corresponding rise at the haunches, which would be prevented by the immense weight upon them. Now, we know there are cases where the crown of arches so loaded settles, which would seem to prove that compensating pressure on the haunches must be wanting. The question would seem to depend upon how far the earth "arches itself," and how large a mass on each side of the center tends to detach itself and press upon the arch.

It would be repeating a twice-told tale to reiterate here the fact that the main cause of failure in an arch is the settling or spreading, or both, of the abutments. If these can be prevented from moving, either vertically or horizontally—and by "abutment" we must in this case understand everything below the line of rupture—arches of very slight dimensions can sustain surprisingly heavy loads without altering their shape, and their safety and rigidity will be then found to depend upon *workmanship* rather than *design*.

NOTE ON WEYRAUCH'S "VARIOUS METHODS OF DETERMINING DIMENSIONS."

By WM. CAIN, C. E., Charleston, S. C.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

In this Magazine for October, 1883, appears an article on determining dimensions taken from "selected papers of the Institution of Civil Engineers," in which the author, Professor Weyrauch, of the Polytechnic of Stuttgart, has given a résumé of many proposed formulæ for determining unit strains when the extremes of strain to which the piece is liable are given. In quoting me on p. 317, Weyrauch says: "for *alternate tension and compression* Cain simply makes $\varphi=0$." This is a mistake, for I did not mention this case in the article in this Magazine for November, 1877, referred to, and in a subsequent series of articles on "Maximum Stresses, &c." (Science Series, No. 38), I used "the American method," as Professor Weyrauch styles it, which I still think about as sound and practical as any proposed. If the reader will compare the results given on p. 393 of the issue of November, 1883, he will notice that the American method does not differ in the results so largely from the purely theoretical formulæ as to cause us to change our practice, even if the latter were based on more correct data. However, as these data are of the most meager kind the formulae can hardly be said to be based on any experimental data; therefore, we shall pass on to the case of simple tension or compression.

For this case, singularly enough, the last theoretical speculation is based on ideas advanced before Wöhler's experiments were made, *i. e.*, that a suddenly applied load causes twice the increase of stress due to the same load gradually applied.

In a paper presented by Messrs. John Griffen and Thomas C. Clarke to the American Society of Civil Engineers, June 5, 1872, this very principle, as deduced in mechanics, was used in the rule proposed—"multiply the live load by two and add it to the dead load," and treat the sum as dead load alone. Clericetti (Vol. 29, No. 5) starts with this assumption, and first shows the principle to agree with Wöhler's results, but to differ very greatly from the English experiments mentioned. He then deduces very easily *the stress per unit of area* (by dividing the sum of live and dead loads by the cross section).

$$b_0=\frac{f}{2-\theta_0} \quad . \quad . \quad . \qquad (1)$$

where θ_0=ratio of dead load to sum of live and dead loads, and f represents some stress less than the primitive strength that must be chosen to answer

the needs of practice. This formula agrees for certain constants with Ritter's and Lippold's. Gerber, in place of multiplying the live load by two, endeavors to ascertain from Wöhler's experiments the factor to multiply by, but it is plain that these experiments are on too small a scale to decide this matter; in fact, they do not agree with the English experiments.

The formula proposed by the writer in *Van Nostrand's Magazine* for November, 1877, is,

$$b=7500(1+\theta) \quad . \quad . \quad . \quad (2)$$

Here b=stress in pounds per sq. in.,
And, θ=ratio of smallest strain the piece ever bears to the greatest.

This is a modification of the Launhardt Weyrauch formula that is supposed to allow for impact.

If, in formula (1), we make $f=15{,}000$, the extremes of safe unit stress for $\theta=0$, or $\theta=1$, are the same by formulae (2) and (3).

$$b_0=\frac{15{,}000}{2-\theta_0} \quad . \quad . \quad . \quad . \quad (3)$$

b is here expressed in pounds per square inch. Neither of these formulae, (2) or (3) are correct, nor agree with all the experiments that have been made; besides they are based upon different principles, yet the form of both is simple, both aim to include the effect of impact, and both embody Wöhler's law—that the minimum strain sufficient for rupture *decreases* as the difference between the extremes of strain to which the piece is liable *increases*. Hence, I thought that it would be interesting to compare the results.

Thus, assuming the values of $\theta=\theta_0$, as in the table below, drawn from the article quoted (in the November, 1877, issue of this Magazine), we deduce for trusses of various spaces the following values of b and b_0 for the lower chords:

Span.	$\theta=\theta_0$	b	b_0
10 ft.	0	7,500	7,500
100 "	$\frac{9}{37}$	9,325	8,540
200 "	$\frac{3}{8}$	10,300	9,230
300 "	$\frac{1}{2}$	11,250	10,000
400 "	$\frac{5}{8}$	12,190	10,910
Dead load only.	1	15,000	15,000

The Weyrauch formula would give, if the constant is so chosen, b=15,000 lbs. per square inch for dead load only, and b=10,000 lbs. for $\theta=0$, and intermediate values for other values of θ. I submit that the extremes are not great enough for practice, which is to be accounted for from the fact that the Weyrauch formala is deduced directly from Wöhler's experiments, and does not otherwise, even empirically, include impact.

When we go to the web members of a 200-ft. span Pratt bridge, Weyrauch's formula gives as the safe strain for counters 10,000 lbs. per sq. inch, and for the end ties 11,750 lbs. per square inch; whereas formula (2) above gives for a 16 panel truss, the safe unit stress for ties:

in 1st	panel....	10,000
2d	"	9,940
3d	"	9,780
4th	"	9,460
5th	"	8,900
6th	"	7,950
7th and 8th	"	7,500
Counters	....	7,500

In this computation the *maximum strain* on tie was found for load extending from farthest abutment to foot of tie, *the minimum* when load extends from nearest abutment to top of tie. without entering into the computation of strains from dead load alone, the minimum to be used in the value of θ_0 in formula (3) it is plain that whilst the values by (3) would still differ from those just given, yet the differences would not be so great as in the case of the chords, where the minimum strains in both cases are due to dead load only.

Considering the totally different deviation of the formulae, the difference in the values is not so great: so that in practice either formula could be chosen, and form the basis of any short rules that might suggest themselves.

The formulae have this to recommend them; that they are systematic, partly based on experiment, aim to include impact, and embrace Wöhler's law.

It is plainly indefensible, as Weyrauch says, to disregard this law, for experiments and experience both sustain it.

Now, although American engineers acted on this principle in certain cases before the law was even derived by Wöh-

ler, yet it has not been uniformly applied. Thus, I am not aware of the first specification where b for web members is made to vary from panel to panel according as the difference between the extremes of strain of the piece changes as we go from panel to panel.

Generally all, or nearly all, of the main ties are designed for the same maximum unit stress, though it is very easy to specify a varying stress in accordance with Wöhler's law. None of the formulae yet proposed are accurate, as Weyrauch says, for the actual conditions met with in practice. So that, if engineers decline to follow any of the formulae strictly, still some simple rule that gives results within the bounds of safety, and that follows Wöhler's law, should be used.

Thus, for any bridge, having given a unit stress for the lower chord and the main ties at the end, also another unit stress for the counters and middle ties, then make the unit stress on the intermediate ties increase regularly from the first panel where a counter is needed to the end panel.

Thus, in the example just given, the 7th is the first panel requiring a counter, so that, subtracting 7,500 from 10,000, and dividing by 6, we get 417, the common difference, from whence we find, by addition, for 6th panel, $b=$ 7,917

5th	"	= 8,334
4th	"	= 8,751
3d	"	= 9,168
2d	"	= 9,585
1st	"	=10,000

These unit stresses are all less, by several hundred pounds than those given above by the formula, $b=7{,}500(1+\theta)$, so that the results are *safer*, at least, than by this formula, and approximate those given by the other formula (3).

Crude as this method is, it recognizes Wöhler's law, and is better than the old method. For very large bridges, especially, there is such a large difference between the unit strains given the end web members and the counters, that it is totally inconsistent not to vary the unit strains on the intermediate web members in some fashion.

Where other styles of truss, as the Fink, bowstring, etc., are used, the simple rule suggested above cannot be used, but a resort to some formula involving maximum and minimum strains is necessary. In fact some formula or formulas of this kind must form the rational basis of any short rules, not simply for bridge members, but for the computation of the sizes of pieces in any structure or machine whatever.

ECONOMY OF STEAM POWER AT WORKS OF NATIONAL RUBBER CO., BRISTOL, R. I.

By JOHN W. HILL, M. E.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

THE following paper is a resumé of a series of investigations made by the writer during the months of August and September, 1883, for the National Rubber Co., at Bristol, R. I., with a view to suggesting improvements in the steam plant, calculated to increase the economy, and comprehending a general examination of all the principal appliances for making and using steam in the works.

The Works of the National Rubber Co.—the largest of the kind in the United States—cover more than forty acres of ground, employ over two thousand operatives, and consume for power and steam-heating purposes alone upwards of sixty thousand dollars worth of coal per annum.

To reduce this coal consumption by well-advised changes in the plant, was the object of the experiment: the results of which are given in narrative form in the following paper.

The investigations embraced the following machinery :

North Boilers.—Steam used for curing rubber goods, heating north building, driving small engine, furnishing power to the sewing machines, and driving elevator engine.

Main Boilers.—Steam used for curing rubber goods, heating rolls and presses, driving 36″ × 72″, condensing engine, driving 26″×48″ and 28″×48″, non-condensing coupled engines, driving 10″× 24″ non-condensing machine-shop engine, driving feed pumps and fire pumps and heating buildings.

Pump House Boilers.— Furnishing steam to 1,000,000 gallon Worthington Compound Duplex Pumping Engine.

Harris Corliss' Condensing Engine.— Furnishing motive power to machinery of new works.

Harris Corliss' Non-Condensing Coupled Engines.—Furnishing motive power to machinery of old works.

Harris Corliss' Non-Condensing Engine.—Furnishing motive power to machine shop and box factory.

Worthington Compound Duplex Pumping Engine.—Supplying sea water to the works.

The boilers in the North house and in the Main house were tested for economy in the following manner:

All water supplied during the interval of trial in each instance was carefully weighed in a large tank mounted upon a new Fairbanks' dormant scale, capable of weighing 3,500 pounds in gross. From the weighing tank the water was drawn into a supplemental tank connected with the suction of the feed pump, from which it was pumped as required into the boilers.

In the North house the feed water was taken from the return pipe, from the curing ovens and radiators. In the Main house the feed water was drawn from the city mains.

In order to check the quantities of water delivered to boilers during certain fixed intervals of time, the levels in the glass gauges were carefully read at beginning of trials, and at end of trials; and in all instances when the original levels were not fairly restored at end of trial, corrections have been made by adding or subtracting a weight of water equivalent to the volume in excess or short of true level at temperature of evaporation.

The feed water was tested for temperature just before its entrance to boilers, and in all instances where it passed through heaters it was tested for temperature before and after it passed the heater.

The coal burned during interval of trial was weighed in uniform charges of 300 pounds upon tested platform scale, and dumped as required in front of boilers. Trials were begun and ended with strong charges upon the grates and clean ashpits, and all ash, clinker and unburnt coal found in the ashpits at end of trials were weighed back and credited as noncombustible.

The temperatures of air in boiler-house, and hot gas passing in the smoke connection, were taken at regular intervals of fifteen minutes.

The steam pressure, temperature of feed to boilers, water levels, and pressure of atmosphere were read at regular intervals of fifteen minutes.

Calorimeter observations for quality of steam were made at regular intervals of fifteen minutes or half-hourly in the several trials.

In the trial of boiler at Pump house, the feed water supplied was measured in a large storm water tank, capable of carrying several days' supply, by taking levels at beginning and at end of trial, and at regular intervals of one hour, during the trial from which volumes (computed from differences of heads and known diameter of tank) the weight corresponding to observed temperatures was calculated. The coal for the trial at Pump house was weighed in uniform charges of 100 pounds and dumped in front of boiler as required.

In the test for economy of Harris Corliss' condensing engine the consumption of steam was measured by weighing the condensation as it was pumped from the condenser (surface) by the air pump.

The discharge of the air pump was conveyed through a tight rubber hose to the weighing tank previously mentioned for weighing feed water to boilers, and weighed for hourly delivery.

The quality of steam, meanwhile furnished by the boilers, being tested by calorimeter at regular intervals of thirty minutes.

Indicator diagrams were taken from each end of cylinder by Thompson indicators, set with short connections, at regular intervals of fifteen minutes. The steam gauge in pipe, engine counter, barometer vacuum gauge, and thermometers in injection, and overflow pipes, and hot well of condenser, and atmospheric thermom-

eter were read regularly every fifteen minutes.

In the test for economy of Harris-Corliss' non-condensing coupled engines, and Harris Corliss' non-condensing machine-shop engine, the consumption of steam was measured by weighing the feed water to the plain cylinder boilers of Main house, all the evaporation of which was confined to the coupled engines and to the machine-shop engine during the interval of trial. The arrangement of steam pipes in the Main house is such that steam to the machine-shop engine could not be furnished independent of the main pipe supplying steam to the coupled engines, and as it was inconvenient to stop the machine-shop engine for a day at time of test, the latter was indicated for power during this trial, and an estimate was made of steam consumption upon precedents from engines, in similar condition and developing similar powers.

The feed water to boilers similar for this trial was reported in hourly quantities.

The data for quality of steam furnished by the boilers were taken half-hourly during trial.

Two Thompson indicators were used upon each cylinder of coupled engines, and a single Elliott-Richard's indicator was used upon the cylinder of machine-shop engine.

Diagrams from all cylinders were taken regularly every fifteen minuter.

The pressure in pipe and engine counter were read regularly every fifteen minutes.

In the tests of Harris Corliss' condensing and Harris Corliss' non-condensing coupled engines for distribution of power, the only data taken were the indicator diagrams which were lettered to correspond with the known machinery driven.

In the test of Worthington compound pumping engine, for capacity, the data taken were the heads at fixed intervals of time in the sea water tank in the factory yard, the engine counter connected with pumps for same intervals, and the length of the stroke made by plungers during the period of test.

In the test of same machine for duty the data taken were the pressures of steam in boiler, water in rising pipe, and of atmosphere in pump house, vacuum in condenser, engine counter, temperatures of injection, overflow and condensation from condenser, rise and fall of tide in the harbor, weights of coal and water supplied to boiler and quality of steam furnished by boiler, temperatures of waste gases from smoke connection temperatures of feed water to heater in smoke connection, and in feed pipe between heater and boiler, and water levels in boiler.

The tests of Pump house boiler for economy, and of Worthington pumping engine for duty, were made at one and the same time, but for convenience of reference of boiler performance to boiler performance in North and Main houses, will be reported separately.

North Boilers.

The North boiler house contains five return tubular boilers, set in independent furnaces, with independent feed water connections, and independent connections of steam spaces of boilers with two steam mains, one leading to the curing ovens, and one leading to the steam radiators in North building.

The boilers are provided with smoke extensions, from which vertical legs or uptakes lead into a horizontal flue of sheet iron running above and in front of boilers to the brick chimney. Each uptake is provided with an independent pivoted damper.

Each boiler is furnished with a pop-safety valve, steam gauge, glass water gauge, test cocks, and separate regulating cock and check valve in feed pipe.

The boilers were constructed by the Whittier Machine Co., Boston, Mass.

The following dimensions are taken partly from the contractor's drawing and partly from personal measurements.

Dimensions North Boilers.

Style............................	Tubular.
Number........................	5
Diam. of shells..............inches	48
Length " ....feet	14
Tubes..........number 60, diameter	3″
Grate............................	4′×4′
Heating surface each boiler....sq. ft.	747.701
Grate " " " .... "	16
Tube vent................ .... "	2.475
Ratio heating to grate surface	46.731
Ratio grate surface to tube vent......	6.464
Total heating surface 5 boilers..sq. ft.	3738.5
Total grate surface 5 boilers... "	80
Chimney Height................feet	101
Chimney cross section.........sq. ft.	12.25

Ratio total grate surface to cross section of chimney	6.53
Space over bridge wall........inches	12
" " grate " .... "	18.5

Of above boilers the four nearest chimney were tested together for economy.

The trials were made of these boilers upon the work of curing ovens.

The East boiler meanwhile running separately to furnish steam to the radiators and sewing machines engine.

The first trial, August 20–21, with maximum consumption of steam, represented the requirements of the nine (9) new curing ovens with open stop valves and maximum loss of heat by contact of air and radiation for subsisting temperatures of steam and air and oven walls, and as many of the five (5) old curing ovens as were then in use under ordinary conditions.

The steam to the elevator engine is taken from the same main which supplies the curing ovens, and this additional work, however slight, was upon the boilers (4) operated for economy.

The second trial, August 21, for economy under ordinary daily conditions of work, represents the requirements of so many of the five (5) old curing ovens as were in use at that time, together with steam consumption by elevator engine as before.

The coal fired was Cliffield, a mixture of bituminous nut and slack.

In the following table are given the principal data, averages, totals, and calculated results from the log of first trial.

FIRST TRIAL OF NORTH BOILERS.

Date of trial	Aug. 20–21
Boilers used	4
Heating surface.........sq. ft.	2990.8
Grate surface.........sq. ft.	64.

GENERAL OBSERVATIONS.

Duration of trial.........hours.	16
Average steam pressure.... pounds,	87.367
Average temperature of feed water to boilers.........161.547=	162.295
Average temperature of air in boiler-room, Fahr.	100.375
Average temperature of waste gases, smoke connection, Fahr.	345.20
Average temperature of waste gases, boiler extension, Fahr.	387.72
Barometer.........inches,	29.943

CALORIMETER.

Average water heated......pounds,	200
Average steam condensed..pounds,	10.389
Average initial temp,. Fahr... 77.444=	77.479
Average final temp., Fahr....130.222=	130.556
Average heat units per lb. of steam..	1,152.643
Thermal units per pound of steam at observed pressure	1,214.28
Difference	61.637
Latent heat at observed pressure....	881.87
Percentage of water entrained in the steam	6.989

TOTALS.

Total water to boilers.......pounds,	91,473.000
Total water entrained in the steam, pounds,	6,393.048
Total steam furnished......pounds,	85,079.952
Steam per hour.........pounds,	5,317.497
Total coal burned.........pounds,	11,100.000
Total ash and clinker and unburned coal weighed back.........pounds,	942
Percentage of combustible	91.514
Coal per hour.........pounds,	693.75

ECONOMY.

Steam per pound of coal from temperature of feed water....pounds,	7.6648
Steam per pound of coal from and at 212 Fahr.........pounds,	8.349
Steam per pound of combustible from and at 212 Fahr.........pounds,	9.123
Steam per square foot of heating surface per hour.........pounds,	1.778
Coal per square foot of grate surface per hour.........pounds,	10.84

The trial of sixteen (16) hours was divided into two runs of eight (8) hours each, the day run to Hammack, and the night run to West, to compare the work of firemen.

No material change occurring between day and night in condition of coal, feed water, and steam consumption, the results for the separate runs are a very certain index of the comparative qualities of firemen. In the following table are given the necessary averages, totals and calculated results for day run by Hammack:

DAY RUN, HAMMACK, 8 HOURS.

Coal burned per hour.........pounds,	675.00
Steam per hour .... "	5,522.53
Temperature of feed water, Fahr., 163=	163.773
Steam pressure.........pounds,	91.46
Steam per pound of coal from temperature of feed water.........pounds,	8.1815
Steam per pound of coal from and at 212 Fahr.........pounds,	8.8998

In the following table are given the averages, totals and calculated results for the night run by West;

NIGHT RUN, WEST, 8 HOURS.

Coal burned per hour.........pounds,	712.5
Steam per hour......... "	5,112.466
Temp. of feed water, Fahr., 160.09=	160.275
Steam pressure.........pounds,	82.75
Steam per pound of coal from temperature of feed ....pounds,	7.1754
Steam per pound of coal from and at 212 Fahr.........pounds,	7.8327

It will be observed that the demands for steam were greatest by eight (8) per cent during the day run, with a difference of 3.5 degrees in temperatures of feed water in favor of day run. The difference in temperature of feed water day and night, is roughly 0.3 of one per cent or too small to affect the comparison if neglected entirely.

For the day run Hammack carried an average pressure of 91.46 pounds, with an evaporation (net) of 8.9 pounds of steam per pound of coal from and at 212 Fahr.; while West for the night run carried an average pressure of 82.75 pounds (a loss of 8.75 pounds) with an evaporation of 7.83 pounds of steam per pound of coal from and at 212 Fahr. With odds in favor of the night run, Hammack develops a better average performance by nearly fourteen (14) per cent.

Hammack's economy in this trial is good, and as a matter of note at the time, fired his coal with less exertion than did West during the night run.

The fires were cleaned one hour before Hammack left and West started, and the latter had the benefit of a pressure of 94 pounds to start with, and no unfavorable conditions calculated to make his economy inferior to Hammack's. The writer is of the opinion that West could scarcely be relied upon to push these boilers for maximum requirements with economy, but with two firemen the equals of Hammack in skill and assiduity, the boilers (4) can be worked to furnish all steam required for old and new curing ovens, under ordinary conditions, with an economy at least good, if not excellent. It is not possible to develop a high economy with these boilers subject as the log shows to great fluctuations in the demands of steam from hour to hour.

In the following table are given the averages, totals and calculated results for second trial of North boilers;

SECOND TRIAL NORTH BOILERS.

Date of trial		Aug. 21.
Boilers used		4
Heating surface	sq. ft.	2990.8
Grate surface	sq. ft.	64

GENERAL OBSERVATIONS.

Duration of trial		13 hrs. 45 min.
Average steam pressure	pounds,	98.86
Average temperature of feed water to boilers		153.87=154.496
Average temperature of air in boiler room, Fahr.		95.41
Average temperature of waste gases, smoke connection, Fahr.		313.86
Average temperature of waste gases, boiler extension, Fahr.		371.73
Average barometer	inches,	29.963

CALORIMETER.

Circulation per hour		8,819.81
Condensation per hour		352.4
Temp. of injection, Fahr.		71.563= 71.586
Temp. of overflow, Fahr.		114.809=115.013
Temp. of condensation, Fahr.		88.209= 88.276
Thermal units per pound of steam		1,175.160
Thermal units per pound of steam at observed pressure		1,216.700
Difference		41.54
Latent heat at observed pressure		876.00
Percentage of water entrained in the steam		4.742

TOTALS.

Total water to boilers	pounds,	39,009.2
Total water entrained in the steam,	pounds,	1,849.81
Total steam furnished	pounds,	37,159.31
Steam per hour	pounds,	2,702.509
Total coal burned	pounds,	4,416.5
Total ash and clinker and unburnt coal weighed back	pounds,	533
Percentage of combustible		87.93
Coal per hour	pounds,	321.2

ECONOMY.

Steam per pound of coal from temperature of feed water	pounds,	8.414
Steam per pound of coal from and at 212 Fahr.	pounds,	9.2554
Steam per pound of combustible from and at 212 Fahr.	pounds,	10.5259
Steam per square foot of heating surface per hour	pounds,	0.9036
Coal per square foot of grate surface per hour	pounds,	5.0187

The trial of thirteen (13) hours and forty-five (45) minutes was divided into a day run of 7:45 hours for Hammack, and a night run of 6 hours for West, with the following results:

DAY RUN, HAMMACK, 7 HOURS 45 MINUTES.

Coal burned per hour	pounds,	286.516
Steam per hour	pounds,	2,587.083
Temp. of feed water, Fahr.		147.21=147.742
Steam pressure	pounds,	102.37
Steam per pound of coal from temperature of feed	pounds,	9.0294
Steam per pound of coal from and at 212 Fahr		9.995

NIGHT RUN, WEST, 6 HOURS.

Coal burned per hour	pounds,	366
Steam per hour	pounds,	2,851.58
Temp. of feed water, Fahr.		161.66=162.41
Steam pressure	pounds,	94.33
Steam per pound of coal from temperature of feed	pounds,	7.7912
Steam per pound of coal from and at 212 Fahr.	pounds,	8.5056

In the comparison of Hammack and West for second trial the average hourly

demand for steam was ten per cent. less for the day run than for night run, and the difference in temperature of feed water 1.21 per cent. in favor of night run.

The results show Hammack's economy to be 1.75 per cent. better than West's; which corrected for difference of temperature of feed water, makes a gain of 18.75 per cent. for Hammack over West.

Although the demand for steam per hour was least during day run, the writer is not inclined to ascribe Hammack's extra economy to this cause; the rate of evaporation being too low for good economy during the whole trial, and the reduced demand for steam during Hammack's run being rather against than in favor of his economy.

Assuming the boilers to be run for one year at a rate midway between first and second trials, or with an hourly consumption of coal, based on Hammack's record, of 480.75 pounds, then the coal burned per day (24 hours) would be 5.769 tons, and for a year of 300 days, 1,730.7 tons, or a cost at $5.50 per ton, or $9,518.84, exclusive of coal required to start fires and raise steam Monday mornings; (which should be 1.25 tons, or 62.5 tons for entire working year of fifty weeks).

Under the same conditions, but estimating from hourly consumption of coal by West of 539.25 pounds, then coal burned per year of 300 days would be 1,941.3 tons, costing $10,677.15, exclusive of Monday mornings coal to raise steam, which would be no greater, and possibly less, with two firemen of Hammack's standard, than with two firemen of West's standard.

Assuming, however, that a new fireman be substituted for West, equal in efficiency to Hammack, then the annual saving to the Rubber Co., under conditions of work midway between the work of first and second trials, would be $579.15, this amount being the saving Hammock makes (by his record) over West.

Referring to the trials as a whole, the economy is fair considering the dimensions of boilers, but these are small for tubular boilers and should have been not less than 60 inches diameter of shell with 50—4″ tubes or 75—3″ tubes each.

Tubular boilers of less than 60 inches diameter of shell seldom give a high economy, from the fact that the shell is the most efficient surface, and the larger it is for a given length the more complete the absorption of radiant heat from the grate; more than 75 per cent. of the total steam is made by the heating surface of shell, and this surface, always consistent with safety, should be as large as possible.

An excellent change, however, would be the substitution of three boilers similar to the tubular boilers in Main house E G 6′ diam., 15′ long, with 100—3″ tubes each; by the latter change an improvement of 20 to 25 per cent. in the economy would be obtained, with ample heating surface for most exacting requirements.

The latter improvement should furnish the Rubber Co. a saving of $2,000 in round numbers in the coal bill per year for North house, a very handsome income for the investment represented by the three 72″ tubular boilers, besides the labor will be reduced by the fewer fires to manage, and the fireman should be able to wheel both coal and ashes with this arrangement.

The present stack is admirably adapted to the proposed change of boilers.

Main Boilers.

The boiler plant in the Main house consists of a battery of six tubular boilers, and four sections of sixteen plain cylinder boilers, the evaporation from which mingles in one system of distributing steam mains.

The tubular boilers are similar in general design to those of the North house, but are of larger dimensions, and set in independent furnaces, provided with independent uptakes and dampers, separate feeds, steam and water gauges.

The feed water is taken from the city mains, and pumped through a fuel economizer placed in the flue leading to the chimney.

The smoke connections of sheet iron pass aft over the boiler setting and bend down at rear end of brickwork into a brick flue, common to all the (tubular) boilers, and leading into the base of brick chimney.

The boilers are well designed and set and show a gratifying economy.

In the following table are given the dimensions of tubular boilers, taken partly

from the contractor's drawing and partly from personal measurements.

DIMENSIONS OF TUBULAR BOILERS.

MAIN HOUSE.

Style		Tubular.
Number		6
Diameter of shells	inches	72
Length of shells	feet	15
Tubes		100—3″
Grate		6′1″×4′6″
Heating surface, each boiler	sq. ft.	1319.472
Grate surface, each boiler	"	27.75
Tube vent	"	4.125
Ratio heating to grate surface		47.548
Ratio grate surface to tube vent		6.727
Total heating surface, 6 boilers	sq. ft.	7916.832
Total grate surface, 6 boilers	"	166.5
Chimney height	feet	113.5
Chimney cross section	sq. ft.	25
Ratio total grate surface to cross section of chimney		6.66
Space over bridgewall	inches	12
Space over grate	"	27

Builder, Whittier Machine Co., Boston.

Two trials were made of the tubular boilers; the first trial owing to breaks in the record was unreliable, and a second trial was made with the following results. This trial represents the daily work of boilers as at date, August 27th, with no known changes either in the management of fires or in the consumption of steam from the daily average performance.

The coal fired was Cliffield, of the same quality as that used under the North boilers.

PERFORMANCE OF TUBULAR BOILERS.

MAIN HOUSE.

Date of trial		Aug. 27
Boilers used		6
Heating surface	sq. ft.	7,916.832
Grate surface	"	166.5

GENERAL OBSERVATIONS.

Duration of trial	hours	10
Average steam pressure	pounds,	70.545
Average temperature of feed water to heate., Fahr.		66.4
Average temperature of feed water to boilers, Fahr. 107.59=		107.746
Elevation of temperature by heater.		41.346
Gain by heater, per cent.		3.613
Average temperature of air in boiler room, Fahr.		95.050
Average temperature of waste gases in flue front of heater		295.5
Average temperature of waste gases in flue behind heater		233.1
Average temperature of waste gases boiler extension		318.600
Average barometer	inches,	30.214

CALORIMETER.

Average water heated	pounds,	200
Average steam condensed	pounds,	10.157
Average initial temp., Fahr. 74.286=		74.314
Average final temp., Fahr. 127.952=		128.264
Average heat units per pound of steam		1190.585
Thermal units per pound of steam at observed pressure		1210.33
Difference		19.745
Latent heat at observed pressure		891.29
Percentage of water entrained in the steam		2.215

TOTALS.

Total water to boilers	pounds,	127,555
Total water entrained in the steam	pounds,	2,825.343
Total steam furnished	pounds,	124,729.657
Steam per hour		12,472.967
Total coal burned	pounds,	12,361.00
Total ash and clinker and unburnt coal weighed back	pounds,	1,650.00
Percentage of combustible		86.66
Coal per hour	pounds,	1,236.1

ECONOMY.

Steam per pound of coal from temperature of feed water	pounds,	10.091
Steam per pound of coal from and at 212 Fahr.	pounds,	11.5208
Steam per pound of combustible from and at 212 Fahr.	pounds,	13.2933
Steam per sq. ft. of heating surface per hour	pounds,	1.575
Coal per sq. ft. of grate surface per hour	pounds,	7.724

COAL FOR BANKING AND STARTING FIRES.

TUBULAR BOILERS, MAIN HOUSE.

Coal to start fires Monday, A. M., Aug. 27th	pounds,	6,839
Coal to start fires Tuesday, A. M., Aug. 28th	pounds,	6,000
Coal to start fires Thursday, A. M., Aug. 30th	pounds,	4,246

The economy from above trial is excellent, and considering the fact that no exertions were made to improve the performance above the daily average, it shows a result seldom equaled and never to the writer's knowledge excelled. By this the writer does not mean to say that the economy is equal to the best he ever had, but in every instance, and the instances are rare, where better work has been done, it has been under "whip and spur" as it were, and not under the conditions of ordinary factory practice.

In brief, omitting special test trials, this is the very highest economy within the writer's experience, and exhibits an excellent proportion of boilers, a good setting, and a careful management of the fires.

The most important remark to be made in connection with this trial is, that it exemplifies an economy which should be made a standard, and which may be obtained in the North house by the change

of boilers previously suggested. The coal required to start fires Monday and Tuesday, August 27–28, is excessive, and represents a thickness of fire of six to seven inches over entire grate.

The coal used to start fires Thursday morning, August 30th—4,246 pounds—represents a fire four inches thick on whole grate, which is ample and should not be exceeded.

When small quantities of steam are required for power or heating buildings at night, it will be found much more economical to make it with one or two boilers and an active fire than with six or more boilers and a slow fire.

All boilers not in active use at night should have the fires banked, and ashpit doors, fire doors and dampers closed. If closing the fire doors with banked fires is calculated to make steam during the night, then the fire doors may be opened just enough to prevent this, but dampers and ashpit doors should be kept closed.

It costs nothing but vigilance to guard the avenues of loss in steam boilers, while it costs dollars to supply the losses if the means to prevent them are neglected.

After it has been determined how much steam is properly required of the main boilers, then the excess beyond a favorable capacity for the six tubular boilers should be made with more tubular boilers of same dimensions.

The plain cylinder boilers are arranged in three sections of four (4), one section of three (3), and a single boiler set in an independent furnace to burn the refuse from the box factory.

During the test for economy, coal was burned under the single boiler as well as under all the rest.

The glass gauges on these boilers were clogged with sediment, and the water levels were taken from the register gauge cocks.

Each section of boilers is furnished with its own feed connection, smoke connection and steam gauge, and a large drum set transversely over each section, collects the steam from the several boilers preliminary to its delivery to the distributing mains.

A long brick flue running transversely of boilers behind the setting collects the hot gases and delivers to the base of brick chimney.

In the following table are given the dimensions of the plain cylinder boilers.

DIMENSIONS OF PLAIN CYLINDER BOILERS.

Style	Cylinder.
Number	6
Diameter of shells.....inches,	30
Length of shells.....feet,	30
Heating surface, each boiler..sq. ft.,	117.801
Grate " " " .. "	44.000
Total heating surface 4 boilers "	471.204
Ratio heating to grate surface "	10.709

One trial of these boilers was made using same quality of coal as before (for North boilers and Main tubular boilers) under the ordinary factory requirements, as at date Aug. 29th.

PERFORMANCE, PLAIN CYLINDER BOILERS, MAIN HOUSE.

Date of trial	Aug. 29
Boilers used	16
Heating surface.....sq. ft.	1884.816
Grate surface..... "	176.

GENERAL OBSERVATIONS.

Duration of trial.....hours,	10
Average steam pressure.....pounds,	69,352
Average temperature of feed water to heater, Fahr.	66.5
Average temperature of feed water to boiler, Fahr.....152.73=	153.339
Elevation of temperature by heater ...	86.839
Gain by heater, per cent.	7.594
Average temperature of air in boiler room, Fahr.	80.727
Average temperature of waste gases in flue	494.024
Average barometer.....inches,	29.881

CALORIMETER.

Average water heated.....pounds,	200
Average steam condensed..... "	10,118
Average initial temperature, F., 67.446=	67.4626
Average final temperature, F., 119.865=	120.1065
Average heat units per lb. of steam...	1,160.706
Thermal units per pound of steam at observed pressure	1,210.07
Difference	49.37
Latent heat at observed pressure.....	891.88
Percentage of water entrained in the steam	5.535

TOTALS.

Total water to boilers.....pounds,	136,215.0
Total water entrained in the steam, "	7,539.5
Total steam furnished.....pounds,	128,675.5
Steam per hour.....pounds,	12,867.5
Total coal burned.....pounds,	19,073
Total ash and clinker, and unburnt—coal weighed back.....pounds,	2,813
Percentage of combustible	85.4
Coal per hour.....pounds,	1,907.3

ECONOMY.

Steam per pound of coal from temperature of feed water.....pounds,	6.746
Steam per pound of coal from and at 212, Fahr.....pounds,	7.382
Steam per pound of combustible from and at 212 Fahr.....pounds,	8.644

Steam per square foot of heating surface per hour.......... ...., pounds, 6.827
Coal per square foot of grate surface, per hour.......................pounds, 10.837

COAL FOR BANKING AND STARTING FIRES.
CYLINDER BOILERS, MAIN HOUSE.

Wednesday morning, August 29th, pounds, 4,627.*

The low rate of evaporation by the Main tubular boilers, and the very low temperature of waste gases in the smoke extension, seem to indicate that an improved economy may be had by causing the plant to do more work.

The rate of work for trial of Aug. 27 was in round numbers 12,500 pounds of steam per hour if the evaporation for the boilers as a whole, was increased to 14,500 pounds per hour. The writer believes the economy would be sensibly enhanced.

The difference in work is about equivalent to that of three of the plain cylinder boilers.

The economy of the plain cylinder boilers is good for the class. The writer's experience, however, with this type of boiler, is very limited, their use in the West being confined to blast and puddling furnaces, where the waste gases from the furnaces are conducted under the boilers in transit to the chimney. In these cases the steam made is clear gain, for otherwise the waste furnace gases would pass into the chimney at a high temperature, and a large percentage of the thermal power of the fuel be lost. The writer knows, however, from English data upon the plain cylinder boiler, that high economy is not had excepting with shells of large diameter, and then the economy is always inferior to well-designed tubular boilers, owing to the naturally high temperature of waste gases passing into the flue, representing a large percentage of loss of heat, a portion of which is bound to be absorbed in the tubes of a tubular boiler, and made to assist in the production of steam.

Of the total heat of combustion with the plain cylinder boilers of the Rubber Company, from 20 to 25 per cent. is carried off by the waste gases into the stack, whilst 12 to 15 per cent. is known to be sufficient to produce good chimney draft.

The writer respectfully suggests that no possible change in the plain cylinder boilers could improve their economy up to a fair standard.

The shells (30″) are too small in diameter to hope for any excellence, even if the boilers were divided into two lengths of 15′ each, and filled with tubes, the only way in which the economy can be increased.

The writer's experience leads to the opinion that long boilers of small diameter are not as safe against rupture as short boilers of large diameter; the latter are more compact, stronger for transverse strains, less liable to injury from expansion and contraction, and with equal factors of safety more liable for continuous use.

PUMP-HOUSE BOILERS.

These boilers, of which there are two, set in independent furnaces, are of same general dimensions and plan as the boilers of North-house, but with fewer tubes. Each boiler has its own feed and smoke connection, check and stop valves, and steam and water gauges.

In the smoke connection which passes aft over the top of setting, a fuel economizer is placed, through which the feed-water is pumped in transit to the boilers.

The feed pump is now driven by the Worthington pumping engine, but, as demonstrated during the trial, is entirely inadequate to the requirements of boilers, and should be replaced by an independent boiler feeder set in the boiler-room, capable of furnishing a delivery against boiler pressure of 300 gallons per hour. The present feed pump is not equal to the requirements of one boiler by nearly 15 per cent.

In the following table are given the dimensions of boilers, partly from contractors drawing and partly from personal measurements.

DIMENSIONS OF BOILERS, PUMP HOUSE.

Style................................Tubular.	
Number..............................	2
Diameter of shells............inches	48
Length of shells................feet	14
Tubes..............................	49–3″
Grate..............................	4′×4′ 6″
Heating surface each boiler....sq. ft.	626.742
Grate surface, each boiler...... "	18.000
Tube vent..................... "	2.021
Ratio heating to grate surface.......	34.819

* The coal 4,627 pounds to start fires Wednesday morning, Aug. 29th, under plain cylinder boilers, is a fair quantity and represents a fire four inches thick upon whole grate.

Ratio grate surface to tube vent		8.906
Total heating surface, 2 boilers,	sq. ft.	1,253.484
Total grate surface, 2 boilers	"	36
Chimney height	feet	73.5
Chimney cross section	sq. ft.	5.444
Ratio total grate surface to cross section of chimney		6.613
Space over bridge wall	inches	12
Space over grate	"	23

Builder . . . Whittier Machine Co., Boston.

Two trials were made with these boilers, the second of which alone is reported.

The steam pressure and management of the coal for first trial, not fairly representing average performance.

The second trial should have been of greater duration, but the inability of the feed pump (furnished with the Worthington engine), to supply the boiler, limited the trial to eight hours during which time all the delivery of the feed pump was evaporated, besides boiling down the level of water in the boiler nearly 1500 pounds.

The boiler exhibits a good economy, size considered, and the size is ample for the work required.

PERFORMANCE OF PUMP-HOUSE BOILER.

Date of trial		Sept. 3
Boilers used		1
Heating surface	sq. ft.	626.742
Grate surface	"	18.

GENERAL OBSERVATIONS.

Duration of trial		8 hours.
Average steam pressure	pounds	67.422
Average temperature of feed water to boiler, Fahr.		69.333
Average temperature of feed water to boiler	157.906=	158.595
Elevation of temperature by heater.		89.243
Gain by heater	per cent.	7.824
Average temperature of air in boiler room, Fahr.		80.531
Average temperature of waste gases, smoke connection		292.937
Average barometer	inches	29.935

CALORIMETER.

Average water heated	pounds	200.
Average steam condensed	"	10.423
Average initial temperature, Fahr.	72.5=	72.525
Average final temperature, Fr.	120.797=	121.046
Average heat units per lb. of steam.		1,052.082
Thermal units per pound of steam at observed pressure		1,209.56
Difference		157.478
Latent heat at observed pressure		893.09
Per centage of water entrained in the steam		17.63

TOTALS.

Water pumped into boiler	pounds	8,817.186
Water level reduced .6.25″=	pounds	1,484.966
Total water to boiler	"	10,302.152
Total water entrained in steam	"	1,816.269
Total steam furnished	"	8,485.883
Steam per hour	"	1,060.735
Total coal burned	"	1,000.000
Total ash and clinker and unburnt coal weighed back	pounds	38.00
Per centage of combustible		96.2
Coal per hour	pounds	125.0

ECONOMY.

Steam per pound of coal from temperature of feed water	pounds	8.486
Steam per pound of coal from and at 212 Fahr.	pounds	9.233
Steam per pound of combustible from and at 212 Fahr.	pounds	9.5977
Steam per square foot of heating surface per hour	pounds	1.692
Coal per square foot of grate surface per hour	pounds	6.944

The Storm water tank collecting the water from the roof of pump house was measured for delivery of feed to the boiler and found to be of following dimensions:

Diameter of tank	feet	10
Height of tank	"	9
Capacity	gallons	5,287.313
Capacity at 60 F.	pounds	44,043.31
Weight per foot of depth	"	4,893.66

In the following table are given the economic and capacity data from the several trials of boilers:

STEAM PER POUND OF COAL FROM TEMPERATURE OF FEED.

		Pounds.
First trial, North boilers (tubular) temperature feed	161.547	7.6648
Second trial, North boilers (tubular) temperature feed	153.87	8.414
Second trial, Main boilers (tubular) temperature feed	117.59	10.091
First trial, Main boilers (cylinder) temperature feed	152.73	6.746
Second trial, pump house (tubular) temperature feed	157.906	8.486

STEAM PER POUND OF COAL FROM AND AT 212 FAHR.

		Pounds.
First trial, North boilers (tubular)		8.349
Second trial, North boilers (tubular)		9.2554
2d trial, Main boilers (tubular)	Main	11.5209
1st trial, " (cylinder)	House	7.382
Second trial, Pump house (tubular)		9,233

STEAM PER POUND OF COMBUSTIBLE FROM AND AT 212 FAHR.

		Pounds.
First trial, North boilers (tubular)		9.123
Second trial, North boilers (tubular)		10.5259
2d trial, Main boilers (tubular)	Main	13.2934
1st trial, " (cylinder)	House	8.644
2d trial Pump house boilers (tubular)		9.597

COAL BURNED PER SQUARE FOOT OF GRATE PER HOUR.

	Pounds.
First trial, North boilers (tubular)	10.84
Second trial, North boilers (tubular)	5.0187
Second trial, Main boilers (tubular)	7.424
First trial, Main boilers (cylinder)	10.837
2d trial Pump house boilers (tubular)	6.944

STEAM PER SQUARE FOOT OF HEATING SURFACE PER HOUR.

	Pounds.
First trial, North boilers (tubular)......	1.778
Second trial, North boilers (tubular)...	0.9036
Second trial, Main boilers (tubular)....	1.575
First trial, Main boilers (cylinder)......	6.827
2d trial, Pump-house boilers (tubular)..	1.692

In the following table are recapitulated the rates per hour, at which the coal was fired in the several trials:

					Pounds.
1st trial,	North boilers,	all day,	1	fireman	693.75
"	"	day run,	1	"	675.
"	"	night "	1	"	712.5
2d trial,	"	all day,	1	"	321.2
"	"	day run,	1	"	286.516
"	"	night "	1	"	366.
2d trial,	Main tubular boilers,		2	"	1,236.1
1st trial,	Main cylinder boilers,		2	"	1,907.3
2d trial,	Pump house boilers,		1	"	125.

Omitting any changes calculated to diminish the consumption of steam for power, which the writer will discuss under the head of engine peformance, and assuming a production of steam in the north house midway between the work of first and second trials, and the production of steam in the main house the same as for second trial, of tubular boilers and first trial, of cylinder boilers, then the total daily steam from these plants, becomes—

		Pounds.
North house, August 20–21, 24 hours...		96,240
Main house, tubular, Aug. 27	10 hrs.	125,000
" cylinder, " 29	10 hrs.	128,600
Per diem, pounds steam...............		349,840

representing upon basis of trials, a consumption of coal per diem exclusive of that required to bank and start fires, of—

12,179.5 lbs., 24 hours, North house (tubular).
12,361.0 lbs., 10 hours, Main house (tubular).
19,073.0 lbs., 10 hours, Main house (cylinder).

43,613.5 lbs., per diem, pounds coal.

Add to this the coal required to start fires Monday morning north house, 2,500 pounds, equivalent to 420 pounds per day, the coal to bank and start fires night and morning daily. Main house, of 4,246 pounds (Aug. 30th), for tubular boilers, and 4,627 pounds (Aug. 29th), for plain cylinder boilers, then total daily consumption as per logs of trials, becomes 52, 906.5 pounds, or 26.5 tons in round numbers, for North house and Main house under assumed conditions of load and present conditions of performance for North house, and present conditions of load and performance for Main house, representing an annual cost of $26.5\times\$5.50\times300=\$43,725.00$.

Assuming that the boilers of North house are replaced with three six foot tubulars (same dimensions as of Main house), and that the sixteen cylinder boilers of Main house are replaced with six six-foot tubulars (same dimensions as present tubular boilers), then the consumption of coal per diem for same work as before with same temperature of feed as obtained by trial, will be—

For proposed three (3) new tubular boilers for North house temperature of feed.

$$\frac{161.547+153.87}{2}=157.708=158.4 \text{ true}$$

then heat,

$$\frac{(1210.33-107.746)\times10.091}{1215.49-158.4}=10.425 \text{ lbs.}$$

of steam per pound of coal upon evaporation of present six-foot tubular boilers, and $\frac{96{,}240}{10.525}=9{,}144$ pounds of coal per diem of 24 hours, exclusive of coal to start fires Monday morning, which should average 2,400 pounds or 400 pounds per day, distributed through the week; and total daily consumption will be 9,544 pounds.

For present six (6) Main tubular boilers as per trial (Aug. 27th) 12,361 pounds for the day of 10 hours, and 4,246 pounds for banking fires and steaming up in the morning, or a total of 16,607 pounds per diem of 24 hours.

For proposed six (6) new tubular boilers, to do the work now accomplished by the sixteen (16) plain cylinder boilers in Main house.

Temperature of feed to boilers 152.73 = 153.34 true heat then,

$$\frac{(1210.33-107.746)\times10.091}{1210.07-153.34}=10.529 \text{ lbs. of}$$

steam per pound of coal upon evaporation of present six-foot tubular boilers in Main house, and $\frac{128{,}600}{10.529}=12{,}214$ lbs. per day of 10 hours, and 4,246 pounds of coal for banking fires and steaming up in the morning, or a total consumption of 16,460 pounds per diem of 24 hours.

And total consumption of coal per diem 24 hours.

North boilers (3) tubulars,	9,544, steam	96,240
Main boilers (6) tubulars (present)	16,607, steam	125,000
Main boilers (6) tubulars (new)	16,460, steam	128,600
	42,611	349,840

or a total consumption per annum of $\frac{42,611}{2000} = 21,305 \times 300 = 6,391.5$ tons at \$5.50 per ton=\$35,153.25 or a saving of \$8,571.85 based on present work for Main house. and an increase over present work for North house of fifty per cent.

But it will appear upon investigation of the motive power, that changes can be made which will diminish the consumption of steam from Main house very materially, and increase the saving of fuel over above estimate by several thousand dollars per annum.

Motive Power.

The motive power of the new works is furnished by a Harris Corliss condensing engine, fitted with a pair of Gannon surface condensers, the condensation and air from which is removed by an independent air pump of the Deane pattern.

The power is taken from the pulley fly-wheel by two four-foot double leather belts, (running upwards of a mile a minute); to two main line shafts, from which it is distributed to the different parts of new works, principally by belts and pulleys, with two line shafts lying at right angles to main lines which are driven by bevel gearing.

The motive power of the old works is furnished by a pair of Harris Corliss non-condensing engines, running coupled, and connected by a spur gearing to the main line shaft.

The principal machinery driven by the condensing coupled engines is

- 4 Calendering machines
- 6 Washing "
- 8 Warming "
- 20 Grinding "

The principal machinery driven by the non-condensing coupled engines is

- 8 Calendering machines
- 8 Warming "
- 20 Grinding "

In the following table are given the dimensions of the condensing engine:

Dimensions Harris Corliss Engine.

Style		Condensing.
Diameter of cylinder	inches	36
Stroke of piston	"	72
Diameter of piston rod	"	5.25
Steam ports (each)	"	1.5×36
Exhaust ports (each)	"	2.875×36
Area, front side of piston	sq. inches	996.251
Area, back side of piston	"	1,017.9
Nominal revolutions per minute		50.
Nominal piston speed per minute, feet		600.
Actual piston speed, economy trial, Aug. 28	feet	601.8635
Factor of H. P., front side piston, nominal speed		9.057
Factor of H. P., back side piston, nominal speed		9.253
Piston stroke to release in parts of stroke		0.9888
Piston stroke from ex.'closure in parts of stroke		0.0902
Clearance in parts of stroke		0.025

Data from Economy Trial.

Piston displacement per hour to release	cu. feet	249,901.45
Piston displacement per hour to ex.' closure	cu. feet	22,796.44
Clearance volume, per hour.	"	5,054.64
Pulley fly-wheel diameter	feet	27.
Pulley fly-wheel face	inches	96.
Pulley fly-wheel weight	tons	39.

Surface Condenser, 36″×72″ Engine.

double dimensions.

25 rows of brass tubes, 8 tubes in each row.

Tubes 200.......... { 0.875″ outside diameter. 6 feet 1.5″ long.

Returns 25, radius of bend, 1.875″, diameter, 1.25 inches.
Returns 25, radius of bend, 4.25″, diameter, 1.00 inches.
Returns 25, radius of bend, 6.875″, diameter, 1.00 inches.
Returns 25, radius of bend, 8.75″, diameter, 1.00 inches.

Cooling surface in tubes	sq. ft.	280.525
Cooling surface, 100 returns	"	38.062
Total cooling surface, 1 condenser,	"	318.587
Total cooling surface, 2 "	"	637.174

Maker....Gannon, Jersey City, N. J.

Two trials for economy were made of the condensing engine: the first trial was unreliable by leakage of hose used to convey the discharge of the air pump to the weighing tank, and second trial is alone reported.

In the following table are given the principal data from second trial:

Harris Corliss, 36×72″, Condensing Engine.

Economy Trial, Aug. 28th.

Duration of trial	hours.	10

averages.

Average steam pressure in pipe	pounds	70.137
Average barometer	inches	30.194
Average barometer	pounds	14.822

Average vacuum condenser......inches	20.473
Average vacuum, condenser....pounds	10.050
Average temperature of injection, Fahr.	72.225
Average temperature of overflow, "	97.675
Average temperature of condensation"	104.037

ENGINE COUNTER.

Counter at 7.02 A.M..........	356.	
Counter at 12.00 M............	15,285.	
		14.929
Counter at 1.00 P.M	15,626	
Counter at 6.00 P.M............	30,688	
		15,062
Revolutions, 9 hours 58 minutes......		29.991
Revolutions per minute..............		50.155
Piston speed........................		601.8635

FROM THE DIAGRAMS.

Average initial pressure,........pounds	68.795
Average terminal pressure absolute, "	8.933
Average counter pressure at mid-stroke, absolute ....pounds	5.383
Average vacuum realized........ "	9.439
Av. mean effective pressure { front "	21.069
Av. mean effective pressure { back "	21.699

LOAD.

Average indicated horse power	392.862

RATIO OF EXPANSION.

Apparent cut-off in parts of stroke....	0.084
Actual cut-off in parts of stroke.......	0.104
Theoretical cut-off....................	0.10895
Expansion by volumes................	9.718
Expansion by pressures...............	9.36

CALORIMETER.

Average water heated,..... ..pounds	200.
" steam condensed.... "	10.162
" initial tem'ture, Fahr.....79.45=	79.489
" final " " ..130.150=	130.481
" thermal units found in steam	1,134.020
Thermal units in steam at observed pressure......................	1,210.33
Difference............................	76 309
Latent heat of steam at observed pressure........................	891.29
Per-cent. of water entrained in steam.	8.561

ECONOMY.

Total water weighed from condenser.............. .....pounds	82.088
Actual saturated steam to engine "	75,103.426
Net steam to engine per hour. pounds	7,510.342
Steam per ind. H.P. per hour "	19.095
Coal per ind. H.P. per hour, based upon main tubular boilers .	1.892
Per-centage of steam accounted for by indicator..................	74.048
Steam per indicated H. P. per hour by diagrams	14.139

SURFACE CONDENSER.

Steam condensed per sq. foot of cooling surface per hour............pounds	11,787

The economy of the engine is excellent, but is scarcely up to the standard of Harris Corliss' engines. With an increase of load to about 600 horse power, the writer is of the opinion that the consumption of steam per horse power will be reduced about fourteen per cent.

With 80 pounds pressure in the pipe, and a mean initial pressure of 79 pounds, the mean effective pressure with a cut off at 12 per cent. of the piston's stroke, and vacuum of 26 inches, would be about 33 pounds, equivalent to a load of 600 horse power in round numbers. The terminal pressure under these conditions will be 13 pounds absolute, and counter pressure 3.5 pounds absolute, corresponding to a consumption of stean per hour by the diagrams of 8,367 pounds, or 13.995 pounds per indicated horse power; under the proposed conditions, I expect the diagrams to account for not less than 85 per cent. of net steam delivered to engine, whence actual consumption of steam per horse power per hour would be 16.416 pounds, and gain over present economy would be $1 - \frac{16.416}{19} = \frac{1365}{10000}$ or nearly *fourteen* per cent.

The estimated economy upon a load of 600 horse power is within personal precedent from Harris Corliss' condensing engines, and can be obtained, and possibly improved, by the Rubber Co's. engine.

The writer's belief is, that with a load of 600 horse power, pressure of 80 pounds in pipe at engine, and a vacuum of 26 inches in condenser, the economy will excel the best he has ever obtained from condensing single cylinder engines, and he has placed the gain at fourteen per cent. as a maximum to be expected.

The expenditure of condensing water for the 36″×72″ engine has been roughly estimated in the following manner:

Thermal units taken up per pound of condensing water 97.675−72.225=25.45 thermal units per pound of steam, difference between observed total temperature of steam as it entered engine and as it left the condenser 1,134.02−104.037 =1,029.983, of which quantity the heat converted into work, and that lost by radiation and conduction during expansion in the cylinder, represents about ten per cent. (all heat converted into work or lost by conduction and radiation prior to cut off being supplied direct from boilers), leaving 926.98 thermal units to be taken up by condensing water, corre-

sponding to 36.42 pounds per pound of steam condensed; the weight of steam condensed per hour was 8,208.8 pounds, and weight of condensing water due this quantity of steam was 299,964 pounds, or 36,010 gallons, or about 68 per cent. of capacity of Worthington pump at wharf based upon delivery during the duty trial (53,790 gallons per hour), 36,000 gallons of sea water pumped 100 feet high equals 3.6 million gallons one foot high, and will cost for fuel alone, not including coal to start fires, \$0.216 or $21\frac{6}{10}$ cents for 393 horse power, or at rate of \$2.16 per day of ten hours, or 1.82 horse power per *one* cent of cost. The coal for ten hours per horse power is 0.25 bushels, and costs $5\frac{1}{2}$ cents or a total cost per horse power per diem (10 hours) of 6 cents in round numbers for fuel.*

The writer is of the opinion that few establishments, in or out of the United States, can exhibit a similar economy for motive power.

In the following table are given the dimensions of the non-condensing coupled engines.

HARRIS CORLISS' NON-CONDENSING ENGINES.

Two coupled cranks set on one shaft at quarters.

RIGHT ENGINE.

Diam. of cylinder............inches,	28
Stroke of piston.............. "	48
Diam. of piston rod.......... "	4.125
Area front side of piston......sq. in.,	602.386
Area back side of piston...... "	615.75
Nominal revolutions per minute......	70.
Nominal piston speed per minute..ft.,	560.
Actual piston speed economy trial Aug. 30th....................ft.,	555.344
Factor horse power front side piston nominal speed	5.111
Factor horse power back side piston nominal speed..................	5.224
Piston stroke to release in parts of stroke..........................	0.9906
Piston stroke from exclosure in parts of stroke..................	0 0489
Clearance in parts of stroke (estimated)...........................	0.025

DATA FROM ECONOMY TRIAL.

Piston displacement per hour to release..................cu. ft.,	137,317.49
Piston displacement per hour to exclosure..................cu. ft.,	6,778.54
Clearance volume per hour.. "	3,465.51

*Coal for steam per H. P. per hour..1.90 pds., \$0.0055
Coal for Worthington pump at wharf to supply condensing water per H. P, per hour, ...0.2 pounds, 0.00055
Total coal 21 pounds per 10 hours............ 0.0605
or say, in round numbers six cents....... 6

LEFT ENGINE.

Diam. of cylinder...........inches,	26.
Stroke of piston.............. "	48.
Diam. of piston rod.......... "	3.875
Area front side of piston...sq. inches	519.137
Area back side of piston .. "	530.93
Nominal revolutions per minute......	70.
Nominal piston speed " " ...ft.,	560.
Actual piston speed economy trial Aug. 30th.........................ft.,	555.344
Factor horse power front side piston nominal speed....................	4.405
Factor horse power back side piston nominal speed....................	4.505
Piston stroke to release in parts of stroke.........................	0.9926
Piston stroke from exclosure in parts of stroke	0.0448
Clearance in parts of stroke (estimated).....	0.025

DATA FOR ECONOMY TRIAL.

Piston displacement per hour to release............. .....cu. ft,	120,588.04
Piston displacement per hour to exclosure........... ...cu. ft.,	5,442.62
Clearance volume per hour.. "	3,037.17
Fly wheel diam. ....ft.,	20.
" " weight..............tons,	18.5

One trial for economy of coupled engines was made Aug 30th, under ordinary conditions of work in the old mill, with the following results:

ECONOMY TRIAL COUPLED ENGINES.

Date of trial....................	August 30th.
Duration of trial...............	10 hrs. 45 min.

PRESSURES.

Average pressure in pipe......pounds	68.488
Barometer estimated..........inches,	29.9
" " ....pounds,	14.678

ENGINE COUNTER.

Counter at 7.01: A. M..........17,917	
Counter at 6: P. M.... 62,622	
Revolutions for 10 hours 44 minutes..	44,705
Revolutions per minute.............	69.418
Piston speed......................ft.,	555.344

FROM THE DIAGRAMS 28″×48″ ENGINE.

Average initial pressure......pounds,	60.415
Av. terminal pressure absolute "	13.094
Av. counter pressure above atm. "	0.914
Mean effective pressure front side of piston...... ....pounds,	14.915
Mean effective pressure back side of piston .... pounds,	13.954
Indicated horse power...............	147.886

FROM THE DIAGRAMS 26″×48″ ENGINE.

Average initial pressure......pounds,	60.953
Av. terminal pressure absolute "	15.794
Av, counter pressure above atmosphere.................. pounds,	1.787
Mean effective pressure front side of piston .... pounds,	23.484
Mean effective pressure back side of piston ....pounds,	22.026
Indicated horse power.............	200.980
Total load average all day H. P.......	348.875

RATIO OF EXPANSION 28″×48″ ENGINE.

Apparent cut-off in parts of stroke.....	0.1299
Actual " " " "	0.155
Theoretical " " " "	0.1743
Expansion by volumes...............	6.613
" " pressures.............	5.735

RATIO OF EXPANSION 26″×48″ ENGINE.

Apparent cut off in parts of stroke....	0.235
Actual cut off in parts of stroke........	0.26
Theoretical cut off in parts of stroke....	0.209
Expansion by volumes	3.942
Expansion by pressures................	4.788

CALORIMETER.

Av. water heated............pounds,	200.
Av. steam condensed........pounds,	10.159
Av. initial temperature, Fahr...77.386=	77.421
Av. final temperature, Fahr..129.75=	130 079
Av. thermal units found in steam,....	1166.759
Thermal units in saturated steam at observed pressure	1209.82
Difference thermal units............	43.061
Latent heat of steam at observed pressure..........................	892.49
Percentage of water entrained in the steam	4.824

TOTALS.

Total water weighed to boilers..lbs.	125,745.
Total water and steam to calorimeter................ ...pounds,	234.5
Total water and steam to engine lbs.	125,510.5
Total saturated steam to engine lbs.	119,455.9
Estimated steam to machine-shop engine.................pounds	8,806.0
Net steam to coupled engines "	110,649.9

ECONOMY.

Steam per hour........... pounds,	10,293.014
Steam per hour per indicated H. P ....pounds	29.503
Coal per horse power per hour upon performance of tubular boilers, Main House..........	2.924

STEAM ACCOUNTED FOR BY DIAGRAM.

By 28″×48″ engine per hour..pounds	4,377.6
By 26″×48″ engine per hour.. "	4,712.5
By both engines .... "	9,090.1
Percentage accounted for	88.313
Steam per indicated H. P. per hour by diagrams.... ....pounds	26.054

Of above load, 46.285 horse power is due to friction of engine and load, and 302.59 horse power represents average load of machinery, including friction of line shafting and gearing, which would be the net average load if the old works were driven by the condensing engine.

The aggregate average load in old and new works, now driven by condensing (36″x72″) engine, and non-condensing coupled engines, is 741.737 horse power, and the aggregate maximum load in the old and new mills from accompanying tables of power per diem, is 949.551 horse power.

In providing for capacity of engine the maximum possible loads should be considered, and in prospecting for economy, the average loads should be considered.

Suppose the total load of old and new mills is divided between the 36″x72″ condensing engine, and the 28″x48″ engine (made condensing), and that the loads are proportioned for best mean effect for the two engines.

Calling the whole load 100, then of this there should be carried by the 36″x72″ engine 64 per cent., and by the 28″x48″ engine 36 per cent.; or if the 36″x72″ and 26″x48″ were run together, which (with the two engines in same condition), is to be preferred for economy; then 68 per cent. of the load to the 36″x72″ engine, and 32 per cent. to the 26″x48″ engine, both engines condensing and producing as nearly as possible similar diagrams.

The writer's recollection now is that the 28″x48″ engine is in much the better condition of the coupled engine, and would probably be a better engine to carry the proportional part of total load.

Of the aggregate average indicated power of condensing and coupled engines, 32.042 horse power is the friction of the 36″x72″ engines running without load at mean speed, 18.086 horse power is the friction of 28″x48″ engine running without load at mean speed, and 15.592 horse power is the friction of 26″x48″ engine under similar conditions.

If the 26″x48″ engine is stopped, then of the aggregate average load (7.42 horse power in round numbers) 15.6 horse power of load would be removed, leaving 726.4 horse power to be furnished jointly by the 36″x72″ engine, and 28″x48″ engine, of this the former engine should carry 465 horse power, as a mean load, and 600 horse power as a maximum load, and the 28″x48″ engine should carry 261.4 horse power as a mean load, and 335 horse power as a maximum load (while working condensing). Under these conditions both engines would be so moderately loaded as to warrant excellent wear through many years of continuous service.

The consumption of steam for power alone under proposed arrangement, will be 12,348.8 pounds per hour, or 123.481 pounds per diem of ten hours, correspond-

ing to a consumption of coal, upon basis of 72″ tubular boilers in main house of 12,237.4 pounds, or 6.1187 tons.

The consumption of steam from main house at time of trials, August 27th to September 4th, was:

From 6 tubular boilers, 124,729.66 pounds.

From 16 plain cylinder boilers, 128,657.50 pounds.

Total steam for 10 hours, 253,405.16 pounds.

Representing a coal consumption for same time of 12,361 pounds for tubular boilers, and 19,073 pounds for plain cylinder boilers, or a total per diem of 31,434 pounds, or 15.717 tons, exclusive of coal to bank and start fires.

Of above quantity of steam as at present working, the 36″x72″ condensing engine consumed (August 28th) 75,103,426 pounds per day (10 hours), the coupled engines, as run August 30th, consumed 110,650 pounds per day, the machine shop engine consumed 8,806 pounds per day, and the steam pumps and various heating and curing apparatus taking steam from main boilers, consumed the remainder, or 58,845.73 pounds per day; of this the consumption by pumps alone will not exceed 10,000 pounds per day, leaving consumption of steam by heating, devulcanizing and curing apparatus, as at date of trial, of 48,850 pounds in round numbers.

The hourly quantities which is the better method of stating steam consumption, is:

For 36″ × 72″ engine	392.86 H. P.	7,510.34 lbs.
For coupled engines	348.87 H. P.	11,065.00 "
For machine-shop engine.......	31.45 H.P..............	880.60 "
For steam pumps	{2 boiler feeders} {1 fire pump....}	1,000.00 "
For heating rolls, devulcanizing and curing apparatus......		4,884.573 "

The above, as working at dates of trial, represented an annual (300 days) consumption of fuel for all purposes (banking and starting fires, and running), of 6,046.2 tons, or an expenditure of $33,254.

Under proposed arrangement with 28″x48″ engine, condensing, the expenditure of steam per hour will be:

For 36″ × 72″ engine	465 H P...	7,905.0 lbs.
For 28″ × 48″ engine	261.4 H.P....	4,443.8 "
For 10″ × 24″ engine	31.45 H.P....	880.6 "
For 3 steam pumps	{reduced duty} {of boiler feeders}	800.0 "
For heating, devulcanizing and curing..........................		4,884.5 "

or a total consumption of steam per day of 10 hours, of 189,285.5 pounds, equivalent to an expenditure of 18,758 pounds, or 9.379 tons of coal, and an annual (300 days) consumption of 2,813.7 tons at a cost of $15,475.35 running time, to which add coal for banking and starting fires, as follows:

For present six tubular boilers per day...	4,246 lbs.
For proposed 3 additional tubular boilers to take the place of present 16 plain cylinder boilers......................	2,123.0 "
Total.....................	6,369.0 "

or 3.184 tons per day, or 955.2 tons per annum, costing $5,253.60, making a total cost for fuel under proposed arrangement of $20,728.95 in main boiler house.

To recapitulate the proposed changes, contemplate:

First—The removal of the 16 plain cylinder boilers, and substitution of 3 tubular boilers of same dimensions as present tubular boilers, *videlicit*, 72″ diameter of shell 15′ long, with 100-3″ tubes to be set, and otherwise arranged as present (6) tubular boilers.

Second—The addition of a condenser to the 28″x48″ Harris Corliss non-condensing engine.

Third—The division of the whole motive power between the 36″x72″ condensing engine, and the 28″x48″ condensing engine, in the proportion of 64 per cent. to the former, and 36 per cent. to the latter, making a difference in cost of power between arrangement as at date of trial, and proposed arrangement of $12,526.05 per annum, from which we must deduct the extra cost of fuel at pump house to supply condensing water to condensers of 36″x72″ engine, and 28″x48″ engine, which upon basis of estimate for 36″x72″ engine with present load (August 28th) will be $550, leaving a net difference of $11,976.05. By substituting for the present 5-48 inch tubular boiler in the North house, 3.72 inch tubular boilers of same size as the tubular boilers in the Main house, and estimating the average requirements of said boilers to be midway between the ordinary work as per trial of August 21st, and the maximum consumption as per trial of August 20th and 21st, then

the difference in cost of steam in favor of the 3-72-inch boilers will be $2,524.50, and a total possible improvement (in sight) of economy of $14,500.55.

The suggested improvement in boiler economy is based upon precedent in the Rubber Company's own works, and the suggested improvements in engine economy is based upon personal experience with Harris Corliss and other cut-off steam engines.

The testing of the 10″x24″ machine-shop engine was incidental to the test of coupled engines, and furnishes valuable information only in the power developed, and probable cost of operating the machinery of machine-shop and box-factory.

Machine-Shop Engine.

DIMENSIONS.

Diam. cylinder	inches	10
Stroke of piston	"	24
Diam. of piston rod	"	1.9375
Diam. of fly wheel	feet	7.5
Width of fly-wheel face	inches	16.
Weight of fly wheel	pounds	3880.
Clearance estimated	per cent.	3.
Area front side piston	sq. inches	75.592
Area back side piston	" "	78.540
Estimated revolutions per minute		100.
H. P. front side piston		.458
H. P. back side piston		.476

The mean speed during trial was 100.-749 revolutions per minute, and mean effective pressure front end of cylinder, 28.057 pounds, and for back end of cylinder, 38.583 pounds, equivalent to a mean load of 31.45 horse power. The estimate of 28 pounds of steam net, per horse power per hour, is justified by the general condition of the engine and the load carried, and any error in estimating the economy of this engine is calculated to enhance rather than diminish the economy of coupled engines, which were tested simultaneously with this engine.

The power developed by the machine-shop engine is relatively so small, that an error of several pounds in steam consumption per horse power per hour, might subsist, with no practical effect upon the economy of coupled engines.

Distribution of Power.

Subsequent to the trials for economy of engines, tests were made upon 36″x72″ condensing engine, and 28″x48″ non-condensing engine for distribution of power, with the following results:

Distribution of Power 36″×72″ Engine.

New Works.	H. P.
A load, average as at 2 P.M. Aug. 31.	415.316
B load consisting of 4 calenders, engine, line shafting and empty rolls.	118.207
C load, consisting of 3 calenders, engine, line shafting and empty rolls.	103,510
D load, consisting of 2 calenders, engine, line shafting and empty rolls.	98.528
E load, consisting of 1 calender, engine, line shafting and empty rolls.	82.193
F load, consisting of engine, line shafting and (44) empty rolls	77.000
G load, consisting of 8 warmers, engine, line shafting and empty rolls.	234.84
H load, consisting of 30 grinders, engine, line shafting and empty rolls.	502.352
I load, consisting of 6 washers, engine, line shafting and empty rolls	114,959
J load, consisting of 10 grinders, engine, line shafting and empty rolls.	198.004
K load, consisting of 10 grinders, engine, line shafting and empty rolls.	259.096
L load, consisting of 10 grinders, engine, line shafting and empty rolls.	238.297

The friction of empty engine at mean speed the writer has estimated at 1.75 pounds per square inch of piston, or 32.042 H. P. Deducting this amount from any of above loads, gives the gross load on engine, which consists of the machinery driven and extra friction of engine due to load; as the machinery cannot be operated without developing this extra friction, the writer thinks it is proper to charge it to the machines as part of the power required to drive them; of course the actual power consumed at the machine is as much less than that shown as is represented by extra friction of engine and connectors (line shafting and gearing), due to load; but if the load of machine is removed this extra friction disappears, and as in any particular instance, this extra friction is due to the load of machine, the writer thinks best to charge it as necessary part of the power to drive machine.

With this explanation the preceding table shows:

First—That the frictional resistance at mean speed of line shafting and gearing, forming the connectors between engines and machinery, and of the rolls running empty in new works requires, 43.159 horse power.

Second—That the average load (all machinery in new mill), exclusive of engine friction and extra friction of engine due to load, is 367.943 horse power.

Third—That 4 calenders require, 41.207 horse power divided as follows:

	H. P.
1st calender, as per supt's mem......	14.697
2d " " " "	4.982
3d " " " "	16.335
4th " " " "	5.193
Average per calender.............	10.302
Fourth, that 8 warmers require........	157.84
Average per warmer.............	19.73
Fifth, that 30 grinders require.........	425.352
Average per grinder.............	14.178
Sixth, that 6 washers require..........	37.959
Average per washer.............	6.326
Seventh, that 10 grinders require......	121.004
Average per grinder.............	12.100
Eighth, that 10 grinders require.......	182.096
Average per grinder.............	18.210
Ninth, that 10 grinders require........	161.297
Average per grinder.............	16.130

The average for last three loads, or loads J, K and L is 15.48 horse power per grinder, while the average for same machines all on load A, shows 14.178 H. P. per grinder, the difference must have been due to different conditions of work in the two cases; that is, the grinders when run in gangs of ten were more carefully charged with gum, than when operated as a whole in load H.

Distribution of Power 28″ × 48″ Engine.

OLD WORKS.

	H. P.
A load, average at 2:00 P.M., Sept. 4th.	401.541
B load, consisting of 8 calenders, engine gearing, line shafting and empty rolls..................	119.482
C load, consisting of 7 calenders, engine gearing, line shafting and empty rolls..................	112.475
D load, consisting of 6 calenders, engine gearing, line shafting, empty rolls..................	105.819
E load, consisting of 5 calenders, engine gearing, line shafting, empty rolls..................	102.188
F load, consisting of 4 calenders, engine gearing, line shafting, empty rolls..................	100.407
G load, consisting of 3 calenders, engine gearing, line shafting, empty rolls..................	87.761
H load, consisting of 2 calenders, engine gearing, line shafting, empty rolls..................	76.222
I load, consisting of 1 calender, engine gearing, line shafting, empty rolls..................	61.609
J load, consisting of 8 warmers, engine gearing, line shafting, empty rolls.	213.938
K load, consisting of 10 mixers, engine gearing, line shafting, empty rolls.	176.908
L load, consisting of 10 mixers, engine gearing, line shafting, empty rolls.	151.780
M load, consisting of 20 mixers, engine gearing, line shafting, empty rolls.	288.498
N load, consisting of 8 warmers and 20 mixers, engine gearing, line shafting, empty rolls..................	389.207
O load, consisting of engine gearing, line shafting, and (28) empty rolls	54.421

The order in which the calenders were tested for power, was:

1st. Machine taken off.	1st. Empress calender.	
2d. " "	2d. "	"
3d. " "	Sole	"
4th. " "	1st. 3 roll sheet	"
5th. " "	2d. "	"
6th. " "	4 roll sheet	"
7th. " "	4 roll cloth	"
8th. " "	Friction cloth	"

The preceding table shows:—

First—That the frictional resistance at mean speed of gearing and line shafting, forming the connectors between engine and machinery of old works and of the rolls running empty, requires, 34.883 horse power, as against 43.159 horse power, for same losses in new works, but the new works contain a greater number of rolls permanently connected with line shafting, and a greater length of line shafting. (The number of rolls always driven in the old works is 28, and omitting difference of line shafting in the two cases, the average frictional resistance of gearing line shafting and empty rolls, per roll, is 1.246 horse power and for new works with 44 rolls always driven is 0.9809 horse power.

The power is transmitted in the old works to the line shafting by spur gearing, and in the new works by belting and the demonstration in the preceding paragraph shows conclusively that the loss of effect between engine and machinery is least with the belting connectors.

The fact is well established by numerous experiments however in flour mill practice, that the loss of power by frictional resistance of gearing is much greater than with belting, and no gearing which can be avoided is used in the large mills of the West to-day.

	H. P.
Second, that the average load (all machinery in old mill on), exclusive of engine friction, and extra friction of engine due to load, is.....	333.234
Third, that 8 calenders require...... divided as follows:	65.060
1st Empress calender................	7.007
2d " "	6.656
Sole " "	3.631
1st 3 roll sheet calender..............	1.781*
2d 3 " " "	12.746
4 " " "	11.539

* It is to be presumed there is an error in the power for first 3 roll sheet calender; where it subsists the writer cannot tell, but it is due, in all probability, to changes in the load of the machines on, at time *E* and *F* loads were indicated.

4 roll cloth calender	14.613
Friction cloth calender	7.187
Fourth, that 8 warmers require	159.516
Average per warmer	19.940
Fifth, that 10 mixers require	122.486
Average per mixer	12.248
Sixth, that 10 mixers require	97.358
Average per mixer	9.736
Seventh, that 20 mixers require	234.076
Average per mixer	11.703
Eighth, that 8 warmers and 20 mixers require	334.785
Average per machine	11·957

In considering the tables of power obtained as these were, with work going into and out of the machines, and with possible variations of stock requiring more or less power, it should be understood that the results can only be approximative as at the particular time when diagrams were taken, and in some instances the power per machine must be above the average and in other instances below the average. Taken as a whole, however, it is believed the tables will prove of value.

Worthington Pumping Engine.

The engine is located in a neat pump house on the wharf, and delivers sea water into a large iron tank in the yard of the factory to supply the circulation to the engine condensers, water to urinals and hoppers, and if required, to furnish water for fire purposes.

The steam end of the engine is compound condensing fitted with a Lighthall surface condenser, taking circulating water from the force pump on one side of the condenser and returning the overflow to the force pipe on the opposite side of condenser, with a regulating valve in the force pipe intermediate of the injection and overflow pipes, aftera system patented by the writer Sept. 6th, 1883.

The water end consists of two double acting plunging pumps with central solid packing glands.

In the following table are given the principal dimensions of engines and pumps:

Dimensions of Pumping Engine.

Builder....H. R. Worthington, N. Y.

DUPLEX COMPOUND CONDENSING.

Diam. H.P. cylinders (2)	inches	12
Diam. L.P. cylinders (2)	"	18.5
Stroke H.P. pistons (2)	"	10
Stroke L.P. pistons (2)	"	10

DOUBLE ACTING PLUNGER PUMPS, CENTRAL GLAND.

Diam. of plungers (2)	inches	14
Nominal stroke of plungers (2)	"	10
Contact stroke of plungers (2)	"	10.375
Diam. pump rods, front (2)	"	1.875
Diam. pump rods, back (1) right.	"	1.25
Diam. pump rods, back (1) left	"	1.375
Calculated discharge per quadruple stroke	U. S. gallons	26.3
Diam. suction pipe	inches	8
Diam. discharge pipe	"	10

Condenser Surface.

Builder....Wm. Lighthall, N. Y.

The preliminary test of the Worthington pumping engine was for capacity, or more properly speaking for relation of actual to calculated delivery of pumps. To this end the iron tank in the factory yard, which receives the discharge of pumps, was carefully measured for capacity, with the following results:

Internal diam. mean of 4 measurements	feet	20.08
Total vertical depth	12′	10.875
Net vertical depth to overflow pipe.	12′	7.5
Total contents in cu. feet		3,998.06
Total contents in U. S. gallons		29,905.51
Contents per vertical foot.	U.S. gals.	2368.75

The calculated delivery of pumps for nominal stroke, I have estimated in the following manner:

	Sq. in.
Area 14-inch plunger	153.94
Area of 1.875-inch plunger rod	2.7612
Area of 1.25-inch air pump rod, R engine	1.2272
Area of 1.375 feed pump rod, L engine	1.4849

Mean area of plunger right engine

$$\frac{(153.94-2.7612)+(153.94-1.2272)}{2}=151.9458$$

Mean area of plunger left engine

$$\frac{(153.94-2.7612)+(153.94-1.4849)}{2}=151.817$$

and mean area both plungers

$$\frac{151.9458+151.817}{2}=151.8814$$

And delivery of both pumps per revolution, or for four (4) single strokes of 10 inches each

$$\frac{151.8814\times 40}{231}=26.3 \text{ U. S. gallons.}$$

The tank in yard was drawn down until the head of water measured from a given datum in bottom of tank, $5\frac{3}{16}$ inches=0.4323 foot when all outlets were carefully closed, and head pumped up to overflow, or 12.525 feet, showing an increase of head of 12.1927 feet, corresponding to an addition of 2,368.75×12.1927=28,881.48 gallons. Meanwhile the tank was filling the engine counter

was read at intervals of five minutes, and head in tank noted for corresponding intervals, the observers' watches at pump house and tank being first made to agree, and directly the water in tank broke over the edge of overflow pipe, the observer at tank noted the time to seconds, from which is obtained by log of engine counter 1103.6 quadruple strokes of engine, corresponding to interval of time during which head in tank was raised from 0.4323 foot to 12.1927 feet, or a plunger displacement based upon nominal stroke of $26.3 \times 1103.6 = 29{,}024.68$ gallons, showing a delivery of $\frac{28{,}881.48}{29{,}024.68} \times 100 = \frac{995}{1000}$ of plunger displacement, or a loss of action of .5 of 1 per cent. As a matter of fact the left pump actually worked to contact stroke, or 10.375 inches instead of 10 inches, while the right pump fairly worked to nominal stroke, whence the plunger displacement for above delivery was 29,662.26 gallons, and the loss of action really 2.59 per cent.

The loss of action is low, not so low as the writer has seen reported for some large Worthington engines, but likely as low as has ever been obtained when plunger displacement is based upon actual instead of nominal stroke. In this case the contact stroke is 3.75 per cent. greater than nominal stroke, this being the double clearance of steam cylinders, and in all those cases of losses of action under 2 per cent. for direct-acting engines, the writer is quite sure the nominal stroke has been exceeded by all or part of the clearance, and the calculated delivery has been based upon nominal stroke, which in such instances was short of true stroke, and leads to a false statement of pump efficiency.

The writer makes this statement not in criticism of the pump at the Rubber Company's works, but in criticism of the practice of builders of this class of pumping machinery, claiming an efficiency of water discharge which the writer is sure is incorrect, and calculated to mislead their patrons.

The trial for duty of Worthington pumping engine was made September 3rd, with the following results:

DUTY TRIAL WORTHINGTON PUMPING ENGINE.

Date of trial	Sept. 3
Duration of trial ... hours	8

GENERAL OBSERVATIONS.

Average steam pressure ... pounds	67.422
" temperature of injection. Fahr.	66.625
" " " overflow "	91.25
" " " hotwell "	107.437
" " " air "	80.531
" vacuum ... inches	22.219
" barometer ... "	29.935

HEAD PUMPED AGAINST.

By water-pressure gauge on force pipe ... pounds	36.182
By diff. tide in harbor and center water-pressure gauge ... pounds	7.388
Allowance for extra frictional resistance ... pounds	1.000
Total head ... "	44.57
Total head ... feet	102.868
Mean area of plungers ... sq. inches	151.8814
Moment of water load ... inch pounds	6769.354

ENGINE COUNTER.

Counter at 8:00 A. M.	1543.
Counter at 4:00 P. M.	17905.
Revolutions in 8 hours	16362.
Total plunger travel ... feet	54540.
Foot-pounds work for 8 hours	369,200,567.05

TOTALS.

	Pounds.
Total water pumped into boiler	10,302.152
Total water and steam to calorimeter	243.2
Equivalent coal to calorimeter	23.607
Total coal to boiler	1000.
Net coal to engine	976.393
Duty of engine per 100 pounds of coal ... ft.-pounds	37,812,701.14
Average temperature of feed water ... 157.906	158.595
Steam per pound of coal from feed	8.486

It will be observed from the table, that with an expenditure of 976.393 pounds of coal during running time, the engine and pumps made 16,362 quadruple strokes, or 16.76 strokes per pound of coal, equivalent to a delivery of 438 gallons of water into the tank at the works; and the corresponding delivery per ton of coal (running time), 877,200 gallons, or 90.236 million gallons 1 foot high at a cost of 6 cents per million gallons for fuel alone.

The probable daily consumption of coal in starting fires, will be 500 pounds, the amount of which should be kept separate from that used in running.

The cost of engineer, coal for starting fires, and interest on investment, being a constant quantity independent of water pumped per diem, it is obvious that as the demands upon the engine increase the economy of performance will increase

also, the writer does not mean that the duty of engines based upon coal burned during the running time will be enhanced, but that the duty based upon all expenses reduced to a coal basis will be increased.

By separating the weights of coal (daily) to start fires, from the weights of coal burned for running time, it can be determined with a slight calculation, how well the pumping house is managed in the following manner: If a division of the coal used for starting fires (for any length of time), by the number of days is in excess of 500 pounds per diem, and the number of quadruple strokes (revolutions) by counter for same time divided by the coal burned (running time), is less than 16.76 per pound, then the works are not well managed.

In other terms the standard for coal per day for starting fires should be 500 pounds, and the standard of quadruple strokes of pumps per pound of coal burned while pumping water, should be 16.76; which standards may be exceeded or improved by skillful management upon the part of the engineer.

The air pump furnished with the Worthington air pump engine is insufficient to properly void the condenser of air and condensation, and another should be supplied with ample capacity to keep in advance of the actual maximum requirements.

During the progress of the writer's discussion of the results, he has suggested several improvements which were used only to illustrate the particular portion of plant under consideration, and were not intended as improvements which should be inaugurated in fact, *e. g.*, the change in setting of present tubular boilers in North house, and substitution of six 72″ tubular boilers for present plain cylinder boilers in Main house.

The improvements which the writer submits to the Rubber Co's consideration, are:

The removal of the plain cylinder boilers entirely, and substitution of three (3) 72″ tubular boilers instead.

The addition of condenser to 28″×48″ engine, arranged to work with 26″×48″ engine if desired.

The stoppage of 26″×48″ engine entirely.

The division of load between 36″×72″ engine, and 28″×48 engine in the proportion of 64 per cent to the former, and 36 per cent to the latter.

The substitution of three (3) 72″ tubular boilers for the present 5—48″ tubular boilers in North house.

By making these changes in a proper manner the writer confidently believes the Rubber Co. will reduce the coal bills $15,000 a year, besides a reduction of labor in wheeling and handling coal.

There are, no doubt, other sources of loss about the works in the consumption of steam, which partly for lack of time, and partly because of the high normal temperatures during his visit, were not investigated.

It is advisable to put a permanent pyrometer in the flue leading to chimney of each battery of boilers, also to furnish convenient dormant platform scales in North house, Main house and Pump house, for the actual weighing of coal, and the coal should be reported so much for banking and starting fires and so much for running. The writer would also have an indicator permanently attached to cylinder of 36″×72″ and 28″×48″ engines, for periodical diagrams to be read for load and economy.

The 28″×48″ engine should be furnished with a permanent counter for daily report of revolutions, and daily reports of counters from 36″×72″ engine, and from Worthington Pumping engine, should be had to check the economy at the several points of coal consumption or steam use. The night men in Main boiler house, and each fireman in North house should be required to brush the boiler tubes each night and at end of each watch respectively.

The foregoing narrative which is an extract from the report to the Rubber Co., may be crude in form and diction, but is, nevertheless, valuable as showing how meritorious improvements may be made in the steam power plants of large establishments, where no losses are supposed to exist. The engines and boilers of the National Rubber Co. are, strictly speaking, first class, yet with precedents from its own works, it appears that the consumption of fuel is $\frac{1}{3}$ more than it should be for equal effects. What is shown to be the losses of this concern, may be repeated, ad infinitum, in many of the large establishments of the country using steam power.

ON THE BEST METHOD OF RAPIDLY CONSTRUCTING LONG TUNNELS.

By G. BRIDEL.

From Selected Papers of the Institution of Civil Engineers.

THE facts from which the author draws his deductions relate principally to the Mont Cenis, St. Gothard, and Arlberg tunnels, a comparison being made between the modes of tunnelling adopted, divisible into two systems, viz., first, that of driving the advance-heading near the level of the roof of the intended tunnel; and secondly, where it is driven at the invert-level. The comparative merits of these two methods are reviewed, as regards speed of execution, suitability under various conditions, and economy.

In the St. Gothard tunnel, where the first-mentioned or "crown-heading" (Belgian) system was adopted, the piercing was effected in much less time than in the case of the Mont Cenis, where the advance-heading was driven at the low level, or on what may be termed the "base-heading" (English) system. This advantage, however, was neutralized by the fact, that after this stage had been attained the completion of the Mont Cenis tunnel was effected in nine months, as compared with twenty-two months in case of the St. Gothard.

The author attributes this discrepancy to the facilities afforded by the "base-heading" system, in the subsequent excavation and transport, and the non-necessity for the frequent shifting of the temporary running-road and the air-conduits.

At the Arlberg tunnel, commenced about two years ago, the base-heading system has been adopted, with the result that although the rate of advance of the heading is 50 per cent. more than that attained at the St. Gothard, the completion of the tunnel section follows as close upon the heading as was the case at Mont Cenis.

The mode of procedure in this instance was to excavate a series of vertical shafts (about three chains apart) from the roof of the heading upwards, until reaching the level of the crown of the intended arch; the upper portion of these shafts was then widened out, the excavation being shot, as through a funnel, into the wagons on the base-heading track.

On the 31st December, 1881, the east heading of this tunnel had been driven for a distance of 2,031 yards, and the length of the finished tunnel was 1,170 yards, or 861 yards in arrear of the former. In July, 1882, the lengths of completed tunnel and heading were 2,544, and 3,288 yards respectively, the difference being 744 yards, or 117 yards less than six months previously. At the west end the amount of arrear was slightly greater, due to the bad character of the ground traversed. This difference, however, is being lessened, and the base-heading system is considered to have been thoroughly successful.

A diagram showing the order in which the various portions of the tunnel area are excavated is given. The ventilation was effected by a special set of air-pipes (independent of their supplying the boring machinery) varying from 1 foot $3\frac{3}{4}$ inches to 1 foot $7\frac{3}{4}$ inches in diameter in the finished portion of the tunnel, and reduced to 1 foot where the work was proceeding. The air was delivered at a pressure of 3 lbs. per square inch.

The base-heading system was adopted at the Mont Cenis, and at first worked in the same manner as the above-described (Arlberg); but afterwards the excavation from above the advance-heading was worked out in two cores, from the face instead of by shafts. This method necessitated the use of very strong timbering for the base heading (advance), but as the fissured nature of the rock would have made this requisite under any circumstances, no additional expense was entailed. Mention is made of the Laveno tunnel, where continuous base and crown headings, with occasional communicating shafts, were pierced by mechanical boring throughout the length of the tunnel, the excavation being re-

moved through the lower heading. The tunnel was 3,210 yards long, and was constructed in sixteen and a half months. This last method is recommended where the motive power sufficient for actuating the additional boring machinery is obtainable.

The crown-heading system is then considered. The adoption of this system at the St. Gothard was principally due to the contractor, who, having his choice, preferred working on a system to which he had been accustomed. It was also hoped that with a crown-heading a better ventilation for the workmen would be procurable than had been the case in the Mont Cenis tunnel.

The advance-heading is followed by the removal of the excavation on each side, and as soon as springing-level is reached, the arch is constructed, and progressively underpinned with timber, permitting the erection of the side walls one at a time. The temporary way is first laid in the crown-heading, and as the various tiers of excavation below this are removed, the difference of level between each tier is surmounted by inclined planes of timber framing. Hydraulic elevators were introduced, but had to be abandoned on account of their being affected by the gases from the tunnel. A diagram shows the order in which the various cores of excavation are removed, and another compares the length of tunnelling under construction (Chantiers), at one and the same time, in the St. Gothard and the Arlberg tunnels. The distance from the completed portion to the heading of advance (crown-heading system) is in the former instance 3,007 yards, whereas in the Arlberg tunnel it is reduced to 1,258 yards.

A third diagram shows the progress of the excavation of the headings and the various cores, and of the masonry from end to end of the St. Gothard tunnel.

From this it is seen that the progress of the arch is irregular, and is accounted for by the desire on the part of the contractor to avoid expense, sought to be attained by shifting as seldom as possible the inclined planes connecting the tiers of excavation, stretching from the face of the advance heading to the formation-level at the finished tunnel. The author, after estimating the lengths occupied by the various processes of excavation, of blasting, and the inclined planes, &c., concludes that the normal length of the tunnel under construction (Chantiers) at one and the same time, with this system, cannot be less than 2,586 yards, and that the crown-heading is not suitable for cases where mechanical boring is resorted to and expedition of importance.

The most suitable system to be adopted in passing through ground exerting great pressure is considered, whether by constructing the arch first and then the side walls, or the reverse. When the arch is constructed first, allowances should be made for its settlement, as however carefully it may be underpinned, in ground of this character subsidence and an inward movement of the springing is almost certain to ensue as soon as the whole excavation of the tunnel section has been removed. An instance is mentioned where the allowance for settlement was as much as 3 feet 3 inches in the "windy stretch" of the St. Gothard, and of the difficulties encountered and distortions of the arching which occurred in the Foggia-Naples tunnel, where the ground was of a plastic nature; the opinion being that under such circumstances the Belgian method of constructing the arch prior to the side walls should not be attempted.

The cost of excavation to the full section of a tunnel constructed by means of the crown-heading, where the boring is done entirely by hand labor, is 10 per cent. less than the same work where the base-heading is adopted; but when mechanical boring is resorted to, the saving in this item is reduced to 1.82 per cent. The cost of transport, however, is less with the base-heading system, and, as before mentioned, it is not necessary to shift the temporary track and the air-conduits, as is so frequently requisite in the crown-heading system; also the drainage is much better under control. A diagram shows the number of changes of the temporary track (including turnouts and lie-byes), air-pipes, &c., on 1 kilometer of the St. Gothard tunnel during the term of construction (thirty-nine months). The percentage of the total number of men employed in connection with these latter items is 10 per cent., and this expense is reduced to

comparatively nil where the base-heading system is adopted.

In the St. Gothard tunnel, up to the time of the junction of the north and south headings, the ventilation was very imperfect; air was supplied to the length under progress by piercing the pipes conveying air at a pressure of 90 lbs. (six atmospheres) to the boring machinery, but the motive power was insufficient to maintain a sufficient supply at that pressure, so that neither proper ventilation nor supply to the boring apparatus was attained.

The contractor for the Arlberg tunnel was paid

	£.	s.	d.	
At the rate of....	79	17	7	per lineal yd.
Interest on plant supplied by the company......	17	5	0	„ „
	£97	2	7	

The contractor of the St. Gothard Tunnel was paid

	£	s.	d.	
At the rate of....	133	4	3	per lineal yd.

The rock formation of the St. Gothard was harder than at the Arlberg.

In conclusion, the author is of opinion that for tunnels required to be executed with rapidity, the method of base-heading is preferable to crown-heading.

REPORTS OF ENGINEERING SOCIETIES.

American Society of Civil Engineers.—January 2, 1884.—The Society met at 8 p. m., Vice-President Wm. H. Paine in the chair; John Bogart, Secretary.

The following candidates were elected members:

William Hammond Hall, Sacramento, Cal.; Charles Warren Hunt, New York City; Stillman W. Robinson, Columbus, Ohio; transferred from junior to member, Frederick Brooks, Boston, Mass; elected junior member, Edward E. Magovern, Hoboken, N. J.

The death, on December 27th, 1883, was announced, of General A. A. Humphreys, U.S.A., Honorary Member of the Society. Arrangements for the annual meeting of the Society on January 16th and 17th were read.

A paper by Mr. A. V. Abbott, on "Some Improvements in Testing Machines," was read by the author, and illustrated by a stereopticon. A 200,000 pound testing machine was first described, its general construction providing for weighing the forces applied by means of platforms and levers somewhat similar to those used in ordinary scale work, with special arrangements to reduce friction. To secure the direction of the pressure upon the test pieces in the axis of the machine, both ends of the piece are conneeted with segments of spheres moving freely in spherical sockets, which take the proper position upon the first application of the stress. Arrangements are also made by means of wedges to grip and hold uniformly the ends of the test pieces. The machine is arranged to test in tension compression, for transverse stress, for shearing, bulging and torsion. In the machine illustrated, the action of applying stress is automatic, and at the same time the same power gives an autographic record of the stress applied, and of any variations which may occur during the continuance of the stress, and with an instantaneous autographic record of the result at the conclusion of the test. The stresses are applied by means of weights which slide upon two parallel lever beams, the one registering up to 10,000 pounds and the other up to 200,000. By means of a remarkably ingenious electrical attachment, connected with clockwork, the movement of these weights is continuous and automatic, and the registering apparatus is also controlled by the same electric current. It is impossible in this abstract, and without the aid of a diagram, to fairly describe the details of these movements, but they seem to be very complete and accurate. Diagrams automatically made by the machine were exhibited and described.

A number of broken pieces of steel were exhibited, and also specimens of woods which had been tested in various ways. Machines of smaller powers were also described, and a number of briquettes of cement were broken upon a small automatic machine which was exhibited.

The discussion of the paper was postponed to a subsequent meeting.

At the annual election, held on the 16th of January, the following officers were elected:

For President, Don. J. Whittemore; Vice-Presidents, Wm. H. Paine, Joseph P. Davis; Secretary and Librarian, John Bogart; Treasurer, James R. Croes; Board of Directors, Geo. S. Greene, William Metcalf, Theodore Cooper, Fred. Graff, Wm. R. Hutton.

ENGINEERING NOTES.

The trusses of the old part of the roof of the Basilica of St. Paul, at Rome, were framed in 816, and were sound and good in 1814, 1,000 years later. These trusses are of fir. The timber work of the external domes of the Church of St. Mark, at Venice, is more than 840 years old, and is still in a good state. Sound logs are dug out of bogs where they have lain for an indefinite period.

A trial has been made by the Consolidated Electric Company with an accumulator used to light up a carriage of Mr. A. de Rothschilds. A test journey from Halton to Tring Station and back was made again on Sunday evening. The light is under the command of the occupant of the carriage, who can light or extinguish either of the lamps. An incandescent light is placed in each of the ordinary outside

carriage lamps, and one arranged with a small bracket and globe inside. The current is applied from five cells, containing six metal plates in each, which can be recharged without disturbing the arrangements by simply connecting two wires. The whole, being contained in a small mahogany case, weighing a little over 1 cwt., can be easily removed from the carriage, if necessary.

EXPERIMENTS have been made at J. W. Pease & Co.'s limestone quarries, Weardale, in drilling the rock with Cranston's steam-power machines in the blue mountain limestone. The trials have been regularly and systematically carried on during the past twelve months, and a correct statement of the work accomplished has been kept the whole of the time, in which many hundred feet of holes have been put down from 4ft. to 18ft. deep each by 2in. to 3in. diameter, some of the holes having been drilled 5ft. deep in one hour with a 2in. gauge drill. One of the holes 18ft. deep, by an excellent system of blasting, is estimated to have removed 3,000 tons of rock, using $4\frac{1}{2}$ barrels of powder; some 9ft. holes, being $2\frac{1}{4}$in. diameter at the bottom of hole, have displaced over 400 tons with a little less than $\frac{1}{4}$ barrel of powder. These quarries are very extensive, being close upon 500 yards long, with a fore breast of splendid rock about 50ft. deep. The average output at the present is about 2,000 tons per week. The rock is principally used in the blast furnaces of Cleveland. A line of railway, 4ft. $8\frac{1}{2}$in. gauge, has been specially laid down from one end to the other on the quarry top, so that a portable boiler runs along it and supplies steam to the rock drills at any desired part.

KERITE TELEGRAPH CABLE.—Kerite is the invention of an American manufacturer, Mr. A. G. Day, who was impelled by the increasing cost of india-rubber to seek a substitute for it as an insulating material for electric wires. The invention is patent, and the substances of which kerite is composed are not made public, but it is known that it is composed of various bodies. It may be combined with india-rubber, sulphur, and oxides of metals in the usual manner; renders the india-rubber more durable, while reducing the cost, and is applicable to the whole class of goods into the manufacture of which caoutchouc enters. At Mr. Day's factory at Seymour, Conn., the whole process of the manufacture of cable is carried on, except that of making the wire. The ingredients of the kerite are thoroughly mixed, put into boilers, and allowed to stand until ready for use. When hot the composition is of about the consistency of thick treacle; it is run into cakes, which, when cool, are kept separated by a thin layer of soapstone, in order to facilitate handling. Pure Para gum, cleansed by a process of Mr. Day's invention, is then thoroughly mixed with the kerite by the usual grinding operation, and the whole carefully strained. It is afterwards cut into strips about an inch square, and fed into a machine, which covers it on to the wire as the latter passes through. The wire is then coiled in layers in large pans, fine soapstone dust being used to prevent adhesion, and is vulcanized. The coils are afterwards rewound upon "testing reels," and submerged in water for forty-eight hours. The wire is most carefully tested while in the bath, and the process is then complete. Wire for single lines is wound and despatched at this point,, while that for cables is wound up for the cable-making machines. Submarine cables have a coating of prepared tape over each strand, they are then surrounded with an envelope of jute fiber, and, lastly, enclosed in an armor of the best "B. B." galvanized iron wire.

TOWAGE BY ENDLESS CHAINS.—An interesting experiment in the towage of vessels by means of endless chains has been made on the Rhone by M. Dupuy de Lome. That river is troubled with rapid currents and stony shallows, hence navigation is troublesome on it, and it has not, therefore, been utilized to its utmost. The state of the bed is improving every day, however, owing to the dredging and other engineering operations now carried out; but after these are executed the Rhône will still remain too swift at several parts for ordinary sailing. Towage by means of ordinary chains is also open to several objections which do not hold in the case of towage by endless chains worked in the following manner: A tug boat is provided fore and aft on each side with an endless chain, sufficiently heavy, and plunging into the water so as to rest on the bottom for a space, the part on board being sustained by pulleys. These pulleys being turned by hand or by an engine, the chain moves with them, propelling the boat against the stream. The chain on each side is actuated by a separate motor, the craft is steered by making one chain move faster or slower than the other. The chains are disposed in such a way that for the greatest depths, the weights resting on the bottom produce an adhesion to the latter still greater than the drag of the tugs and its convoy. The experiment of M. Dupuy de Lome was made at the instance of the Minister of Marine, M. Zédé, director of naval constructions in France, and took place at the Port-de-Bouc. The tug was a vessel 33 meters long by 7.50 meters wide, and 2.10 meters deep. The two strong chains employed weighed 46 kilogrammes per running meter. Each was worked by an engine of 15 horse power, and the two machines were completely independent. but the valves and starting gear were under the control of one man. The trials showed that the barge could be properly steered and propelled in this manner in varying depths. The proper length of chain for the depth was regulated by increasing or diminishing the distance between the front and rear pulleys. The depth varied from 1 to $6\frac{1}{2}$ meters, and provision was made accordingly. The coefficient of friction of the chain on the bottom was found to vary with the nature of the bottom from 80 to 120 per cent. of the weight of the chain in air. It was thus possible to calculate what the current should be in order that the chains should not slip on the bottom. Currents of 3 meters per second were successfully encountered and overcome, and the vessel could be properly manipulated in these rapids with a safety unknown to other methods of navigation. The new plan of M. Dupuy de Lome is in fact highly interesting

and ingenious, and may be useful in mounting rapid rivers in many other countries.

IRON AND STEEL NOTES.

DESPITE the fact that the steel trade has not of late been advancing with as rapid strides as could be desired, the promotion of extension of works and the establishment of new works continue. The Glasgow Iron Company has acquired 15 acres of ground at Wishaw, near its ironworks, for the purpose of erecting a steelworks. The contracts have been placed for the machinery and buildings, part of the machinery to be supplied by an English house, part by the Vulcan Foundry, Glasgow, and the roofing of the buildings by Messrs. P. and W. M'Lellan, of Glasgow. It may be stated here that the last-named firm have secured the contract to supply the ironwork to be used in the erection of the Glasgow Municipal Buildings.

THE iron trade of Belgium is in an exceptionally depressed state for want of orders; and ironmasters have been casting about for something just to keep their works open during the winter. They observe that iron sleepers are largely used in Germany, where they are ordered in lots of 20,000 and 30,000 tons at a time, whereas in Belgium the trials made were not considered to give good results. They attribute this circumstance to the attempt to make iron permanent way for the same price as one with timber sleepers. The economy due to the use of the iron results from the greater duration and the reduction in labor for renewals, while the old iron is always worth something for working up again. M. Sadoine, of the Cockerill Works, convened a meeting of ironmasters at the Grand Hotel, Brussels, when it was agreed to petition the Minister of Public Works to institute on the State railway a fresh series of experiments with iron sleepers, and to intrust a trial order of 1,500 to 2,000 tons to each of the principal ironworks, one-third of the price to be spread over five years, at the rate of interest to be agreed upon. M. Victor Gilliaux, President of the Association de Maitres de Forges de Charleroi, undertook to hand the petition to the Minister. The ironmasters rely upon the Minister's well-known solicitude for the welfare of the working class to give this proposition a favorable consideration.

RAILWAY NOTES.

THE Railway Commission appointed to investigate the management of the passenger traffic of the Queensland railways have recommended that a board be appointed to supervise the management and construction of railways, and to see that safety, convenience, and the interests of the public are duly regarded. The commission have also recommended that appointments to the railway service should be vested in a board. An inquiry into the management of the freight traffic is strongly advised.

RAILWAY MATTERS.—The largest locomotive ever built in America is now being made in Sacramento by the Central Pacific Railroad. The engine and tender will weigh 105 tons, and will be 65ft. 5in. long.

TWO engineers of the Paris, Lyons and Mediterranean Railway have made the discovery that the rails on the line between Marseilles and Rognac have become strongly magnetized by the friction exerted by the passing trains. The magnetization is not appreciable until the fish-plates are removed and leaves the rails soon after they are taken up.

THE railway from Liege to Maestricht, says a Belgian paper, has obtained permission to put on its track, between Liege and Vise, small trains, made up of cars, of the tramway pattern. These trains stop between stations, and thus are a great convenience for the small villages along the route. They are much appreciated, and, not the least striking fact, the railway company adopted this system in order to prevent the establishment of any competing tramway line. This, says the *Railroad Gazette*, is an idea that perhaps might be advantageously adopted by some lines in our own country.

IN a paper on "Compressed Air Locomotives for Mines," published in the "Génie Civil," M. Edmond Boca claims for them the following advantages over chain haulage: (1) Greater simplicity of installation, owing to the suppression of pulleys, rollers and guides. (2) The dispensing of a double line of way and a special road for workmen. (3) Their capability of serving several galleries, of being easily transported from one working place to another, and of avoiding the risk of stoppage. (4) The facility of apportioning the power to the work, while the driver always has his engine under command.

"DURING a recent visit to Darjeeling," writes a *Times* correspondent, "I was much impressed by the great advance visible in the prosperity of the station and district since they were brought into connection with the Indian railway system two years ago. Within a few days' march of the terminus of the mountain railway lines is the richest and most populous part of Tibet, inhabited not like the trans-frontier districts of the Punjab, by Mahomedan cut-throats, but by a peaceable and trade-loving people. At present the bulk of the trade between India and Tibet is obliged to follow a most circuitous route, through Nepaul, and it is charged duty on entering and leaving that country. Notwithstanding these difficulties, the trade continues to increase. The direct route lies unquestionably through Darjeeling, and goods can now be conveyed by rail to within a week's journey from the frontier. The removal of the existing restrictions would open new and enormous markets for tea, indigo, tobacco, and other Indian products, as well as for hardware and the cotton goods of Manchester. This market would supply us in return with inexhaustible quantities of the finest wool, and with musk, borax, &c." Cheap railway extension works would, it would appear, promise a good future return.

The question of the junction of the network of the Austro-Turkish railways now occupies the attention of the Ministry. Austria raises objections to the point of junction chosen by Turkey, while the admission into the protocol of the invidious phrase that the junction should be made "par le tracé le plus convenable" places the Porte in a dilemma out of which it is difficult to escape. The shortest, cheapest, and most direct "trace" is from Vranje to Uskup, and this is therefore "le trace le plus convenable," adopted in principle in the protocol signed by the Turkish Minister, who is now instructed to oppose it.

Experiments have of late been carried out on various Prussian railways with a view of deciding the relative merits of petroleum torches and of those made from pitch or resin. The results arrived at indicate that, although the former give a clear light, and are especially convenient in some respects, they are more dependent upon the weather, and are not well adapted for casting light for a little distance around where they are placed. While of service in repairs connected with the maintenance of the permanent way, it is considered that in accidents pitch or resin torches are preferable. With a view of these latter being always available upon an emergency, orders have been issued for a supply being kept at all stations, signal cabins, &c., and every luggage van and tender will carry several of them.

There is a fiction that engines on the Metropolitan railway are not as other engines, and that they never leave the rails. Board of Trade reports tend to destroy this fiction, and to show that rigid wheel base and sharp curves are as evidently the cause of derailment on the subterranean as on other lines. Colonel Yollard has written a report on the accident which occurred on the 3rd October, between Farringdon Street and Aldersgate Street, when the four leading bogie wheels of a Metropolitan District engine mounted the rails, and ran for a distance of about thirty-eight yards off the rails before the train was stopped. No damage was done to the engine or carriages in the train or to the permanent way, and no complaints have been received from any of the passengers in the train. A precisely similar accident occurred on the 10th May last, at the same spot, to a Metropolitan District Railway Company's passenger train, and it was then recommended that the Metropolitan Railway Company should insert check-rails on all their lines on curves having a radius of ten chains or less, and not permit such curved lines to be laid tight to gauge. Since this second accident in the same place in one year, check rails have been put in.

ORDNANCE AND NAVAL.

The third ironclad, the Help in Need, corvette, built for the Chinese Government, has been launched at Stettin. She has a waterline length of 72 meters, a breadth of 10.5 meters, and a depth of 7.20 meters, while her greatest draught of water when fully equipped, will be 4.8 meters. The two bi-cylindrical compound engines, each driving a screw, have together an indicated horse power of 2,800, and ought to give the vessel a speed of fifteen knots an hour. The displacement of the finished ship will be 2,355 tons. Her armament will comprise two turret Krupp cannon of 21 centimeters, another Krupp gun of 15 centimeters, four torpedo guns, and six Hotchkiss cannon. The vessel will be manned by a crew of 180 officers and men.

At the official trial of the little twin steamships Jeanne and Louise, on the Mersey, a speed of 8 knots was obtained with a load of 18 tons on a draft of 2 feet 6 inches. The vessel, which is built of steel, is 60 feet long, 12 feet beam, and 4 feet 3 inches deep, with raised forecastle, and deck-house amidships. She is fitted with two single-crank tandem compound engines, having cylinders 5 inches and 10 inches diameter, by 8 inch stroke, fitted with a wrought iron surface condenser, and a separate engine for air and circulating pumps. The boiler is of Cochran's patent multitubular type, 4 feet diameter by 8 feet 6 inches high, and is made of mild steel throughout. The vessel and machinery were built for service on the Gaboon River, on the West Coast of Africa.

The 3,660 shipping disasters which occurred off the coasts of the United Kingdom during the year 1881-82, comprised 4,367 vessels. Unfortunately, the number of ships is larger than the total of the previous year by 70; it exceeds the casualties reported, because in cases of collisions two or more ships are necessarily involved in one casualty. Thus, 686 were collisions, and 2,974 were wrecks and casualties other than collisions; 523 of these latter disasters weie wrecks, &c., resulting in total loss; 719 were casualties resulting in serious damage, and 1,729 were minor accidents. In the previous year, 1880-81, the wrecks and casualties other than collisions on and near our coasts, numbered 2,862, or 112 less than the number reported during the year 1881-82. We observe that out of the 2,974 casualties, other than collisions, 2,623 occurred to vessels belonging to this country and its dependencies, and 351 happened to foreign ships. Of these 2,623 British vessels, 1,663 were employed in our own coasting trade, 720 in the—oversea—foreign and home trade, and 240 as fishing vessels. There were 8 casualties to ships belonging to foreign countries and States employed in the British coasting trade, and 275 to foreign vessels which, although not engaged in our coasting trade, were bound to or from British ports; while there were 68 casualties to foreign ships which were not trading to or from the United Kingdom. Excluding collisions, the localities of the wrecks are thus given: East Coast of England, 809; South Coast, 586; West Coasts of England and Scotland, and East Coast of Ireland, 1,046; North Coast of Scotland, 99; East Coast of Scotland, 161; other parts of the coast, 273; total, 2,974.

BOOK NOTICES.

Publications Received.

Circulars of information of the Bureau of Education, Nos. 2 and 3. Washington: Government Printing Office.

Report on the Waters of the Hudson River. By C. F. Chandler, Ph. D. Albany: West, Parsons & Co.

Professional Papers of the Corps of Royal Engineers, containing:

Provisional Fortification, Errors in Graduated Arcs, Lord Lake's Campaigns, 1804–6; Blasting Operations, Demolition of Barque Caroline Z., Bridges of the Kabul River, Railway Curves, Railways for Military Communication in the Field, Tables of Service Ordnance.

Professional Papers of the Signal Service:

No. VIII.—Motion of Fluids and Solids on the Earth's Surface. By Professor William Ferrel.

No. IX.—The Geographical Distribution of Rainfall in the United States. By H. H. C. Dunwoody, First Lieut. 4th Artillery.

No. XI.—Meteorological and Physical Observations on the East Coast of British America. By Orray Taft Sherman.

No. XII.—Popular Essays on the Movements of the Atmosphere. By. Professor William Ferrel.

A Text-Book on Physics. By Henry Kiddle, A. M. New York: Wm. Wood & Co.

This is an abstract of Ganót's Physics, adapted for the use of academies and high schools.

It has been arranged by a skillful instructor, and the illustrations are made to appear with all of the original excellence which made the original edition so attractive.

A Treatise on the Motion of Vortex Rings. By J. J. Thomson, M. A. London: Macmillan & Co.

This essay earned the Adams prize at Cambridge University in 1882.

The discussion involves mathematical analysis of a difficult order, and will prove profitable reading only to students who are well advanced in pure mathematics.

The literature of this line of research is not extensive, but the subject has of late excited a deep interest among physicists everywhere.

Notes on Gunpowder and Guncotton. By Lieut.-Col. W. H. Wardell, R. A.

This is prepared for the use of the gentlemen cadets of the Royal Military Academy, Woolwich.

In separate chapters the writer treats of Constitution and Action of Explosive Substances, Ingredients and Properties of Gunpowder, Fired Gunpowder, Preparation and Purification of the Ingredients of Gunpowder, Manufacture of Gunpowder, Examination and Proof of Gunpower, Guncotton, its Chemical and Physical Properties; Manufacture of Guncotton at Waltham Abbey.

A Summary of Tactics. By H. F. Morgan. London: Marcus Ward & Co.

This is a companion to "A Summary of Military Law," compiled by the same writer. It is arranged in the question and answer style, and includes the following topics:

Characteristics of the Three Arms, Ground in Relation to Tactics, Space and Time Required, Marches, Advanced Guards, Outposts, Reconnoitring, Employment of Infantry, Employment of Cavalry, Employment of Artillery, The Three Arms Combined, Attack and Defense, Rearguards, Defiles, Rivers, Woods, Villages, Convoys, Inferences drawn from Tactics in late Wars. A few very neat diagrams illustrate the text.

The Topographer: His Instruments and Methods. By Lewis M. Haupt, A. M., C. E. New York, J. M. Stoddard. Price $4.00.

A book which treats of the improved methods of topographical surveying has long been needed. Learners have been obliged to seek for such instruction in scientific journals and to search through many scattered articles to acquire a fair summary of the best modern practice.

We find this manual of Prof. Haupt. all that seems necessary for a complere treatise.

The improved instruments are described and their adjustments and use explained.

Mapping, modeling, and recording notes are fully described.

The illustrations are numerous and pretty good. The work is a valuable supplement to the common treatises on surveying.

Knight's New American Mechanical Dictionary. Section 4. Boston, Houghton, Mifflin & Co.

This is the concluding part of the new edition. It concludes articles from "Printing Press" through to "Zoogyroscopie."

The former edition was an excellent reference book, indispensable to the mechanical engineer. The present edition is an improvement inasmuch as it contains descriptions of later devices and a larger proportion of good illustrations.

MISCELLANEOUS.

A course of lectures on "Meteorology," by Mr. W Marriott, F. R. M. S., was delivered at Westminster. This lecture was devoted to the consideration of atmospheric pressure. Having referred to Torricelli's experiment, proving that a column of water 32ft., or of mercury 30in., is in equilibrium with the pressure of the atmosphere, the lecturer explained the construction of the barometer, and described the Fortin, Kew siphon, aneroid, and other forms of this instrument. As the pressure decreased with altitude, it was shown how the barometer could be used for the measurements of heights. It was pointed out that there is a diurnal range of atmospheric pressure, which consists of two minima about 4 A. M. and 4 P. M., and two maxima at about 10 A. M. and 10 P. M. This phenomenon is most remarked in the tropics. Having referred to the connection existing between the changes of atmospheric pressure and the flow of underground water, and also colliery explosions, the lecturer explained the construction of isobaric charts, and by the aid of such charts showed

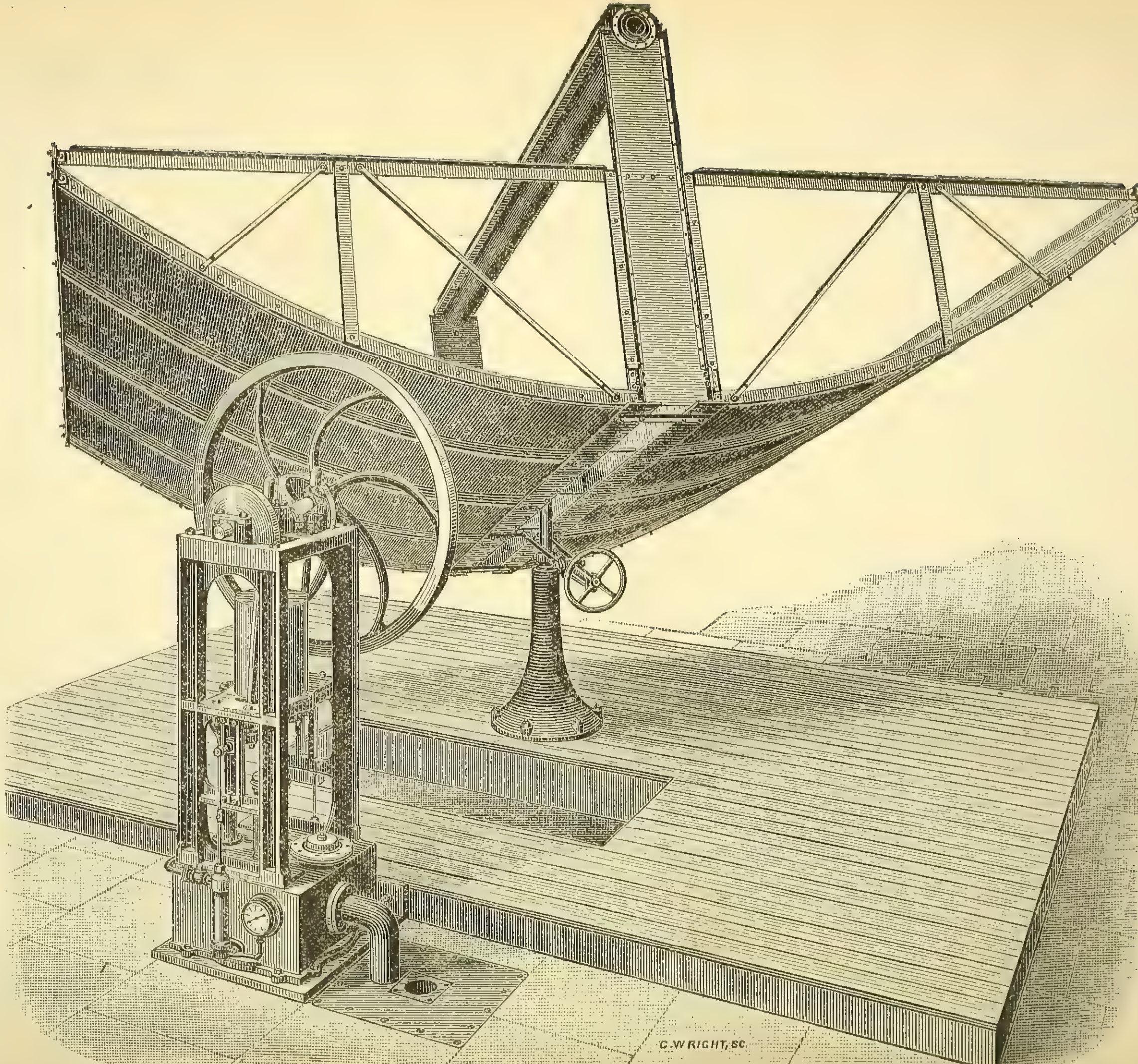
the distribution of pressure over the globe during the months of January and July.

Conversion of Light into Electricity.—The conversion of electricity into light is now a fact of every-day utility, but the reverse process has been very slow of accomplishment. It has, however, been effected by Herr Sauer. whose sunlight battery has been described in the *Electrotechnische Zeitschrift*. The chemical rays furnish the power, and the battery will only act in sunlight. It consists of a glass vessel, containing a solution of fifteen parts of table salt and seven parts of sulphate of copper, in 106 parts of water. In this is placed a porous cell containing mercury. One electrode is made of platinum, and the other of sulphide of silver, and both are connected with a galvanometer. When not in use the whole is enclosed in a box. When in use, the platinum electrode is immersed in the mercury, and the other in the salt solution; the battery is placed in the sunlight, and the galvanometer needle is then found to be deflected, the sulphide of silver being the negative pole. If the sun is clouded, or any other change in the intensity of the light occurs, it is indicated by the needle. The exact effect produced by the light rays does not as yet appear very clear, but their presence distinctly produces electrical action, and their absence suspends it.

A New Compass.—M. Mascart, the well-known electrician, has devised a new compass which finds the magnetic meridian by the well-known experiment of moving a coil of wire across the lines of magnetic force of the earth and inducing a current in them. M. Mascart employs an azimuth circle on which is mounted a ring movable round a horizontal axis. The angle made by the ring with the horizon is measured by a vertical circle. A coil of 0.21 metres in diameter is carried by the ring and can turn round an axis perpendicular to that of the ring. The size of the apparatus is not greater than an inclination compass. It acts on the principle that when the axis of rotation of the coil is in the magnetic meridian the induced currents in the ring when rotated will be nil. A sensitive galvanoscope is employed to show the induction currents. In using the apparatus, a series of trials show that the axis of the ring is perpendicular to the magnetic meridian. A second series place the axis of rotation of the coil in the line of the inclination needle. The observation, with checks, occupies half an hour which is less time than is necessary to find the inclination by a magnetic needle. From observations made at the Observatory of the Parc Saint Maur, by M. Mourceaux, the new compass seems to be as correct as the inclination compass.

At a recent meeting of the Royal Society, Edinburgh, Sir William Thomson read two papers on gyrostatics and on oscillations and waves in an adynamic gyrostatic system. The papers were in great part experimental illustrations of the theorems regarding gyrostatic stability which are laid down in Thomson and Tait's "Natural Philosophy." It was thus demonstrated to the eye that a system when under gyrostatic domination is stable in positions for which, statically considered, the system is unstable as regards an even number of degrees of freedom; so that, to take a particular case, a gyrostat which is unstable, because statistically unstable as regards one mode, is rendered stable by making it statically unstable as regards two modes. Hence also an ordinary spinning top is stable because it is statically unstable in two of its degrees of freedom. The curious behavior of a gyrostat resting horizontally on gimbals with its axis of rotation vertical was also shown, viz., its instability as soon as the framework on which it rested was moved in the opposite rotational sense to the spin of the gyrostat. The author then proceeded to point out that all phenomena of elasticity which are ordinarily treated by assuming forces of attraction or repulsion between parts or stresses through connections, can be as readily explained by the assumption of connecting links subject only to gyrostatic domination. The gyrostatic hypothesis led to other consequences which the ordinary dynamic assumption did not involve; but it had not been found as yet that elasticity had properties corresponding to these.

The Chinese Foot Rule.—A writer in the *North China Herald* gives some curious information respecting the foot measure in China. At present it varies largely in different parts of the country, and according to different trades; thus the foot of the carpenter's rule at Ningpo is less than ten, while that of the junk builders at Shanghai is nearly sixteen inches. But a medium value of twelve inches is not uncommon. The standard foot of the Imperial Board of Works at Peking is twelve and a half inches. A copper foot measure, dated A. D. 81, is still preserved, and is nine and a half inches in length. The width is one inch. The small copper coins, commonly called *cash*, were made of such a size, sometimes, as just to cover an inch on the foot rule. In the course of two centuries it was found that the foot had increased half an inch, and a difference in the dimensions of musical instruments resulted. Want of harmony was the consequence, and accordingly in A.D. 247 a new measure, exactly nine inches in length was made the standard. Among the means employed for comparing the old and new foot are mentioned the gnomon of official sun-dials, and the length of certain jade tubes used according to old regulations as standards. One of these latter was so adjusted that an inch in breadth was equal to the breadth of ten millet seeds. A hundred millet seeds or ten inches, was the foot. The Chinese foot is really based on the human hand, as is the European foot upon the foot. It strikes the Chinese as very incongruous when they hear that we measure cloth, woodwork, masonry, &c., which they regard as especially matters for the hand, by the foot. Of the jade tubes above mentioned there were twelve, and these formed the basis for the measurement of liquids and solids four thousand years ago. They are mentioned in the oldest Chinese documents with the astrolabe, the cycle of sixty years, and several of the oldest constellations. It is likely that they will be found to be an importation from Babylon, and in that case the Chinese foot is based on a Babylonian measure of a span, and should be nine inches in length.



Ericsson's Sun Motor, erected at New York, 1883.

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THE SUN MOTOR AND THE SUN'S TEMPERATURE.

By J. ERICSSON.

From "Nature."

The annexed illustration (Fig. 1) represents a perspective view of a sun motor constructed by the writer, and put in operation last summer. This mechanical device for utilizing the sun's radiant heat is the result of experiments conducted during a series of twenty years; a succession of experimental machines of similar general design, but varying in detail, having been built during that period. The leading feature of the sun motor is that of concentrating the radiant heat by means of a rectangular trough having a curved bottom lined on the inside with polished plates so arranged that they reflect the sun's rays towards a cylindrical heater placed longitudinally above the trough. This heater, it is scarcely necessary to state, contains the acting medium, steam or air, employed to transfer the solar energy to the motor; the transfer being effected by means of cylinders provided with pistons and valves resembling those of motive engines of the ordinary type. Practical engineers, as well as scientists, have demonstrated that solar energy cannot be rendered available for producing motive power in consequence of the feebleness of solar radiation. The great cost of large reflectors and the difficulty of producing accurate curvature on a large scale, besides the great amount of labor called for in preventing the polished surface from becoming tarnished, are objections which have been supposed to render direct solar energy practically useless for producing mechanical power.

The device under consideration overcomes the stated objections by very simple means, as will be seen by the following description: The bottom of the rectangular trough consists of straight wooden staves, supported by iron ribs of parabolic curvature secured to the sides of the trough. On these staves the reflecting plates, consisting of flat window glass silvered on the under side, are fastened. It will be readily understood that the method thus adopted for concentrating the radiant heat does not call for a structure of great accuracy, provided the wooden staves are secured to the iron ribs in such a position that the silvered plates attached to the same reflect the solar rays towards the heater. Fig. 2 represents a transverse section of the latter, part of the bottom of the trough, and sections of the reflecting plates; the direct and reflected solar rays being indicated by vertical and diagonal lines.

Referring to the illustration, it will be seen that the trough, 11 feet long and 16 feet broad, including a parallel opening in the bottom, 12 inches wide, is sustained by a light truss attached to each end; the heater being supported by vertical

plates secured to the truss. The heater is $6\frac{1}{4}$ inches in diameter, 11 feet long, exposing $130\times9.8=1274$ superficial inches to the action of the reflected solar rays. The reflecting plates, each 3 inches wide and 26 inches long, intercept a sunbeam of $130\times180=23{,}400$ square inches section. The trough is supported by a central pivot, round which it revolves. The change of inclination is effected by means of a horizontal axle—concealed by the trough—the entire mass being so accurately balanced that a pull of five pounds applied at the extremity enables a person to change the inclination or cause the whole to revolve. A single revolution of the motive engine develops more power than needed to turn the trough, and regulate its inclination so as to face the sun, during a day's operation.

The motor shown by the illustration is a steam engine, the working cylinder being 6 inches in diameter, with 8 inches stroke. The piston rod, passing through the bottom of the cylinder, operates a force pump of 5 inches diameter. By means of an ordinary cross-head secured to the piston rod below the steam cylinder, and by ordinary connecting rods, motion is imparted to a crank shaft and fly-wheel, applied at the top of the engine frame; the object of this arrangement being that of showing the capability of the engine to work either pumps or mills. It should be noticed that the flexible steam pipe employed to convey the steam to the engine, as well as the steam chamber attached to the upper end of the heater, have been excluded in the illustration. The average speed of the engine during the trials last summer was 120 turns per minute, the absolute pressure on the working piston being 35 lbs. per square inch. The steam was worked expansively in the ratio of 1 to 3, with a nearly perfect vacuum kept up in the condenser inclosed in the pedestal which supports the engine frame.

In view of the foregoing, experts need not be told that the sun motor can be carried out on a sufficient scale to benefit very materially the sun-burnt regions of our planet.

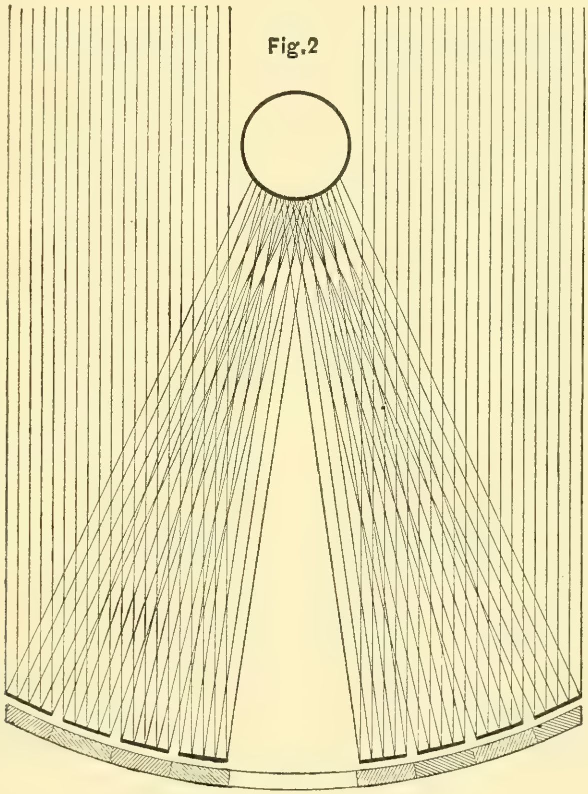
With reference to solar temperature, the power developed by the sun motor establishes relations between diffusion and energy of solar radiation which show that Newton's estimate of solar temperature must be accepted. The following demonstration, based on the foregoing particulars, will be readily comprehended.

The area of a sphere whose radius is equal to the earth's mean distance from the sun being to the area of the latter as $214.5^2:1$, while the reflector of the solar motor intercepts a sunbeam of 23,400 square inches section, it follows that the reflector will receive the heat developed by $\frac{23400}{214.5^2}=0.508$ square inch of the solar surface. Hence, as the heater of the motor contains 1274 square inches, we establish the fact that the reflected solar rays acting on the same are *diffused* in the ratio of $1274:0.508=2507:1$. Practice has now shown that, notwithstanding this extreme diffusion, the radiant energy transmitted to the reflector by the sun is capable of imparting a temperature to the heater of 520° Fahr. above that of the atmosphere. The practical demonstration thus furnished by the sun motor enables us to determine with sufficient exactness the minimum temperature of the solar surface. It also enables us to prove that the calculations made by certain French scientists indicating that solar temperature does not exceed the temperatures produced in the laboratory are wholly erroneous. Had Pouillet known that solar radiation, after suffering a *two thousand five hundred and seven fold* diffusion, retains a radiant energy of 520° Fahr., he would not have asserted that the temperature of the solar surface is 1760° C. Accepting Newton's law that "the temperature is as the density of the rays," the temperature imparted to the heater of the sun motor proves that the temperature of the solar surface cannot be less than $520°\times2507=1{,}303{,}640°$ Fahr. Let us bear in mind that, while attempts have been made to establish a much lower temperature than Newton's estimate, no demonstration whatever has yet been produced tending to *prove* that the said law is unsound. On the contrary, the most careful investigations show that the temperature produced by radiant heat emanating from incandescent spherical bodies diminishes inversely as the *diffusion* of the heat rays. Again, the writer has proved by his vacuum-actinometer, inclosed in a vessel maintained at a constant tempera-

ture during the observations, that for equal zenith distance the intensity of solar radiation at midsummer is 5°48 Fahr. less than during the winter solstice. This diminution of the sun's radiant heat in aphelion, it will be found, corresponds within 0°.40 of the temperature which Newton's law demands. It is proposed to discuss this branch of the subject more fully on a future occasion.

The operation of the sun motor, it will be well to add, furnishes another proof in support of Newton's assumption that the energy increases as the *density* of the rays. The foregoing explanation concerning the reflection of the rays (see Fig 2), shows that no augmentation of temperature takes place during their transmission from the reflector to the heater. Yet we find that an increase of the number of reflecting plates increases proportionably the power of the motor. Considering that the parallelism of the rays absolutely prevents augmentation of temperature during the transmission, it will be asked: What causes the observed increase in mechanical power? Obviously, the energy produced by the increased *density* of the rays acting on the heater. The truth of the Newtonian doctrine, that the energy increases as the density of the rays, has thus been verified by a practical test which cannot be questioned.

Fig.2



It is scarcely necessary to observe that our computation of temperature—1,303,640° Fahr.—does not show maximum solar intensity, the following points, besides atmospheric absorption, not having been considered: (1) The diminution of energy attending the passage of the heat rays through the substance of the reflecting plates; (2) the diminution consequent on the great amount of heat radiated by the blackened surface of the heater; (3) the diminution of temperature in the heater caused by convection.

COMPENSATION FOR CURVE RESISTANCE ON RAILWAYS.

By BEVERLY R. RANDOLPH.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

In the construction of railways where curves occur upon the heavier grades, the reduction of the rate of grade, in order to compensate for the increased resistance due to curvature is generally considered necessary in order to attain the most economical results in transportation.

The argument being that, if the total resistance which is necessarily opposed to the movement of trains is distributed evenly over the entire line the locomotives will be able to haul larger trains, and, consequently, freight can be carried at less cost. The amount of grade reduction which is necessary or advisable on the various curves to accomplish this result is not established with any certainty. So great is the diversity of opinion on this subject that many engineers still cling to the old practice of no compensation whatever. While this is frequently due to a prejudice against "book engineering," or results from a desire to remain in the old beaten paths, a little consideration of the subject appears to indicate that there are many cases in which it is the true policy.

In the location of every road we find what may be called "controlling points." A stream cannot be crossed at more than a certain height above the water, and the cutting in the next ridge cannot be made greater than a certain depth, and the difference between these two elevations, together with the horizontal distance between them, establishes the character of grade. If, in order to distribute the resistance evenly, the rate of grade is lessened on curves, it must be correspondingly increased on tangents. This even distribution would produce the best results if a locomotive could be made which would make steam as fast as it could use it; but every machine yet contrived, if worked anywhere near its utmost capacity, must ultimately fail for want of steam. In view of this fact it has been suggested that long grades be divided into sections, with short levels in between, on which the locomotive would have an opportunity to make a surplus of steam for use on the next grade section. This would involve an increase in the rate of grade, which the advantage gained for steam making is expected to make up. On a grade line on which no reduction is made, the irregular resistance obtained by the broken grade just mentioned is furnished in a lesser degree. The curves representing the grade sections, and the straight lines the level sections or resting places where steam can accumulate. If on a line with evenly distributed resistance the load must be lessened, on account of the steaming ability of the locomotive, to an amount which does not exceed the load which can be hauled round the maximum curve on the evenly distributed grade, then the advantage of compensation is lost. Provided, of course, that the straight portions of the line afford sufficient relief to make up for the additional steam required on the curves.

I am not aware of any experiments which will throw light on this subject, though the following computations may assist toward a conclusion:

Suppose two grade lines A and B, each two miles long, having a total of one mile each on 8° curve, or 42240° of curvature, the total elevation overcome being 132 feet. Suppose line A to be compensated on a basis of .03 per degree per one hundred feet, then

Resistance on
A=1.37 per cent. regular grade.
Resistance on
B (straight)=1.25 per cent. regular grade.
Resistance on
B (curve)=1.49 per cent. regular grade.

With a load of 400 gross tons we will have

		lbs.
Resistance A	Due to grade...........	10,960
	Due to friction, wind, etc.	1,400
		12,360
Resistance B	(straight) Due to grade..	10,000
	Due to friction, etc.....	1,400
		11,400
Resistance B	(curve) Due to grade and curve	11,920
	Due to friction, etc...........	1,400
		13,320

Ratio of driving rod leverage to wheel diameter............................ 4.10

Total piston area, 309 square inches.

		lbs. per in.
Pressure on piston.	A 30,900 lbs...............=	100
	B (straight) 28,500 lbs......=	92.2
	B (curve) 33,300 lbs........=	107.7

Let n represent the number of cubic feet of steam used in making ascent on line A, then $\frac{1}{2}n$ will represent that used on B (straight) and B (curve) respectively. If, now, we suppose engines to be worked at the proper point of cut-off, so that the steam is admitted to the cylinder at the pressure indicated above in each case, a thing which must be accomplished either with the cut-off or throttle, in order to maintain an even speed, we will have the following number of units of heat absorbed in the work:

A..................................	$17.529n$
B (straight)....$17271.8 \times \frac{1}{2}n = 8635.9n$	
B (curve)......$17697 \times \frac{1}{2}n = 8848.5n$	
	$17,484.4n$
Excess required by compensated line	$44.6n$

The number of heat units here given are from "Nystrom's Mechanics," 1880.

The uncompensated line would therefore require a lower duty from the boiler, and the same locomotive could haul a larger load than on the line where the resistance is evenly distributed.

Further investigation proves this difference to vary approximately as the square of the amount of curvature.

It may be observed that, following this line of reasoning, the greater the variations in the resistance the greater would be the capacity. Under certain circumstances this is true, as mine managers find it is much more economical to haul their output to the foot of a shaft and then hoist it, than to bring it out by a slope, though part of this is, of course, chargeable to more extensive repairs and additional friction. This fact would indicate that the method first suggested with grade and level sections would be productive of good results. On the other hand, the difference in the amount of heat required is relatively so small that it would be inappreciable in practice. Theoretically, however, it is clearly on the side of no compensation. Where the steaming ability of the locomotive controls the load, compensation for curvature would therefore be worse than useless. To what extent this is the case can only be roughly determined in practice, though discussion of it may convey some idea.

We will take, for example, the consolidation locomotive, exhibited by the Bald-Locomotive Works at the Chicago Exposition. This machine is described in the *Railroad Gazette*, June 22d, 1883, the characteristics with which we are concerned being,

Total weight......................lbs.		114,000
" weight on drivers...........lbs.		100,000
Cylinders......................inches		20×24
Diameter of driving wheel.....inches		49
Grate surface.................sq. feet		30

From which it will be seen that the tractive force is sufficient in average weather for the assumed load.

Contents of each cylinder..cubic feet 3.63

Steam per revolution......cubic feet 15.52

No. of revolutions in two miles....$\frac{10560}{12.83} = 823$

Amount of steam used...$15.52 \times 823 = 10949.96$

Cubic feet of water evaporated $\frac{10949.96}{290} = 412$

Supposing the boiler to evaporate 15 cubic feet of water per square foot of grate surface per hour, we have $30 \times 15 = 450$, or 112.5 cubic feet in 15 minutes consumed in ascending the supposed grade at eight miles per hour. There would be, therefore, a heavy draft on the amount of heat previously stored in the boiler. By reducing the speed to two miles per hour the steam could be furnished, if the boiler could still produce at the same rate, but as we would then considerably reduce the draft, we could not expect the same rate of production.

Among practical engine runners there seems to be a prevailing impression that a locomotive fails for want of steam in about two miles, or, in other words, that the surplus energy which can be accumulated on a level at the foot of a grade will supply what the boiler fails to provide for this distance. Some experiments made on the East Tennessee, Virginia and Georgia Railroad, February 15th, 1882, and reported in the *Railroad Gazette*, March 31st, 1882, will show a much lower limit.

In this trial four different locomotives of nearly the same class were tried over the same ground, with the same trains being run up the grade until stalled.

The following extracts from the tabulated report will give a fair idea of the results attained:

No. of engine.	Maker.	Type.	Cylinder.	Weight on drivers in lbs.	Total Weight.	Gross load.	Distance made.	Curvature where stalled.	Time --minutes.
92	Baldwin.	Ten-wheel.	18×24	60,000	82,750	1,003,900	3820	6°	7
82	Rogers.	"		62,100	86,600	same.	3600	6°	6
34	Baldwin.	Mogul.		66,000	78,000	same.	3394	Tang.	5
79	Rogers.	Ten-wheel.		62,100	86,600	same.	3376	Tang.	6

Both grade and curvature are reported, the former being very irregular, and ranging from level to 79.7 feet per mile on the portion included above, while the latter ranges from 3° to 10°. Unfortunately for the purpose of this paper the situation of the curves is not reported, though from some remarks made in the editorial of the same number it may be assumed that in the trials here given, the 10° curve was not reached, though it was passed in some further trials with lighter loads, it being 100 feet long and between 4,200 and 4,300 feet from the foot of the grade.

Two additional sets of trials were made, one with 903,100 lbs. gross load, with which neither locomotive reached the top of the grade, though all but No. 82 passed the 10° curve on a 98 feet grade; the other with 851,700 lbs. gross load, in which all four locomotives reached the top, 5,700 feet.

Here it is very evident that some factor, other than the tractive power, controlled the load, since the locomotive with least weight on drivers, passed the points where the heavier ones stalled, and three locomotives passed the point of greatest resistance, but were unable to reach the top. Since the object of reducing the grade on curves is to increase the tractive capacity of locomotives, it would here, in a grade 5,700 feet long, have had no effect in increasing the load which could be carried over the grade, and the arrangement which would require least steam would undoubtedly be best.

Leaving this subject we will now pass to the matter of compensation without regard to its advisability.

The numerous discussions of this subject which have appeared in the various technical journals and books in the last few years, seem to have done comparatively little to clear up the difficulty, and the inquiring engineer finds a different opinion in each authority which he examines. From one he gets a constant which is to be multiplied by the degree of the curve, another tells him this constant should increase with the degree of the curve, while a third gives him a long formula, and when he comes to make the substitutions for the unknown quantities he finds as great a variety of values in the practical data as he has previously found opinions in authors.

A little consideration of the various factors which must enter into any formula, whether theoretical or empiric, will, I think, disclose good reasons for this disagreement.

The common truck or bogie with which we have to deal may be described as a four-wheeled wagon, all wheels rigid on the axles, the propelling power applied at the central point of the wagon, the guiding being done by the rails. The force required to drag this wagon through a curve will vary,

First. With the length of the truck, since the wheels are rigidly attached to the axles, and consequently to one another the effort to preserve the straight line in their motion causes the flange of the forward outside wheel and the after inside wheel to impinge on the rail. For experiment on this subject see *Van Nostrand's Magazine*, Vol. XXIV., page 390.

This flange friction is a fruitful source of resistance, and is decreased by the draught, acting with the flange contact of the after inside wheel as a fulcrum to draw the forward flange away from the rail. Its effect in so doing will depend on the angle which the line of draught makes with a line drawn from the pin to the point of contact of the flange of the after inside wheel, and on the distance from this same point of contact to the front wheels, the former controlling the lever arm of the power, and the latter of the resistance or weight to be moved, both being dependent on the length of the truck, though not bearing the same ratio to it.

Second. With the tightness or looseness of the gauge. The additional width or "play" allowed in the gauge on some roads, by permitting the truck to assume a greater or less angle with line of draught, varies the resistance. For practical demonstration see *Van Nostrand's Magazine*, Vol. XXIV., page 517.

Third. With the length of the train. The draft acting on each truck of a train is the resultant of the traction exerted through the truck ahead of it and the resistance from the truck behind. Since there is approximately a regular increment added at each truck, the ratio of these two forces will continually decrease toward the head of the train, and the resultant will therefore continually approach a direction radial to the curve, thus continually varying the angle of draught and the resistance, as before explained. Up to a certain point this will diminish the resistance by drawing the forward wheels from the outer rail and lessening the flange friction. After this point is reached the forward wheels will come in inner contact, and the flange friction will increase from this point forward.

Fourth. With the velocity of the train. Owing to the unequal length of the two rails on a curve, and the fact that the wheels are rigidly fastened, one wheel must slip, and we have resistance in the force required to produce this slipping. With sufficient velocity the centrifugal force throws so much of the weight on the outside wheel that the inner wheel slips more or less readily, in addition to which the flange friction is also effected by the extent of the centrifugal force.

Fifth. With the super elevation of the outer rail. By means of this super-elevation the force of gravity is brought in to counteract the centrifugal force, and also to effect the direction of the draught through its horizontal component, which would cause the truck to slide down the inclined plane toward the center of the curve. This super-elevation varies constantly with the opinions of the parties who have charge of the maintenance, and with the attention it receives from the section men.

Here we have five factors all effecting the result of any experiments made to determine curve resistance, and all varying for different roads and parts of roads, through frequently very wide limits. Hence it would seem that anything in the shape of an exact universal formula is scarcely to be expected in the present state of railroad construction. But railroads must be built, and the practical engineer must find some way out of the difficulty, however rough it may be.

In the matter of the length of truck we may hope for some uniformity, as the influence of the Master Car Buiders' Association grows and their standards are adopted, provided they do not change their standard as the years pass by, and leave us with a mixture of old and new standard trucks.

For uniformity of gauge we have not so hopeful an outlook. Opinions will probably differ on this subject so long as railroads are used, and even should the necessity for widening be universally admitted the amount will still be a question of dispute, and as the man who builds is not often the man who maintains, the construction will be without any definite guide as to what is best for him to do.

In determining the length of trains we have two important factors to consider, one, the constantly increasing size and tractive power of the locomotives tending to long trains, and the other, in the constantly increasing load per car without a corresponding increase in the length of the car, which results in shorter trains. Here, again, we are at sea, and the best thing to be done in view of future contingencies must depend a good deal on bold guessing.

In the matter of velocity, the coast is more clear, since, if the other elements are fixed, an intelligent operator would not be long in finding the best speed at which to run his trains, which, supposing the entire calculation to have been correct, would be the speed originally intended. There is, however, an element of uncertainty in the future for this matter. As before explained, the principal cause of resistance which is effected by the velocity is the slipping of one wheel, owing to the rigidity of the wheels on the axle. Experiments show that after a speed of twenty-five miles per hour has been attained the effect of this slipping disappears. Unfortunately I have not the data which would enable me to state whether they were made with long or short trains, or both.

If with short trains they are very readily explained, on the supposition that the centrifugal force takes all weight off the inner wheel at that speed, thereby allowing it to slip without resistance. With long trains, on the other hand, we would expect the flange friction from the inner contact in the forward part of the train, produced by the draft as before explained, to cause the inner wheel to slip with difficulty. However this may be, the speed at which the majority of cars pass over a railroad, especially up heavy grades, is near eight miles per hour, and we must expect a considerable increase of power to be required in order to produce the necessary slipping of the wheel. This difficulty a good "loose wheel" would of course, obviate, and would also do away with a great deal, if not all of the flange friction from the outer contact of the leading wheels. Just why something of the kind has not come into use, I have never been able to find any one who could explain. The objection seems to be largely a practical one, simplicity of construction and cheapness of maintenance being the main points. There have been, however, several patents secured for wheels which seem exceedingly simple, and though the repairs would, of course, exceed those of the common wheels and axles, yet, on the other hand, the advantage gained for freight service especially would be considerable. If a good contrivance of this nature should be invented, or one of the present ones come into use, roads on which the grades were reduced on curves to satisfy the present condition would reap no benefit from the new state of affairs, since the grades on which their straight portions are built establishes their maximum resistance. It may be argued that as nothing of the kind has yet appeared, nothing is likely to, but the same argument has been used against the steam-boat, railroad, telegraph, and innumerable other contrivances which are every day demonstrating their practicability. The thing may even now be in existence, and only awaiting the energy of some railroad officer sufficiently sanguine and strong to give it a fair trial. The present furor for cantilever bridges gives an idea of how a thing of the kind is taken up when once made a success.

When we come to compare the duty of locomotives drawing fixed and loose wheels, with reference to their steaming capacity, the advantage of the loose wheel seems to disappear, although very apparent in detail experiment. Tests with the loose wheels of the Miltimore Car Axle Company, made on the Central Vermont Railroad, in 1874, by Mr. Henry Waterman, show a saving of 23 to 48 per cent. in the power required to pass a 3° curve on a grade of 32 feet per mile, and a 4° curve on a grade of 41 feet per mile, the percentage of resistance being calculated on the total resistance while passing the curve, after deducting the influence of grade, without deducting that which is due to journal friction, wind, etc., and is found on straight as well as curved line.

Making allowance for the fact that the experiments were made for the Car Axle Company, and that the experimenter naturally desired to make as good a showing as possible, we will call the saving 30 per cent.

Applying this to line "B," mentioned in the earlier part of this paper, we have,

B (straight)	Due to grade.........	10,000
	Due to friction, wind, etc	1,400
		11,400
B (curve)	Due to grade and curve..	11,344
	Due to friction, wind, etc.	1,400
		12,744 lbs.
Pressure on pistons	(straight).......	28,500
	(curve).........	31,860
Area of pistons, 309.		
Pressure per sq. in.	(straight).......	92.2
	(curve).........	103.1
Units of heat per cubic feet of steam (straight)......................		17271.8
Units of heat per cu. ft. of steam (curve)		17627.5
		2)334899.3
Average........................		17449.7
Average with rigid wheels, as previously calculated................		17484.8
Saving per cubic feet of steam.		35.1

This is about .002 of the total number of heat units required, and would therefore allow an increase of .8 of a ton to the assumed load of four hundred tons. This is, of course, a mere nothing, and will account for the lukewarmness of the railroads in adopting the Miltimore Axle, since it has been quite thoroughly tested, and, so far as I know, is still in use without having been generally adopted.

In the matter of super-elevation, the

constructing engineer is, of course, completely in the dark as to what will be done by the operating department, so his estimate of this factor is a mere guess.

From the foregoing, it would seem that the chances of obtaining an exact expression of the additional resistance caused by curvature is exceedingly remote, if not altogether in infinity. Even if a thoroughly correct formula could be constructed, the uncertainty of the data to be obtained would render its practical application more or less misleading, and the end would probably be as well obtained by a rough general average of all opinions as in any other way.

After reviewing the whole field the question still remains—What is best to be done by the working engineer who must build at once? The case seems scarcely to be made out either for or against compensation for curvature. The arguments in favor of compensation appear to have confined themselves to the supposition that a locomotive can exert its full tractive force throughout the gradient. and have thus constructed general results out of detail experiments. The other side has also proceeded in much the same way, having accepted laboratory experiments as to the number of units of heat required to produce certain effects. Both need practical verification, which will no doubt be furnished in the future from some of the numerous roads recently built with compensated curves. So far, the two sides seem about evenly balanced, though there are still arguments on the side of no compensation.

The constructing engineer should bear in mind that he is not building for the present alone, but for all time. He should therefore consider the probable future of his work in connection with the actual present. If we take a common four-wheel road wagon on a smooth, hard road, we find that the friction on the shortest turns is not appreciably greater than on a straight line. With a railroad truck we have the smooth, hard road, but owing to the rigid wheels and the method of guiding, we have a very much increased friction. It would therefore seem but natural to expect that the future will bring a contrivance which shall pass a curve with as little frictional resistance as road wagon does now. There is certainly no little incongruity in building a practically everlasting structure, like the permanent way of a railroad to satisfy the evanescent conditions of its rolling stock, which lasts at most, but a few years, and is almost always replaced by a different type. Let the rolling stock conform to the permanent way.

A more present and tangible advantage of no compensation is the fact that for descending trains the acceleration is more regular, and the trains can therefore be handled with more safety and less wear of rolling stock.

All things considered, it would seem that an exceedingly conservative policy is the safest, and that if compensation is attempted at all, it should be to as small an extent as is consistent with any results of reliable experiments. If, however, it should be necessary to locate a sharp curve very near the top of a grade it would be good policy to reduce the grade, if it could be done without much increased expense, since the boiler pressure would here be near the lowest limit at which it could move the train, and a very slight additional resistance might determine whether a train could pass over the summit or not.

Another point of interest in this connection, though scarcely germain to the subject of the paper, is, that since the steam pressure is high at the foot of the grade and low at the top, considerable economy can be effected by using the steam expansively from a high boiler pressure at the foot of the grade, hence sharp curves and consequent high resistance is not so objectionable on the lower as on the upper portions of the grade.

ASBESTOS ENAMEL.—Powdered asbestos is used by M. Erichsen, of Copenhagen, for making an enamel or coating to be applied to pipes, walls, and so on. The powder is mixed with soluble salts, such as silicate of potash, and mineral or other colors which combine with silicic acid, so as to form a product which resists the action of oxygen, heat, cold, or damp. The coating furnishes a refractory glaze, which protects the material it is applied to, whether wood, gas or water-pipes, and stone or brick buildings. When applied to masonry or wood the surface of these is first washed with soap and water. In preparing the enamel the refuse asbestos only need be employed. It is also proposed to apply the coating to boilers in order to protect the plates against a too intense fire.

ON THE COMPARATIVE ENDURANCE OF IRON AND MILD STEEL WHEN EXPOSED TO CORROSIVE INFLUENCES.

By DAVID PHILLIPS, M. Inst. C. E.

From Proceedings of the Institution of Civil Engineers.

II.

DISCUSSION.

Dr. C. W. Siemens said, it perhaps would have been better if the discussion had been commenced by persons more interested in the use of iron and steel, than by those who, like himself, were intimately connected with their production; but in another respect it might possibly save the time of the Institution, if he took that early opportunity of referring to some conclusions in the paper with which he could not agree. The author had given the results of elaborate experiments on a subject which was of the utmost importance to engineers; and if his conclusions were to be relied upon, engineers were daily committing a grave error in using a material which gave so slight a guarantee of endurance. But while he accepted every one of the experimental facts adduced by the author, he thought he was in a position to prove, from the author's figures alone, that his conclusions were entirely erroneous. He had referred, in the first place, to a long series of experiments made by the Admiralty, under the direction of Admiral Aynsley, and as far as the collection of facts was concerned, nothing could be more conscientious or thorough than that series of experiments; but as regards proof, they went no further than to show what every experimenter ought to avoid, and how he ought not to conduct his experiments in the future. The author had placed before the Institution the apparatus then used. It consisted of thirty-eight tubes of iron and steel riveted in metallic contact with the shell of a boiler, and exposed partly to air and partly to hot water and solutions. Although the author had a very poor opinion of electricity and its effects, Dr. Siemens had a strong belief in electricity wherever it had a chance of acting for good or for evil, and he was convinced that the results obtained in those experiments were rendered entirely unreliable through galvanic agency. The results were most variable. Whereas one iron (common iron seemed to be the best) gave a corrosion of only 7 grains, on an average per square foot, Bessemer steel gave 21 grains, or three times the amount during the time of exposure. The author was really most merciful when he stated that the result was only 69.3 per cent. in favor of iron, because it was really 300 per cent. in that instance. Notwithstanding these experiments, the Admiralty had adopted a mode of action which seemed strangely at variance with the conclusions to which the experiments would point. They now used steel almost to the exclusion of iron, and he hoped that some one connected with the Admiralty would state the result of more recent experiments undertaken with a better knowledge of the conditions under which they should be made. He believed the conclusions since arrived at were very different from those deduced by Admiral Aynsley some years ago. At Table VII. the author had compared Landore metal and iron, the one giving an average loss per square foot of 506.24 grains, and the other of 483.17 grains, showing a difference of only 4.8 per cent. against the steel, and that was, at any rate, a great deal better than 69 per cent. On the same page the author stated: "The only peculiarity worth noticing in this experiment is that, while the two plates in the feed-water heater lost 381.8 and 394.2 grains, the two in the boiler fed from the heater lost only 8.0 and 3.4 grains respectively." Therefore in the boiler the iron lost by corrosion about one forty-eighth, and the steel about one hundred and twentieth of what they respectively lost in the feed-water heater, the loss of iron in this case being about two and a half times that of the steel. In Table IX., set 98, the Y steel produced by the Siemens process gave a corrosion of 1,220 grains, and the DD. Yorkshire iron 1,221 grains, in each case per square foot of surface; the corrosion in those

instances being practically the same. In the next set, 99, the Y steel gave a corrosion of 259 grains; Staffordshire iron 269 grains, and DD. Yorkshire iron 260 grains, showing that the steel came out best in that series. In set 102, the Y steel gave 109.5 grains (in rain water), and the DD. Yorkshire iron 144 grains. And yet the author followed up these facts with the conclusion that steel corroded on an average 64.8 per cent. more rapidly than iron. He entirely objected to the mode of reasoning adopted; he contended that averages were only applicable to errors of observation. If an observer was not certain of his weighings, and he made a hundred weighings of the same piece of iron, he would be pefectly justified in taking the average. But nothing could be more unscientific or erroneous than averaging several materials, one group of which he chose to call iron, and another which he called steel. It was as if a moral philosopher wanted to find out whether fair complexioned people were more virtuous than dark complexioned people, and were to take six fair people and six dark people promiscuously; then finding that they were all very well behaved, except one of the fair people, who happened to have just escaped from gaol, and had committed six murders, he were to draw his average and say: "I find that fair people have committed, on an average, one murder each, and should therefore not be trusted." That was the kind of argument which the author appeared to have adopted. There were substances, compounds of iron, manganese and silicon sold for steel, which no doubt corroded very rapidly; but it was for the consumer not only to select the proper material, but also to see that it was properly used. He believed it was in regard to the proper selection and use of the materials that the enormous discrepancies with which they had to deal would be found. The author stated that there was more cinder in iron than in steel, and therefore that there was *prima facie* ground for supposing that iron would corrode more than steel. There was, however, an essential difference between cinder in iron and the scale of steel. The cinder in iron was a glassy substance, which was a dielectric, and therefore had no effect upon the corrosion of the metal, whereas the scale on steel, which was produced in rolling, had a very deteriorating influence; it was a magnetic oxide, which was negative to the steel, and wherever the metal was exposed in the presence of such magnetic oxide, corrosion took place rapidly. Again, if the scale should be rolled into steel plates, as was sometimes the case, rapid corrosion ensued, for the same reason. But he need hardly say that with proper care those causes of undue corrosion could be and were prevented, and the extensive use to which steel was now put proved sufficiently that there was, at any rate in ordinary practice, no such destructive effect going on. The author stated that those interested in steel had been singulary negligent in not following up the question of corrosion. Being himself much interested in steel, Dr. Siemens had for some years caused a running set of experiments to be carried out by Mr. Willis, the chemist at Landore, which told a very different story from the author's. In one series, extending over six months, made partly in a boiler supplied with salt water, and partly by exposure in a tidal river—the plates being exposed to the air for six hours, and then immersed for six hours in salt water—the result was in some instances of open exposure slightly in favor of iron, but, in the cases of boilers, always very much in favor of steel. He had just received a report from Mr. Willis, in which reference was made to a point of importance, the perfect cleaning of the surfaces. It had been found that if the surfaces were carefully cleaned of oxide by dipping the plates in the first instance in an acid solution, the corrosion was always much diminished. The evil effects of scale on steel were pointed out at the Institution of Naval Architects, by Mr. Barnaby, on the 5th of April, 1879, when he clearly showed that the magnetic oxide scale was very deleterious in its effects. He believed that it was now the practice of the Admiralty to clean the scale off before using the plates for shipbuilding. During the past week he had received a number of letters, quite unsolicited, from gentlemen interested in the use of steel, all speaking in the most definite manner in favor of steel as a metal not liable to corrode under ordinary circumstances. One of them was from the Clyde Bank

Foundry, in which it was stated by Mr. Thompson that forty steel boilers had been at work for more than two years, and that their examinations had led to the conclusion that no active corrosion was going on. He believed it would be found, from general experience, that steel under proper conditions lasted at least as well as iron. He hoped that the discussion would bring out such further facts as would put the question practically at rest. That there was, under certain conditions, a very active corrosion going on both upon steel and iron was clearly proved by the paper, and by other experiments; but the conclusions drawn by the author in favor of iron were, he thought, unjustified by the results of his own experiments as well as of others.

Mr. B. Martell thought engineers were much indebted to the author for bringing his valuable experiments before them. When it was remembered that at the present time 82,000 tons of steel ships were being built, the great importance of the subject would be at once recognized. The question of the mechanical properties of mild steel was no doubt well understood. It was known that it could be produced possessing all the qualities required for ship purposes (he referred especially to the hulls of ships), with all the ductility and strength, as compared with iron, that could be hoped for in a material of that kind. The question of corrosion, however, was one that required to be solved, and for that purpose more information was needed than had been hitherto obtained. Many shipowners were anxious to build ships of steel, but an opinion was abroad that it deteriorated more quickly than iron; hence the importance of having reliable facts upon which to form a correct opinion on that branch of the subject. About eighteen months ago a steel ship in the North of England, not a year old, was hauled up on a slip-way; he went there for the purpose of examining her, and his examination appeared to bear out in a striking manner one of the results mentioned by the author, who had shown the rapid deterioration that took place where steel was exposed alternately to salt water and to air. The vessel was riveted with iron rivets, and he found that between the light water mark and the load water mark, which was alternately wet with sea-water and then dry and exposed to the air, a rapid deterioration had taken place as compared with the other parts of the vessel, and with iron vessels; in fact, the steel round the rivets had wasted to a considerable extent, so that the rivet points were protruding some distance beyond the steel. He thought it might probably be due to galvanic action. Dr. Siemens had referred to magnetic oxide, but that could not have been the explanation, because by hammering the rivets the whole of that would have been certainly beaten off. He attributed it more to the galvanic action taking place to some extent between the iron rivets and the steel plates. The result seemed to show that under such conditions as those to which he had referred a more rapid deterioration ensued than under other circumstances. Two months ago, however, that same vessel, which had been continuously running since, was hauled up again and thoroughly examined, and owing to the greater care taken in protecting that part, no deterioration of any moment had taken place more than in any other part of the vessel. A striking and important fact was at the same time brought out. The vessel was constructed with a double bottom, or water ballast tank, extending fore and aft, and inside that no deterioration had taken place beyond what would have occurred in iron, and possibly less. That seemed to show that where steel was entirely immersed in water no more rapid deterioration followed than in iron. He was of opinion that the results stated by the author might be correct, but they would not prevent his adopting steel for ships in preference to iron. Mild steel was superior to iron for every purpose for which it was used mechanically, and by continually coating the surfaces it could be protected as much as iron. With regard to chemical action, he had known an iron vessel carrying sugar, in which some of the bottom plates had been eaten through in a few months, and nothing could be worse than that with the use of steel. But there, again, there existed an excellent preservative in Portland cement, which would protect the bottom from any action of that kind. The inside was open to observation, and it could always be coated. By the use of steel the ship was stronger, and most

of the parts could be protected from deterioration, with the exception of that between the light and the load line, and that only required a little closer attention in keeping it coated so as to prevent rapid corrosion. He hoped the result of the paper and of the discussion would not be to scare shipowners from the use of steel. He trusted it would not be considered, as the result of experience, that steel had deteriorated 120 per cent. more than iron, as would appear to be the case from some of these experiments; but that the facts derived from the actual wear and tear of ships would be considered before any decisive conclusion was arrived at.

Mr. N. Barnaby, C. B., drew attention to the statement in the paper: "In spite of these reasonings, it is undoubtedly a fact, that under almost all circumstances iron, and particularly the harder classes, is far superior to the finer steels in its resistance to corrosion, and this the experiments described by the author incontestably prove." When he saw that paragraph he remarked to the engineer-in-chief of the Admiralty, Mr. Wright, who was a member of the first Boiler Committee (of which the author was also a member), and had continued the experiments to the present time, "it is necessary that there should be some explanation with regard to this, because we are using steel shells for boilers very largely, and people will expect that some one from the Admiralty should say whether this statement, derived from Admiralty experiments, has been borne out by later work." Mr. Wright replied: "We have continued the experiments from the time when Mr. Phillips left the Boiler Committee, and we have come to the conclusion that there is no difference in the rate of corrosion between iron and the mild steels we are using. By such experiments as those which he conducted, and which have been continued since, so far as we can make out, the results are pretty nearly the same." He stated, moreover, that steels could be as well protected as iron by zinc suspended in boilers. That was the justification for the present practice of the Admiralty in using mild steel for the shells of boilers. That practice was a very recent one, the Admiralty having commenced making steel boilers only within the last few years; and it was hardly right that he should speak of it when there were so many others who had had a long experience of the use of steel in boilers. His only justification in alluding to the subject was that engineers might wonder how it happened that the Admiralty report, as the author had presented it, should not be in accordance with the present Admiralty practice. With regard to what Dr. Siemens had said as to the effect of the hard black oxide upon the surface of steel, it was true they discovered a long time ago that the effect of that oxide was very strong indeed, almost like that of copper, yet they were foolish enough (he could use no other word) in building two ships at Pembroke, to allow them to be coated with anti-fouling composition before the black oxide was completely removed. His excuse was that the portions of black oxide remaining were very small, and that the officers who were charged with the duty of getting the bottoms quite clean thought they had done so. It was not until the Iris had been at sea for some months that it was discovered that rust was forming under the coat of paint with which the bottom had been covered. The Admiralty had learned wisdom by that occurrence, and he believed it would not happen any more. They now took pains to clear the oxide off completely. He thought the reason why they had found it out before others had discovered it was, that it was the practice in private trade to build vessels of that kind in the open air, and it was there easier for the black oxide to get removed from the surface of the steel. The Admiralty built their ships under cover, where it did not come off so easily.

Mr. J. Farquharson said he had a memorandum showing the importance of removing the oxide. The Admiralty were aware of the view set forth in the paper, that mild steel was much more liable to corrosion in salt water than iron, long before the Report of the Boiler Committee was printed, and they determined to test that point specifically. In looking at the diagrams he was of opinion that no reliable inference could be drawn from such a combination; it was so complicated and was affected by so many conditions. By Mr. Barnaby's directions it was arranged that they should try—not

surfaces partly covered with oxide and partly uncovered, in an apparatus the condition of which they knew nothing about, and which was itself a different metal—but plates of iron and steel of a considerable size carefully prepared, the oxide being entirely removed by two processes, first pickling it off by chemical means, and secondly, planing it off to see how far the process of removing it affected the result. The plates had been tried under conditions which had led to the inference that iron corroded a great deal more rapidly than steel; but the result of the experiment when the plates had been divested of oxide on the surface, was that they were practically alike; if there was any difference it pointed in favor of iron. It was a rather extended series of experiments carried on at Portsmouth with great care. It was then determined to ascertain something about the electric effect of the oxide, and another series of plates was prepared, each 2 feet long, 1 foot wide, and $\frac{3}{16}$ inch thick of mild steel. Some of the plates were prepared in the way he had described—the oxide being removed by two processes. The plate from which the oxide had been pickled lost 4 oz. and 70 grains; the one from which it had been removed by planing lost 3 oz. and 390 grains. That was the case with two pieces of the same plate. In another experiment, one plate with the oxide removed as he had described was combined with another plate of the same size, 2 feet long and 1 foot wide, which had been cut from the same piece of Landore steel; they were placed parallel to each other, 3 inches apart, in a wooden frame, and were connected by wire, one with the oxide on, the parts where the oxide was not on being touched over with a protective varnish to prevent local action. The plate with the oxide on lost nothing; the plate in contact with it, combined electrically, lost 10 oz. 95 grains. He had before him the record of another series treated in the same way, but he would only refer to a few instances to show that there was a great amount of harmony in the results. A plate with the oxide pickled in contact with another with the oxide on, held parallel to each other 3 inches apart, immersed in the same way in Portsmouth Harbor, lost 12 oz. and 34 grains; that with the oxide on lost 75 grains—that happening from minute parts unobserved and unprotected by the varnish pitting. With another pair of plates, one with the oxide removed, combined with a copper plate of the same size, and placed in precisely the same relation to it as had been the plate with the oxide on, lost 11 oz. 345 grains—less than one of the other plates where steel alone was used, showing that a tolerably compact coating of oxide was as detrimental to steel exposed with the oxide on as to copper. There were a number of other experiments, the general result of which was the same. He was considerably surprised when he read the paper. He knew that the experiments of the Boiler Committee had been continued, and he could only suppose that the author was not aware of what had happened. Mr. Barnaby had rather understated the case in favor of steel. In most of the cases that had been tested by the late Boiler Committee the results were strongly in favor of steel. Having a full knowledge of all the experiments conducted for the Admiralty for some years, his opinion was greatly at variance with that of the author as to the general result of the comparison between iron and steel. No one acquainted with the electrical effects of the combination of metals would expect to find any reliable inference from such a combination as had been presented. Copper, gun-metal, zinc, iron, steel, had all been combined electrically, and at various distances, some in contact, some supported on metal rods, some on iron, some on steel; and it was stated that the results were so much per cent. in favor of iron. For himself he would not undertake to say what the particular result was in any of those tests with any such combination.

Mr. John Donaldson could corroborate the remarks of Dr. Siemens and Mr. Barnaby, as to the evil influence of the presence of black oxide on steel plates, by mentioning two cases which had come under his notice. The practice of his firm, at the time when the boats were built, was to preserve, as far as possible, the oxide on the plates, with a view to prevent them from getting rusted, the oxide itself protecting the plate covered by it from rust. The first case was that that of a boat built for service on the west coast of Ireland. For some time

after leaving the yard they had most favorable accounts of her performance, but suddenly they received a letter, stating that she had one day gone out fishing, and it was as much as two men could do to keep her from sinking. The boat was examined, and it was found that the plates in the bow and in the stern, and others in the boiler and engine-room, were pitted with very small holes. It appeared as if some of the black oxide had been knocked off in the process of working the plates, and that the oxide left on had contributed to increase the rusting of those parts. At first he thought it was due to the engineer not clearing away the ashes from the stoke-hole, and the soot from under the smoke-box; but an examination of the second case, that of a boat built for the Zoological Station at Naples, seemed to show that it was more the action of the black oxide, helped largely, perhaps, by the plates not being kept well painted. In that case the principal pitting took place at the bow and stern, and scarcely any in the neighborhood of the engine-room. He therefore concluded that it was the black oxide that had been acting galvanically in oxidizing those parts of the plates that were exposed. When the plates of a boat were well painted and kept clean, very little of that action took place, as was shown in the case of the Lightning, which his firm built some years ago, and the bottom of which they had lately scraped and painted, the plates being found in excellent condition, due no doubt to the great care taken of the vessel while in the hands of the Admiralty. Their practice now was not only to remove the scale from the plates, but to galvanize the whole of the hulls, and since that had been done there had been no trouble as far as oxidation was concerned.

Sir Henry Bessemer was sure no one could help feeling that the question brought forward in the paper seriously affected a great deal that had been done within the last twenty years, and was well worthy of investigation. As a preliminary to his remarks he desired to say that he accepted unreservedly all the figures which had been given by the author; he accepted his experiments as stated, and had no doubt of their entire fairness. But if he were asked from those experiments to draw the inference that mild steel in the form of a boiler such as was ordinarily used would not endure half the amount of use and resist corrosion as well as common Staffordshire plate, and that phosphorus being present in the commoner irons, was the cause of its standing so well as compared with mild steel, he must entirely demur to any such conclusion. In the first place, the experiments on plates suspended inside a steam boiler appeared to him to be under totally different conditions from those to which a steam boiler was exposed when in use. A steam boiler in use had its internal flue exposed to a very high temperature on the inside, and on the outside of the water only, subject more or less to a deposit on its surface, and to a great many strains continuously by the raising and lowering of the temperature, which a suspended and insulated plate in the water did not feel or come in contact with. The external shell of the boiler also was exposed on the inside to the corrosive action of the water, and on the exterior to the corrosive action more or less of the gases of the furnace passing along the flues, and also to the escape of water occasionally from the weeping at the rivets, and so on, sometimes cutting large notches, or, if the boiler was well riveted, not affecting it at all. To these violent changes of temperature the suspended plate was not subjected. The deposition of scale upon the surface would not take place on the suspended plate in the same manner as in the case of a boiler. The conditions, therefore, were in reality very different, and that difference would sufficiently account for the fact that boilers in actual use did not corrode at the rapid rates that the suspended plates appeared to have done, from the evidence given in the paper. Some twenty-three years ago he manufactured a great many boiler-plates for a gentleman who was about to make experiments upon their use, and who desired to know what was the actual working condition of those new steel plates. In one instance 50 tons of plates were made into boilers, and the result, after twenty-two years' use, was very different from that which had been described with regard to the suspended plates, in consequence of the different conditions to which they were subjected. As intro-

ductory to the experiments, he might be permitted to refer to the remarks made by Mr. Daniel Adamson, M. Inst. C. E. (than whom there was no more thorough and practical man connected with boiler work), and by Mr. William Richardson. The occasion was the discussion on a paper which he had read on the 31st July, 1861, in Sheffield, before the Institution of Mechanical Engineers, Sir William Armstrong occupying the chair. Some specimens of flanging for locomotive boilers were then exhibited, and after they had been examined by the chairman, the remarks he had referred to were made in reply to an inquiry whether the plates from the new steel were much used. The plates of some of the boilers were exceedingly thin, and if anything like rapid corrosion took place in those plates, they would soon have become too weak to sustain the pressure of 85 lbs. to which they were subjected, and would have given way. It had occurred to him that it would be well to ascertain the condition of those boilers at the present moment, inasmuch as, if corrosion had gone on at the rate stated in the paper, in connection with some of the Bessemer steel, the plates would have been entirely destroyed in about eight and a half years, and as the boilers were made twenty-two years ago, it might be supposed that a second or third set would now have taken the place of the original ones. He accordingly sent the following telegram to Mr. Richardson: "A paper on corrosion of steel boilers will be discussed to-morrow at Civil Engineers; are the six boilers of my steel, made twenty-two years ago, still in use?" To which Mr. Richardson replied: "The six boilers of your steel, made twenty-two years ago, are still in use, and have no appearance of corrosion." That was the best answer he could give to the assumption that Bessemer steel lasted about one-third the time of common Staffordshire plates. Had common Staffordshire plates been used twenty-two years ago he fancied that the boilers would be in a rather queer condition at the present time. Mr. Richardson's practice was to overhaul his boilers annually, and thoroughly investigate them; and as the boilers in question were experimental ones, and put up so long ago, he had no doubt that a strict examination had gone on in connection with them, so that Mr. Richardson would well know whether they were corroded or not. This statement was, he thought, the best evidence that could be given that Bessemer steel, and other mild steels of a similar character, were thoroughly well adapted for the manufacture of steam boilers, and might be relied upon quite as well as Staffordshire iron, notwithstanding the results arrived at by the author.

Mr. E. Matheson said the author had dwelt entirely upon boiler tubes and ships; but there were other matters in connection with the corrosion of steel and iron which were interesting to many members of the Institution; he referred to structures like bridges, exposed not to salt water or steam, but to the weather. The corrosion of wrought iron was very serious, especially in cities and in railway tunnels, and if steel were still more sensitive to corrosion, it would, indeed, be a grave matter. It would be interesting to compare the means taken to preserve wrought iron with those used in connection with steel, and to see how one would be suited for the other. He thought that the means taken to protect wrought iron from rust were imperfectly understood. There always appeared to be an attempt to preserve the skin of the iron as it left the rolling mill, and he ventured to say that that was impossible, and was to a large extent, a mistake. Wrought iron, in passing through the rolls, coming in contact with the atmosphere, was at once oxidized; it got a black scale or oxide upon it which must ultimately fall off; and although elaborate specifications were prepared for oiling or painting such wrought iron, that treatment only postponed the evil. Oiling wrought iron was, he thought, a better protection than painting, but even that did not fulfill the purpose intended. He believed there were only two, or perhaps three, modes of protecting wrought iron from rust. One was to keep it entirely from the air. Iron built into lime or brickwork or masonry would remain for centuries almost in the same condition as when it was put in. The second plan was that of Professor Barff, exposing the iron to superheated steam, but this was possible only with pieces of moderate size. A third plan was to remove the thin scale

entirely before the painting was applied, and he thought that was seldom, if ever, done in England in any sort of structure. The only place where he knew of its being done was in Holland, where the specifications of the engineers generally described, with the greatest minuteness, how the iron was to be treated before the oil and paint were applied. The iron was treated as the galvanizers treated it in this country; it was dipped in baths of dilute acid, which removed all the black scale. After washing it was painted, and if the paint was renewed from time to time, the iron might be permanently preserved. In this country the plan was followed necessarily by galvanizers, but he believed by no one else. If the structure had parts that were inaccessible to the painter's brush, rust would be sure to destroy it. He had lately seen a curious instance of the way in which rust deteriorated structures. He had taken down a beautifully-made bridge that had been put up twenty-five years ago by Messrs. Fox and Henderson; it was the first pin-bridge made in England, and was placed over the Commercial Road at Stepney. The upper boom or box of the bridge had been riveted and calked like a boiler, and was perfectly air-tight; and the inside plates, which had never been painted, were as good as the day when they were first put in, while some of the parts exposed to the atmosphere, and ineffectually painted, had deep pits bitten out in all directions, materially weakening them. The worst part was where the iron had been brought in contact with wood, the acid of which had so destroyed it that an angle iron ½ inch thick was worn down to a knife edge. It was not often that one had the opportunity of dissecting an existing bridge, and he thought it might be interesting to mention the facts to which he had referred. Another bridge, an approach to a large terminus, put up some twenty years ago, was now being taken down (having to be widened) in the City of London; the rivet-heads were eaten away, and the T-iron stiffeners were nearly rusted away. At the present rate, in ten years he imagined the bridge would have begun to sink under its load. When he saw it there were four locomotives on it, so that it had to undergo severe strains. It would be interesting if some steel-maker or chemist would compare steel with cast iron. Like cast iron, it had been in a molten condition, and had not undergone the intermediary process of piling and laminating which wrought iron had to undergo. Cast iron, when run into a sand-mould, got a skin on it, which was a very valuable protection, and might be permanently preserved. Steel was cast in an ingot, and it might be useful to be informed, by those cognizant of such matters, how far steel cast in a sand mould was like iron cast in a sand mould; what difference there was on the surface of the steel, because it was cast in an iron ingot mould, instead of in sand, and what alteration took place in the steel by its being reheated and passed through the rolling mill. Apart from any chemical difference, there were these differences between iron and steel, and he felt sure there was a difference of surface caused by the way in which they were manufactured.

Professor F. A. Abel said the author had stated "that the commoner sorts of iron, containing the most phosphorus, resist corrosion far better than the superior kinds; and also that the harder steels, containing the greatest amount of carbon and phosphorus, are better in this respect than the softer and finer sorts." The author had further remarked that the conclusions at which he had arrived from the results of experiments had been confirmed by the recent analysis of some brands of metal under consideration. The subject being one of great interest to him, he had searched the paper, but in vain, for any facts upon which those conclusions were based. The author had very justly stated that "It is important to ascertain how far want of uniformity in composition has to do with local corrosion in metals, and particularly in steel; also how far the presence in a medium degree, or absence in a minimum degree, of impurities in iron and steel can affect their durability." Another statement made by the author was, "that the manufacturer and chemist, in their anxiety to produce a metal containing the least possible amount of impurities, and thus to attain a high standard of ductility, in depriving it, perhaps, to a greater degree than necessary, of elements such as phosphorus, carbon, &c.,

or adding to it manganese, probably thus render it more liable to corrosion." It was stated as a probability, but further on it was given as a decided fact; and he confessed that he was unable to conjecture how the probability had been converted into a matter of certainty. The author had made a general statement with regard to the proportions of phosphorus, carbon, and manganese in irons and steels. In regard to phosphorus, his argument might be to some extent borne out by that statement; with reference to carbon, the proportion in steel, according to his statement, was from two to three times that existing in wrought iron, and he had given the proportion of manganese as three or four times greater in steel than in iron. He imagined that the author wished to compare the two materials together, since, in the case of phosphorus, he had referred, not to the different proportions of phosphorus contained in one and the same material, but to the different proportions contained in the two materials. Those were the only facts bearing on the above conclusion which he had been able to find in the papers, and as, according to these, carbon and manganese were present in larger proportion in steel than in wrought iron, he should have imagined that they ought, according to the author's conclusions, to exert their protective influence to a far greater degree upon steel than they did upon wrought iron. That was the extent to which he had received instruction in reference to the impurities contained in wrought iron and steel, which the chemist was so anxious to remove; and he confessed the conclusion at which he had arrived was that, as regarded "assumptions and theories," the author did not stand upon much higher level than those whom he had condemned. The author had been very severe upon those who indulged in the view that possibly galvanic action might have something to do with the deterioration of boiler plates, whether iron or steel, but he thought there were very few persons, even among those who were not what the author had called indiscriminate partisans of mild steel, who shared that view, and who were not convinced that even in one and the same piece of plate galvanic action might come into play very decidedly in promoting the corrosion of the metal. In reply to the possible objection that more definite results would have been obtained from more extensive trials, the author had stated that all experiments in which the conditions were not precisely similar, could not be considered satisfactory; and Professor Abel thought there were few who had experimented with a view to obtaining precision of results, who would not most heartily agree with him in that respect. But there, again, he could not quite understand how such very strong conclusions as the author had drawn could be based upon the results of experiments made by the distribution of large numbers of plates to different ships, in which they had received all possible kinds of treatment, especially when he found that the results of those various experiments with fifty-six sets of plates were lumped together afterwards, and that the conclusions were drawn from the average loss of weight of the plates in those remarkably various experiments. He was one of those who considered that among the most dangerously misleading figures with which practical and scientific men could have to deal were so-called averages. No doubt the results of the various experiments were interesting, as illustrating the character of the corrosion caused by different treatment, such as the water used, the different practices of blowing off, and the comparative effects of chemical agents. It was, however, from those various experiments that a definite conclusion was drawn with regard to the stability of steel as compared with iron, for the author stated, "These results clearly prove that the conclusions arrived at by many experienced engineers and chemists as to the causes of corrosion in boilers, previous to the appointment of the Boiler Committee, were erroneous." He should have been glad if a statement of those conclusions had been given, in order that their fallacy might have been demonstrated; for he should have been most anxious to learn from such an extensive and instructive series of experiments as the Boiler Committee had the power to institute, and did institute, to what extent previously existing conclusions were wrong. The author had also stated that "in spite of these reasonings, it is undoubtedly a fact, that under almost all circumstances iron, and particularly the

harder classes, is far superior to the finer steels in its resistance to corrosion, and this the experiments described by the author incontestably prove." That was a very strong statement, but he had no doubt that the author had considered himself perfectly justified in making it. He must, however, pardon those who were unable, from the summary of experiments which he had given, to arrive at the same conclusion. Taking even the averages upon which the author relied, this statement was not at all well carried out, for he could hardly consider that 10 per cent. difference in average loss would be so great in practical experiments as to constitute an "incontestable proof" of the difference between the durability of two descriptions of material. Further on he found that there was only a difference of 5 per cent. to establish the same point "incontestably." With regard to the averages of the experiments made on board the many ships, there were differences, in one case between crucible steel and Staffordshire iron, amounting to 0.3 per cent. only upon the weight of the total metal employed, and in another case between Siemens steel and Yorkshire iron of less than 1 per cent. upon the total weight. He could hardly accept results of that kind as "incontestable" proof that the superiority of iron over steel had been established. Again, in some of the special experiments made by the author, it had been "incontestably proved" that iron was on an equality with steel, and even that steel was superior to iron. Taking all these points into consideration, although he had read the paper with great interest, he could not admit that the author had in the least degree established, by the "facts" with which alone he proposed to deal, the "incontestable" superiority of wrought iron over steel as a material for boilers.

Mr. E. A. Cowper wished to say a few words in reference to the bridge to which Mr. Matheson had alluded. He had constructed it more than thirty years ago for the late firm of Fox, Henderson and Co. It was in the Commercial Road, and was 120 feet span, and had been lately pulled down to increase the width of the bridge. He believed it was the first bow-and-string bridge made of iron in this country; the bow flange was of a box form, with an overhanging top section. The span of the bridge did not require that the rib should be large enough to let a man pass through. There were vertical ties and large bolts which would prevent this; therefore the box was built as air-tight as it could be—not absolutely, but so close that the air could not work in and out with freedom. In that way the inside plates, he believed, were protected very fairly, until it had been lately pulled down. Mr. Matheson had not said whether the inside plates were in a perfect condition, but he believed that they were so. In some of the outside plates, where wood had been placed against the side of the box, the iron was corroded, and in some cases very badly, no doubt from the tannic or gallic acid in the wood. But where bridges were made with laminated plates, as they were in many instances—notably in one near at hand where corrosion was going on rapidly—he imagined that in a few years there would be no plates left. It was impossible, with a number of plates riveted together, to get them all so air-tight at the edges as entirely to exclude the atmosphere, and especially the damp air coming from the chimneys of locomotives. The mode of protection suggested by Mr. Matheson was no doubt a good one, but he would suggest that a further protection should be adopted by forcing varnish or boiled oil, or whatever might be the best material, between the plates by a powerful syringe; the oil might not go all the way in, but it would go as far as the air would, and two or three injections might entirely stop the entrance of air. In that way he believed that many bridges of laminated plates which were now in a state of deterioration might be preserved from further deterioration between the plates. Thirty years was too short a life for a bridge, and some improved method of protection ought to be adopted.

Mr. Peter Samson remarked that the paper was bristling with facts which were exceedingly interesting and useful to Engineers, especially to those who were entrusted with the maintenance of such structures as bridges, ships and boilers. At the same time engineers ought to be exceedingly cautious in drawing deductions from those experiments, otherwise they might, without ground, get alarmed and scared from the use of mild steel.

He thought it would have added to the value of the paper if the loss from corrosion had been given per unit of time as well as per unit of surface, because it was impossible to compare one experiment with another, or to compare the experiments with the actual corrosion going on in boilers unless that were done. He had calculated the loss of weight of the iron plates as given in the tables per square foot of surface per month, and he had found that it averaged 55.6 grains, the greatest loss being 170 grains, and the lowest 10.9 grains. That corrosion, although it might appear large, was small in amount when compared with the active corrosion going on in some boilers. Taking the average corrosion of the experimental plates as a standard by which to arrive at the durability of a ½-inch plate, 1 foot square, if corroded on one side only, he found that it would take more than two hundred years to corrode the plate entirely away. Comparing this with the average life of a well-kept marine boiler, which was only about ten years—although it was not an uncommon thing for a ½-inch plate in a boiler to be corroded away in a very few years—it was evident that the conditions under which the experiments were made, and those actually going on in boilers were entirely different; and the question arose, what effect the difference of conditions had on the results of the experiments? It was important to note that the percentage in favor of iron was 45.4 in the case of the tubes referred to in Table II., and only 10.9 in the case of the feed-water heater plates, where the rate of corrosion was about 70 per cent. greater. It would be exceedingly interesting to know the author's views as regarded the corrosion being so little in the experimental plates compared with that in actual boilers. Referring to the experiments which the author had made at home, he thought those results would be found useful and interesting, but they required some explanation. In those referred to as set 98 the corrosion in the first twelve months was considerably higher than that during the second twelve months, and as there was no apparent reason why it should be so, especially as the plates must have been scraped and cleaned for weighing at the end of the first period, he thought it would be useful to have the author's explanation on that point. It would also be interesting to have his opinion as to why the corrosion in the experiments marked 102 was so much less than in those marked 98. Possibly it might be due to the water in the former series being kept in a vessel without being changed, and if that was the case it would tend to prove that the water in boilers should be blown out as seldom as possible. Referring to the experiments with the crucible steel, it would be found that the plate which was placed in the water-butt corroded more than that placed in the tank, and that the plate in the kitchen boiler corroded less than the plate in the tank. The difference might have arisen in this way: in the water-butt, the corrosive agents were very active, owing to their not having previously come in contact with a substance for which they had an affinity, and combined freely with the experimental plates suspended therein. They were then carried on to the tank, where the second set of plates were placed, but being weakened by their first attack, less corrosion occurred, and they finally passed into the kitchen boiler weaker still, and therefore less able to combine with the last set of plates.

Mr. D. Phillips in reply observed that his sole purpose in writing the paper had been to lay before the Institution facts which had come to his knowledge regarding the comparative corrosion of iron and mild steel. He had no wish to depreciate mild steel. True, the paper showed that that metal had defects; but these, he was of opinion, could be counteracted with further knowledge. It would not do the cause of mild steel any good to conceal its faults, for without clear knowledge of the defects of steel it would be impossible to remedy them. Many of the criticisms had been wide of the mark. Whilst the paper dealt chiefly with the corrosion of iron and steel having bright surfaces, considerable time had been spent in discussing the mechanical qualities of steel, and its liability to corrode with or without black or magnetic oxide.

He could not but admire the convenient way in which Dr. Siemens had attempted to prove from the figures in the paper that its conclusions were erroneous. Dismissing the tube and disk experiments as worthless, and skipping the experiments in the two tugs and the feed-

water heater, and those with Lowmoor iron and Bolton steel, he observed that in the test of the Landore steel and the Lowmoor iron, the latter proved only 4.8 per cent. better than steel; but this was in fresh water, and only for six months. Referring to Table VII. and the remarks following, he said that in this case the iron lost more than twice as much as the steel; but then the iron mentioned as having been tested in a former experiment was under trial over twice the time that the steel was. Ignoring the experiments in ocean and coast-going steamers, perhaps the most important of all to those engaged in steam-shipping, Dr. Siemens proceeded to pick out from Table IX. such results as suited his purpose. In set 98, he compared the Landore steel with the Yorkshire iron, whilst in set 99 he compared it with Staffordshire iron. Passing over sets 100 and 101, in which both the steels gave bad results, he wound up with set 102. Now this was hardly an impartial way of treating the matter. To take into consideration only such facts as confirmed one's own opinions was a species of special pleading upon which Dr. Siemens could not be congratulated. In sets 99 and 102 the corrosion was scarcely perceptible. The D.D. 102, to which Dr. Siemens drew attention, had a patch of cinder in it, which was picked out before testing, but after it was weighed; the loss of weight should therefore have been approximately reduced. In calculating the percentages in Tables VIII. and IX., the two steels and the two irons were compared, the difference between each steel being but trifling. The results obtained, after a little over two years' trial, from the metals tested in the tube apparatus, were considered by Dr. Siemens to be valueless; but when he was better acquainted with the conditions to which the apparatus and the tubes had been subjected, this opinion would probably be modified. Mr. Phillips possessed, from a scientific point of view, only a scanty knowledge of the effects of electricity on metals, but he claimed to have a fair amount of knowledge as to the nature of corrosion in marine boilers and its causes, and he failed to see why, in the tube experiments, with metals so closely allied to each other, one metal should feel the effect of electricity more than another. He thought it would be admitted that neither in the steam chamber nor in the condensing chamber were there present conditions such as would promote galvanic action between the iron of the apparatus and the tubes, or between the tubes themselves, nor after the experiment was any such action to be perceived. At, and a little above, the water level in the condensing chamber the tubes suffered severely, but below and above that point there was scarcely any corrosion. Again, in the steam chamber the surfaces, especially of the cold-drawn tubes, were scarcely affected. There were no disks or rods in any of these tubes. The conditions necessary to promote galvanic action inside the tubes were also absent, except, perhaps, in set 2, which contained salt water. The other three sets contained fresh water. In set 2, galvanic action could only take place between the iron plugs and the bottom ends of the tubes, but there were no signs of such action. If, as Dr. Siemens remarked, the common iron alone was compared with the Bessemer steel tubes, the difference was nearly 200, not 300, per cent. in favor of the iron; but in Table II. the losses in all the tubes were given, and in Table III. the losses in ten samples of each metal, under precisely similar conditions. In calculating the percentages resulting from them, he had given the true results. The tube apparatus had been designed by him at the suggestion of the Chairman of the Boiler Committee, Admiral C. Murray Aynesley, for testing tubes of various kinds of metals. Mr. Farquharson had nothing to do with it, and his remarks as to magnetic oxide showed that he did not understand the conditions under which the tubes had been tested.

Mr. Phillips could not understand Dr. Siemens when he said "there were substances, compounds of iron, manganese, and silicon, sold for steel, which no doubt corroded very rapidly." The metal apparently referred to by Dr. Siemens was made by the Bolton Steel and Iron Company, on the Bessemer principle, and was considered by most practical men to be as good, in every way, as Landore steel. The weighing of the specimens in the experiments had been carefully done, chiefly by Mr. Tookey, one of the members of the first commit-

tee, and by Mr. Ireland, one of the members of the second committee, every plate, disk, or tube having been separately weighed.

As regarded the effect of magnetic oxide on steel, Dr. Siemens did not make it clear whether iron suffered from its oxide in the same manner and proportion as steel was said to do, nor as to the conditions under which this action took place. From what Mr. Phillips had been able to gather in this direction, the oxide could only affect a metal to which it was partially attached in salt water at ordinary temperatures, its effect in cold fresh water, and in fresh and sea water of high temperatures, such as in marine boilers being absolutely nil. Further, every practical engineer knew only too well that this magnetic oxide in a marine boiler, especially below the level of the water, soon disappeared, much sooner, indeed, than could be wished. With the exception of the small plates and the welded tubes, all the specimens in the experiments were effectually freed of their oxides, either by planing, filing, or grinding, so that the remarks of some of the speakers as to the black oxide were hardly to the point. According to Dr. Siemens, the fact, if fact it were, that black oxide materially assisted the corrosion of steel, was a recent discovery of Mr. Barnaby, or of one of his staff, but not in connection with the working of marine boilers. Professor Williamson had, however, pointed it out to the committee in 1874. It might prove of value to shipowners and builders, but if not it would not want companions in the limbo of neglected discoveries. The first volume issued by the Boiler Committee was full of theories as to galvanic action, which, with the better knowledge now existing of the working of boilers, were considered unworthy of serious consideration.

The remarks of Mr. Traill and Mr. Martell contained nothing new and nothing to comment upon, except, perhaps, the instance of extraordinary corrosion mentioned by the latter. Mr. Martell, attributed this, as many others would do, to galvanic action between the iron rivets and the steel plates. Had the action taken place below the light water-mark, there would be some reason for attributing it to galvanic action, but in his opinion, had the rivets been of the same material as the plates, a similar corrosion would have taken place. Mr. Martell had remarked, in May, 1879, "that it was not uncommon to find soft iron rivet points in iron ships somewhat pitted or worn within the surface, but the converse action of the plate wearing around the rivets was peculiar." This seemed to confirm Mr. Martell's view that the softer or purer metals did not resist corrosion so well as the harder sorts, though the action would, in such cases, be assisted by a sort of breathing or springing of the plates in a line with the rivets, especially if the plates were thinner than they would have been if of iron, by which, and friction, the paint was soon removed. Mr. Martell had no doubt often seen the sides of iron ships corroded similarly, though not to such a serious extent, after a long voyage in the tropics; he had himself seen many, amongst others two old iron hulks in Bombay harbor, completely riddled, a little above the water line from the effect now of air, now of salt water and want of attention. According, however, to the experience of Messrs. Martell, Barnaby, and Donaldson, deterioration went on in steel hulls which never occurred in iron hulls. In one case this was attributed to galvanic action between the iron rivets and the steel plates; in the other two cases to galvanic action between the steel plates and their oxides. Mr. Barnaby got over the difficulty by removing the magnetic oxide by pickling the plates, and Mr. Donaldson by galvanizing them with zinc. Why should this be necessary with steel hulls, and not with iron hulls? Practical men, he thought, would pause before they adopted either plan, and would consider how it was that the oxide on iron plates did not produce the same effect as that on steel plates.

It had been mentioned by Mr. Barnaby, not for the first time, that the results of some experiments carried out by the Admiralty contradicted those given in the paper. In Paris, in 1878, Mr. Barnaby said that the Admiralty "had made some experiments extending over four years, and although at first the pieces of steel suffered more loss than the pieces of iron, when they were put unpainted on the bottom of the ship in salt water, as times went on they discovered that the loss did not continue, and that, so far as these

experiments went, steel was at least as good, and it appeared to him to be better, than iron." In London, in 1879, he said "in the matter of oxidation in salt water, we have found by a series of trials extending over about three and a half years, that the rate of oxidation of three plates of iron of the same brand, differed more among themselves than they differed from steel; that when the surfaces of steel plates are carefully freed from the black oxide produced in the rolls by a wash of weak acid, or otherwise, the surface corrosion in salt water is very uniform; that when the surface oxide is left on, the effect of the oxide on the neighboring bared metal is as strong and continuous as copper would be." Now, he was curious to know what experiments Mr. Barnaby referred to. Experiments had been made by the Admiralty with iron and steel plates at Portsmouth and Devonport, between 1874 and 1877, to test their comparative durability in sea water and in bilge water. The specimens tested and the results obtained came under the notice of Admiral Aynsley, Mr. Tookey, and himself; they ascertained that the steel plates tried at Portsmouth lost 80 ozs. 341 grains, and the iron plates 61 ozs. 52 grains, or 24 per cent less. In the experiment at Devonport, which lasted a much shorter time than that at Portsmouth, the steel plates lost 43½ ozs., the iron plates 26½ ozs., or 64 per cent. in favor of the iron. The dockyard officers reported that the corrosion in the steel plates was more severe and irregular than in the iron. Surely these experiments were not those to which Mr. Barnaby alluded. In a letter addressed to the Controller of the Navy, dated the 6th of June, 1877, the committee pointed out the confirmation the results obtained at Portsmouth gave to their own conclusions. He would ask, too, why these results, and those of other experiments since carried out by the Constructor's Department, had not been made known? In such an important question the Mercantile Marine was as much interested as the Admiralty.

Some time had been devoted by Sir Henry Bessemer to show the different conditions to which plates suspended in boilers, and boilers themselves, were subjected. That was hardly necessary. One of the objects of the committee's experiments was to ascertain the comparative endurance, as regarded corrosion, of different metals when under similar conditions, and this object they had attained. Sir Henry Bessemer had quoted Messrs. Richardson and Adamson, and it was always interesting to hear the experience of such practical men. But these gentlemen had to deal with land boilers, worked with water comparatively harmless to the materials of which they were composed. Mr. Adamson had been principally concerned with the construction of boilers; and he would ask why the results of the experience of these gentlemen, or of others, with iron boilers and steel marine boilers, had not been given? Mention had also been made by Sir Henry Bessemer of some steel boilers at Oldham, which had lasted twenty-two years, and which were now said to show no signs of corrosion, though this last remark could hardly have been seriously meant, and he opined that had these boilers been made twenty-two years ago of Staffordshire iron they would now have been in a very dilapidated condition. When the Boiler Committee visited Oldham in November, 1874, Mr. Richardson was engaged in removing and resetting iron boilers which had already done duty for more than eighteen years, and which were, he had been told, still at work. The shells of these boilers were, as usual, of ordinary iron. Again, at Wigan, in 1875, he had seen some iron boilers which were more than thirty-one years old, working at pressures of from 60 lbs. to 70 lbs. to the square inch, the front ends of the flues only, over the fires, having been renewed; these, he was informed, were still at work. The following was a copy of a letter, dated 7th April, 1881, which he had received from Mr. Mason, Superintendent Engineer of the Furness Railways: "Dear sir, replying to your favor of yesterday's date, the boiler of Firefly, paddle steamer, Lake Windermere, was put in in 1848, that of the Dragonfly in 1850; both were broken up last year. Plates (internally) good, having the bloom still on them. Boilers of Walney were put in in 1868, taken out in 1878. Steel boiler completely done. Iron boiler might have lasted three or four years longer with care. Yours, &c., R. Mason." This vessel had since been fitted with two iron boilers. In 1876 he brought

with him from one of the boilers on Lake Windermere a piece of iron tube which had been in use rather more than eighteen years. On the water-side of the tubes, furnaces, &c., there was very little corrosion, but the surfaces exposed to the action of fire, sulphur and the atmosphere, had suffered a great deal, especially round the lower part of the fronts. Here were cases of iron boilers lasting over thirty years on board steamers, as well as on land.

It had been remarked by Mr. Ravenhill that if steel wasted as quickly as the specimens in set 100, "there ought now to be little of the original steel left" of the Dover boats. These boats made runs of about one hour and three-quarters' duration, allowing of their sides being examined and painted daily, if necessary, whilst the set of plates (100) were under a very severe test, and not protected—not even by their oxides; both sides of the experimental plates were wetted daily and exposed to the weather, and it was only to be supposed that they would suffer more from corrosion than plates in ships' sides painted and taken care of. He could not admit that the report received by Mr. Ravenhill from Captain Dent gave such evidence as was desirable in these matters. Indeed, he had heard that the steel boilers in the Duke of Sutherland, one of the Holyhead boats, had suffered more than the iron boilers.

With regard to Mr. Matheson's remarks, if iron and steel were kept dry, or well looked after and painted, they would suffer but little, if any, corrosion; also with care the magnetic oxide would remain attached to the surfaces of iron structures, under ordinary conditions, for years, and would, while it remained, effectually retard corrosion.

In reply to Professor Abel he contended that the results of the experiments he had described confirmed his conclusions. If the method of giving averages were unfair, the gross results would still uphold this view. He would refer any one desiring fuller information concerning the ocean plate and tube experiments, and the experiments in the tugs, &c., to the Blue Book just issued by the second Admiralty Boiler Committee. Professor Abel went on to say that since there was more carbon and manganese in steel than in iron, these elements should afford steel greater protection than iron. Carbon undoubtedly did so, but the part manganese played in this respect was doubtful, probably the reverse; taking, however, all the so-called impurities in iron and steel together, there was a greater sum total of foreign matter in common iron than in mild steel, and these foreign impurities or ingredients might be the cause of common iron corroding less than mild steel. Professor Abel said that he stated this as a fact; that was not so; he stated as a fact that the metals which contained most impurities, especially those containing the most phosphorus, resisted corrosion better than the more refined sorts. More than one scientific man supported the views he had advanced as to the class of metal which suffered most from corrosion; and Dr. Frankland held the same opinion as himself regarding galvanic action in boilers. He would refer to the summary of the evidence given before the Boiler Committee in 1874 by such authorities as the late Dr. Letheby, Dr. A. W. Williamson, Dr. E. Frankland, Dr. J. Percy, Mr. G. J. Snelus, and Mr. W. Weston, and from speeches made at the meetings of the Iron and Steel Institute in 1878-9, by Mr. I. Lothian Bell, Dr. Siemens, and Mr. E. Riley, and it would be seen what different conclusions such gentlemen came to. He regretted that Professor Abel should have confined his remarks to criticism without endeavoring to throw any light on the subject of the paper.

Two of the experiments described by Mr. Phillips showed only 5 and 10 per cent. in favor of the iron, and this it was said was not "proof incontestable," that iron was superior to steel as regarded corrosion. If even these percentages were added to the 13, 20, or 25 per cent. reduction in the sectional area of steel plates and scantlings in ships or boilers accepted by the Board of Trade and by Lloyd's, which matter had been conveniently forgotten in the discussion, or if the corrosion were assumed to be equal in the metals, the result would be, if not fatal, very damaging for steel.

The greatest care was taken that the specimens in each experiment should be exposed to exactly the same treatment. With regard to the ocean-plate experi-

ments, when fifty-six sets of test plates had been subjected to the various sorts of treatment of boilers now in practice, and furnished a percentage of 21.3 in favor of the irons over the mild steels, it was not only fair, but reasonable, to infer that iron on the whole withstood the corrosive effects of those treatments better than steel. That the treatment of boilers might be so modified as to alter this state of things he had already suggested, and indeed had often put practically to the proof.

In reply to Mr. Samson's queries regarding the results of the experiments given in Table IX., the greater corrosion in sets 98 and 100 during the first period of their trial than in the second period, was due in his opinion to the weather in the summer of 1879 being so much more changeable than in 1880. Mr. Samson had furnished the reply he should have given as to the greater corrosion in set 98 than in set 102, viz., that the vessel containing set 98 was much larger than that containing set 102, and that the water in it was constantly changed. The tank and boiler containing the crucible steel were small, and the water in the boiler was kept at a temperature of from 120° to 212° during about thirteen hours daily, which would account for the difference in the corrosion in the three plates.

It had been calculated by Mr. Samson from some of the experiments described in the paper, that it would take two hundred years to corrode boilers entirely away; several cases were mentioned in the Blue Book in the ocean-plate experiments, when a ⅜-inch plate would be entirely gone in from two and a half to five years, while other plates would last more than thirty years, both sides of the plates being exposed to the water in the boilers. In other instances extraordinary differences would be found as regarded the amount of corrosion sustained by one metal compared with others of the same set, besides those given in the paper. In set 24 the loss of the steels was more than twice that of the irons; in set 32 more than half as much again; in set 33 the loss of the Landore steel was more than twice that of the Bolton steels; in set 66 the steels lost nearly three times, and the Lowmoor iron nearly twice as much as the Staffordshire iron.

He had been informed that the experiments carried out and concluded some time ago by Mr. Parker, Chief Engineer Surveyor to Lloyd's, confirmed the results given in the paper, the percentage in favor of the irons, taking all the results, being over 19, and in favor of the rough over the bright specimens 12. It was to be regretted that the conditions and results of these experiments in detail were not made known to those interested immediately they were obtained.

In conclusion he would ask why it was that iron tubes had been nearly always substituted for steel tubes in steel boilers, and why iron tubes were now put into almost every boiler, even though made otherwise of steel? Why the Admiralty should now be going back (as stated by Mr. Barnaby) to the use of iron furnaces and combustion chambers, whilst making boiler shells of steel? And why composite boilers of iron and steel should be made at all, especially by the Admiralty, after Mr. Farquharson's discovery as to the effect of magnetic oxide on steel, and the galvanic action which was asserted to be set up between iron and steel? Neither Mr. Farquharson nor Dr. Siemens threw light on the effect, if any, of this oxide on iron, nor as to the action between iron and steel, nor between these metals and their oxides at high temperatures in marine boilers. If Mr. Farquharson's discovery, or the theories of galvanic action were to be relied on, there would by this time have been very little steel left in the composite boilers made by the Admiralty and mentioned in the paper. He believed that when the question as to the effect of magnetic oxide had been more practically considered, it would be found that the local corrosion was not due in iron or steel to galvanic action, but to other causes, frequently to perfect protection being afforded, for a time, to parts of the surface by the now much abused oxide, the adjoining parts being unprotected, or to want of homogeneity, or uniformity in composition, or both. When portions of surface were so protected as to be comparatively unaffected, the difference between them and the adjoining affected parts was most pointed, and to unpractised eyes very deceptive. Some of the longest-lived marine boilers had either brass tubes or back copper tube plates, or both.

TESTING MACHINES, THEIR HISTORY, CONSTRUCTION AND USE.

By ARTHUR V. ABBOTT.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

I.

HISTORY.

To a celestial visitor, approaching the globe toward the Western Hemisphere, the American Continent would appear like the web of some gigantic arachnid. Long lines of glistening black threads, running from point to point, and here and there converging to a focus, would, on a nearer examination, be decomposed into parallel lines of railway, with accompanying telegraph wires. Spider-like bridges of wood and iron span the rivers, while over the waters, under them, glide all sorts of naval structures, bearing the commerce of a nation. A still closer inspection would decompose the foci of the railways into busy towns and cities, whose substantial buildings, of wood and stone and iron, bear testimony to the truth of the saying; that the prosperity of a nation may be measured by the success of the people as constructors. Thus, to conquer time and space by joining the two oceans with the railway and the telegraph; to drive a five thousand ton ship across the Atlantic in the face of adverse gales; to erect the structures that now span the Mississippi, the Niagara and the East River; and to dot the continent with buildings, such as may be seen in the streets of any of our cities; would seem to require the most intimate conceivable knowledge of all the properties of all the materials used in construction. Yet such is far from being the case. Indeed the art of construction, perhaps more than all others, is involved in mystery and obscurity; unless the fact that engineers, having arrived at a point where they are ready to say that they do not know, may be credited with having gained a long stride toward perfect knowledge. This unacquaintance with the subject is by no means unaccountable, for a little consideration will show that it is one of the greatest complexity. In designing an iron bridge, the engineer is at once brought face to face with such a catalogue of operative causes affecting the quality of the material to be used, each one having the power to vary so greatly the iron, that only the keenest foresight, and the most subtle analysis, can predict the result. A slight variation in the ore changing the relative proportion of carbon, phosphorus or sulphur, may give rise to the greatest differences in the refined metal. Changes of manipulation in the rolling mill, apparently slight, such as differences in the temperature at the time of rolling, or variation in the rapidity of the reduction of the rolls, are capable of enhancing or spoiling the value of the product. The knowledge of materials is at present, at least, an absolutely empirical one, for those who are most skilled do not hesitate to admit that any predictions from existing data regarding a new material, or the effects of a new process on an old one, are hazardous in the extreme, and should be received with the greatest caution. "Experimenta docet" seems to be the only motto for this science. Before the constructor makes use of either a new material, or an old one in a new form, the only safe method is to experiment with the piece in question, carrying the researches far enough to demonstrate all the physical properties, so that the qualifications for the work in question may be accurately known. Thus, and thus only, can the engineer inspire confidence in the structures that he plans, and by a knowledge superior to that of his competitors exhibit such an economy and fitness of design as shall ensure success.

To make experiments on large pieces of iron and steel, having a tenacity of from fifty to one hundred thousand pounds per square inch of section, requires the use of very expensive and ponderous machinery, to which may be at-

tributed the slow progress that has attended investigations of this character. The entire history of tests, and testing machines in this country may be comprised within the last thirty years. One of the first machines for making physical tests was designed and built by the late Major Wade, for the United States Government, in 1855 and 1856, and was used in making experiments on the cast iron intended for the ordnance service. A little later this machine was remodeled and improved by Capt. Rodman. Two of the Rodman machines are now in use by the Government, one being in the Washington Navy Yard, and one in the Army Building New York City. These machines consisted of a heavy frame of cast iron carrying three levers, the last and smallest one being used as a scale beam. One end of the specimen (which was turned so as to have two collars on each end) was secured to the iron frame, and the other end to the large lever of the scale system. This whole scale system was then lifted by means of a screw and compound gearing, thus producing stress on the specimen, which was estimated by placing weights on the scale beam. Contemporaneously, the late John A. Roebling was engaged in making experiments on the properties of wire at his works in Trenton, and investigating the qualities of rolled iron beams at the works of the New Jersey Iron and Steel Co. A little later Geo. W. Plympton, now Professor of Physical Science in the Brooklyn Polytechnic, made numerous experiments on some full-sized rods designed to be employed in bridges built by Murphy & Whipple. This was one of the first testing machines built to make experiments on full-sized bridge members, and was of unusual dimensions. It consisted of a heavy framework composed of yellow pine timbers 16 ins. by 20 ins., and thirty feet long, set 18 ins. apart, and secured against side springing by the pieces of oak. The stresses were applied by a screw, three ins. in diameter, working through a thick iron plate, and one of the oak end pieces. The outer end of the screw was furnished with a rachet wheel 12 ins. in diameter, which worked on the extended screw as a fulcrum. A stout rachet on the lever completed the movable outside portion on the head of the machine. Inside the head-piece the screw terminated in a stout cast iron cross-head, in which it turned freely. The cross-head carried a pair of parallel flat bars to which the rod under test was made fast. To facilitate the fastening, and to render the machine adjustable for testing rods of different lengths, the parallel bars were furnished with holes 6 ins. apart, and a sliding cross-head that could be attached to the bars by means of pins placed in these holes. At the other end of the machine the stresses were measured by a balance beam 10 ft. long. A heavy iron bolt carried through the head-piece was (after being passed through a thick iron plate which formed the end of the balance beam) forked so as to hold securely a steel block, with a face at right angles to the axis of the rod; this face was made to bear against a knife edge when the machine was in operation. The balance beam was constructed like a king-post truss, with an extra rod following the line of the inclined studs. At the end next to the machine the plates above referred to were secured by bolts so as to form the extreme end of the lever. The plate had two blunt knife edges, one on each side; the one toward the machine rested on a plate attached to the oak head-piece; the knife edge on the other side, which was $\frac{1}{10}$ of a foot higher up, had for a bearing, the steel block which received the stress from the rod under test. To the extremity of the balance beam was hung a platform to receive weights. It will be seen that the tensile forces were measured by a bent lever, whose arms were respectively $\frac{1}{10}$ of a ft. and 10 ft. The machine was designed and built by Mr. J. W. Murphy of Philadelphia.

The outbreak of the war diverted the thought of the country to other channels, and little more was heard of testing machines till shortly after its close, when the scale apparatus of a large machine was built for Colt's armory by Fairbanks & Co. This machine is illustrated in Fig. 1, and had a capacity of 100,000 lbs. It has a platform a, with a central opening, through which pass two screws, b, entering the cross-head c, and connected at their lower ends to two arms of a forked lever below the floor. The long arm of this lever is coupled with the differential levers d, e, f, which act on the scale beam in such a manner that by depressing the free end

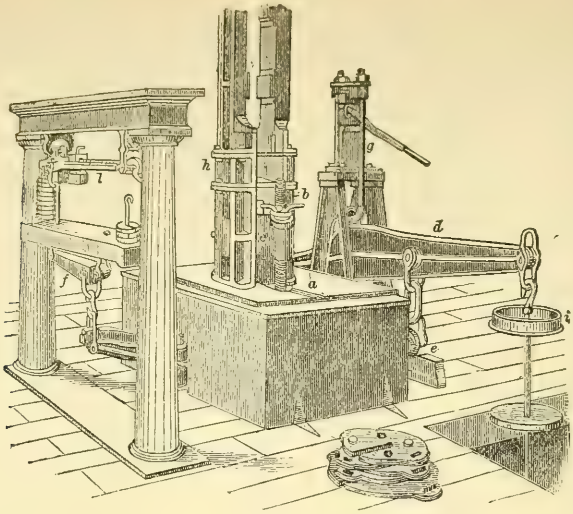


Fig. 1.

of the lever d, the cross-head is pulled down. This same effect may be produced by raising the fulcrum of the lever with the hydraulic jack g. The machine was fitted with appliances to make test in tension, compression and transverse stress.

This was the first *platform* testing machines ever built.

cross-head for holding the upper end of the specimen. Through the platform run two screws operating the lower cross-head, and moved by the gearing and hand wheel at the side of the machine. The force produced by the screws, whether in tension, compression or transverse stress, is brought on the

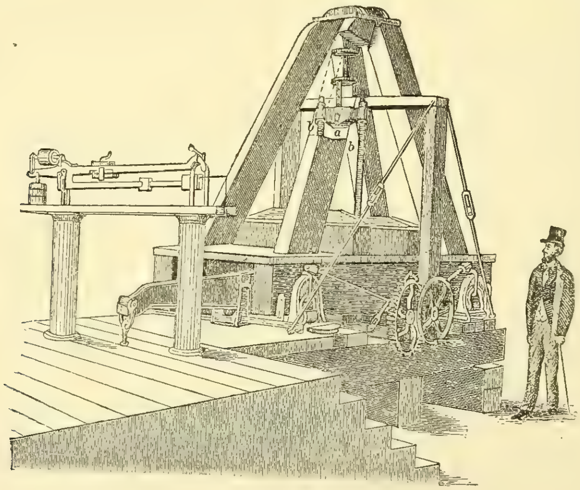


Fig. 2.

Another machine was soon built by Fairbanks & Co. for Columbia College, New York. Fig. 2 presents a view of this machine. A large platform supports four heavy timber columns that carry a top

platform and estimated by the beam. This machine is fitted with a clockwork attachment for automatically moving out the poise on the beam as fast as the stress increases in the test piece. From

these machines as a starting point, Fairbanks & Co. have continued with constant improvements the manufacture of testing machines. Much interest was now manifested in the subject of testing, and the firm of Riehle Bros., of Philadelphia, started in the manufacture of all kinds of machines as regular articles of commerce.

At a convention of the American Society of Civil Engineers held at Chicago, June 5, 1872, it was, on motion of Gen. Sooy Smith, resolved, that:

"*Whereas*, American engineers are now mainly dependent upon formulæ for the calculation of strength of the different forms of iron and steel not based on experiments upon American materials and manufacture; and, whereas, these differ greatly in many of their characteristics from those of foreign production, both in their nature and form; therefore,

Resolved, that a committee of five be appointed to urge upon the United States Government the importance of a thorough and complete series of tests of American iron and steel, and of the great value of the formulæ to be deduced from such experiments.

Pursuant to this resolution a committee was appointed by whose efforts Congress was induced to pass a law for the appointment of a United States board to test iron and steel, and an appropriation of 75,000 dollars was made for that purpose. This board consisted of Col. T. T. S. Laidley, Ord. Dept. U. S. A.; Commander L. A. Beardslee, U. S. N.; Lieut-Col. Q. A. Gillmore, U. S. A.; Chief Engineer D. Smith, U.S.N.; Gen. W. Sooy Smith, C. E.; A. L. Holley, C. E., and R, H. Thurston, A. M. C. E., Secretary. One of the first steps of the board was to secure an adequate machine for making the intended tests; for at that time there were no machines in the country capable of making accurate experiments on specimens of more than a square inch of steel. After a thorough investigation of the subject a contract to build a machine having a capacity of 1,000,000 lbs. was awarded to Mr. A. H. Emery, of Chicopee, Mass. During the four years that elapsed prior to the completion of the machine the board did a very large amount of very valuable work in investigating with the Rodman machine, and the large chain-testing machine at the Washington navy yard, the character of wrought iron rods commonly used for chains; and under the direction of Prof. Thurston, at Stevens Institute, the properties of the different bronzes. In 1879 the Emery machine was completed, and the government came into possession of a machine having a capacity of 100,000,000 lbs., and capable of testing specimens (in tension and compression) up to 30 ft. of length. Unfortunately the act incorporating the board limited its life to the duration appropriation of 75,000. With the completion of the testing machine this appropriation was exhausted and the board ceased to exist without having had an opportunity to demonstrate the value of the new testing machine to the community.

At the Centennial Exhibition in 1876, Professor Thurston exhibited a testing machine for making experiments in torsion, which was provided with an apparatus whereby the amount of the stress applied to the specimen and the resulting strain were graphically recorded by the machine on a sheet of cross-section paper. This was the first testing machine arranged to autographically record the results produced by the experiments, and marks a great step in the improvement of such apparatus.

In 1877, in connection with the inspection of the steel intended for the superstructure of the East River bridge, the author of this essay designed, and constructed what is believed to have been the first testing machine for autographically recording the results of the tests made in other than torsional stresses. This machine was not a large one, having only a capacity of 100,000 lbs., and susceptible of experimenting on pieces in tension, compression and transverse strains up to 2 ft. in length, and in shearing to capacity of machine. Briefly described, it consisted of a cast iron base supporting two cast iron columns, on which was placed a differential scale beam. The end of this beam was connected with a swinging weight, so that the stress produced on the specimen caused the end of the beam to rise, swinging the weight through a measurable arc. Connected with the scale beam was a cylinder, carrying a sheet of cross-section paper, over which was mounted a little carriage, sustaining a pencil or stylographic pen. This carriage was caused to move along the axis of the cylinder by means

of a steel tape, connected with the specimen under examination. As fast as the stress was applied to the specimen, the scale beam rose, the cylinder rotated, and caused the pencil to make a mark, parallel to the axis of y, while as fast as the stress produced any deformation in the specimen, the pencil was drawn along the paper parallel to the axis of x. The combination of these two movements gave a curve, whose abscissae and ordinates indicated the amount of stress and strain produced in the specimen.

About two years ago Messrs. Fairbanks & Co. conceived the idea of establishing, in New York City, a physical laboratory completely equipped with testing machines of the most approved pattern, and supplied with such gauges, and instruments for making all kinds of physical investigations, so that engineers, and those interested in construction, could be afforded an ample opportunity to make investigation in any desired direction. The many improvements in testing machines, now to be described, had their origin in the desire of that firm to equip their "Department of Tests and Experiments" so completely that it should be speedily accepted as a standard of scientific accuracy, and should become to America what the laboratory of Kirkaldy has been to England.

Scientifically speaking, a testing machine is a piece of apparatus for the purpose of breaking samples of material, and registering the amount of stress required, as well as the strain produced thereby; and an analytical consideration of the subject divides testing machines into two general classes, namely, machines for special tests and those for general work. Each of these classes may again be subdivided, according to the position that the specimen occupies into horizontal and vertical machines.

With regard to the method of applying the stress, testing machines may be classified as screw or hydraulic machines. While having reference to the method employed for measuring the stress applied, they may be classed as lever, hydraulic scale or hydraulic gauge machines. These several classes will now be treated in their respective order.

GENERAL TESTING MACHINES.

The machines under this class are those so planned as to make experiments on material in at least three or four different ways, as for example: machines arranged to make tests in tension, compression, transverse stress, and in shearing, bulging, punching and torsion. There are, however, very few machines in the country that aim to make tests in all of the above varieties of stress. Generally speaking, the investigations have been confined to tests made in tension, compression, transverse and shearing stress. While the appliances for torsion, bulging and punching are less frequently added. The classifications into horizontal and vertical machines, simply depends upon the position that the specimen has with reference to the machine. In a vertical testing machine the specimen is placed vertically. Machines of this kind are to be preferred for many considerations, whenever the specimens are not over 5 or 6 feet in length, so as to render this method of construction feasible. In such cases the machine may be built small enough, so as to be placed in an ordinary room, and may be conveniently manipulated from the floor; whereas, when the specimen exceeds the above length, a special building would be necessary; and when large and heavy specimens are to be tested it is exceedingly difficult to handle and place them in the machine with requisite care. Vertical machines may be built of any capacity that may be desired; but of course in making experiments on short specimens it is rarely desirable to carry this capacity above 300,000 or 400,000 lbs. For testing bridge columns, eye-bars, full-sized members of all kinds; angles, channels, etc., a horizontal machine, wherein the specimen may be placed parallel with the floor of the building, becomes almost a necessity. In a machine of this type the weighing apparatus is generally placed at one end of a long frame-work, designed to carry the reactionary stresses of the machine, while the mechanism for applying the stress, either a powerful hydraulic press or corresponding screws, is placed at the other. Over head, a traveling grip or crane may be conveniently arranged, and by this device specimens of any size or weight, may be readily and conveniently placed in the testing machine, supported

during the application of the stress, and the pieces removed as soon as rupture takes place.

The classification according to the method of applying stress into screw or hydraulic machines, simply indicates the method by means of which the force is applied to the specimen. In machines of capacity under 200,000 or 300,000 lbs., intended especially for experimental work, on the qualities of materials, screw apparatus is to be preferred for the following reasons:

First, the screw is absolutely positive. By arranging the driving mechanism, so as to turn the screws either rapidly or slowly, rapidity with which the stress applied to the specimen may be varied at pleasure. As soon as the screws stop turning, the whole apparatus remains absolutely rigid, and the stress may be continued on the specimen for an indefinite length of time. It will be readily seen that this is of especial advantage in the investigations calling the elastic limit and modulus of elasticity into question. For example, should the problem be presented to determine what load a specimen will, for a long time, sustain without detriment, the piece may be placed in the testing machine, the screws turned until the requisite load is indicated on the beam, and the poise left, with positive assurance that unless the machine is wilfully touched from the exterior there can be no yielding or springing from its position, and the load on the piece will not be in any way changed. Again, the method of applying stress by means of the screw is perfectly uniform and steady, provided that the gearing, driven by the engine or other motor, be run in a steady manner. The piece is, therefore, never subjected to any shocks or jerks or to any variation in the steady increase of the stress. The screw, however, is liable to some disadvantages, for example, the co-efficient of friction is so large that even in the best built testing machines, on this plan, an efficiency of more than ten per cent. is scarcely, if ever, realized. Again, in order to obtain the requisite speed for making tests rapidly, it is necessary to drive the auxiliary gear very fast, so that the screws may be rotated with sufficient speed.

For machines of more than 200,000 or 300,000 lbs. capacity, especially those designed for making experiments on full-sized members, where extreme accuracy of the testing machine is not so rigidly required, hydraulic power is far preferable. By means of a hydraulic press, and an accumulator, or three piston pumps, for continuously supplying fluid under a maximum pressure, stress up to any desired capacity may be readily and easily applied, as rapidly as may be wished. By means of an accumulator, the fluid forced in the jack may be maintained under a continuous and uniform pressure, so long as the accumulator will run. This piece of apparatus is, however, extremely bulky, if it be designed to supply fluid to a large or highly strained press. By means of a three-piston pump, the fluid may be supplied to the press with almost as steady a flow as that obtained from the accumulator, and of course by supplying a sufficient quantity of fluid to the pump that piece of apparatus may be made to work continuously so that a jack of any length of stroke may be operated without bulky machinery. Testing machines operated by hydraulic power have the advantage of extreme rapidity of action. For example, in making many thousands of tests for the East River bridge, conducted by the author, at the Cambria Iron Co.'s works, in Johnstown, it was customary to break a bar of steel an inch square and 1 foot long, stretching some 3 or 4 inches in from 6 to 10 minutes; a rapidity which it would be hardly possible to equal in a machine worked by screw power. There is, however, a serious disadvantage in the hydraulic testing machine, from the fact that the pressure is constantly varying, even with the best regulated pieces of apparatus. If it is wished for a moment to stop the action of the pump, either to take a reading on the stretch gauge, or for any other purpose, the beam drops almost immediately, showing that even the pistons of the best packed jacks do leak quite appreciably, and allow the stress to be relieved, unless some means is arranged for retaining a constant pressure on the fluid. Of course, in machines so constructed, it is impossible to ascertain the effect of a long continued stress, unless some means of clamping, or securing the piston rod can be arranged, to prevent this release of stress by leakage around the piston. The co-efficients of friction in the hy-

draulic machines are comparatively slight, so that in well-built machines of this class it is possible, as has been demonstrated by numerous experiments, to obtain an efficiency in the machine from 30 to 50 per cent. However, the constant leakage of fluids, around the joints or packings in various parts of the apparatus, gives rise to continued annoyance, and is apt to render the test room a dirty and disagreeable place.

The most important classification of testing machines is, that involving the apparatus for estimating the amount of stress to which the specimen has been subjected. The older machines of large capacity were generally arranged to have a fixed cross-head at one end of the framework, to which one end of the specimen was attached, while the hydraulic press for applying the stress was furnished with some form of gauge for estimating the amount of fluid pressure per square inch, to which the ram of the jack was subjected. By knowing the area of the piston, a simple example in multiplication gave the amount of stress to which the ram was subjected, and deducting therefrom an estimated quantity for friction, the result was assumed to be the stress to which the specimen was subjected. This, it has been recognized, was an exceedingly crude and only approximate method of obtaining that stress. The co-efficients of friction in the jack are exceedingly variable quantities. If the interior of the jack is smooth and well polished, if the packings have been in for a long time; and have become fitted to the cylinder, and are working at that part that has been most frequently used; if the jack is driven at a comparatively low rate of speed, and if the fluid is a good lubricator and free from grit and from any foreign substance, the co-efficient of friction may fall as low as $\frac{1}{2}$ or $\frac{1}{4}$ per cent. of the entire force exercised by the fluid. But if the interior be rough, the packings, new or working, in a part of the cylinder not often used, the co-efficients may rise to the extraordinary amount of 40 per cent. of the entire pressure placed on the ram. If this co-efficient of friction were a constant quantity and susceptible of being determined once for all, it would oppose no serious obstacle to the accuracy of the testing machine. But, unfortunately new packings and new supplies of fluid are necessary, and varying conditions of the same are essential, consequently this co-efficient is constantly changing, and may give rise to results on one day that are totally different from those on the succeeding one, so that no machine depending solely and entirely on the registration given by a gauge, of the hydraulic pressure of the fluid can be accepted as giving results that are constantly reliable and accurate. As an example of this may be mentioned the following experiment: A ram having an area of 90 square inches, was on one day worked at a pressure of 1,000 lbs. to the square inch, giving an effect on the scale placed at the other end of the testing machine of only 50,000 lbs., whereas, according to gauge pressure, there should have been a result of 90,000. On the day succeeding the experiment detailed above, and without there having been any change in any of the parts of the jack, a gauge pressure of 1,000 lbs. gave a scale reading of 85,000 lbs. What was the cause of difference has been an unsolved problem.

The next most common method of estimating stress is by means of the lever or combination of levers. In machines of this class the straining apparatus is so arranged as to be attached to one end of the specimen, while the other end is held in a cross-head directly attached to the levers forming the weighing apparatus. The following examples from some of the most prominent testing-machine makers in the country may be selected as exponents of this system of estimation:

Fig. 3 represents a 100,000 lbs. testing machine, built by J. L. Gill, of Pittsburgh. The machine consists of a heavy cast iron frame-work, supporting the driving mechanism, consisting of some gears and band wheel, seen at the right hand, which are arranged to drive a powerful screw directly at the left of the machine. This screw carries a cross-head to which one end of the specimen may be attached, while directly over it may be seen a second cross-head suspended from the large lever on the top of the machine. This lever connects, by a link at the left hand, with a secondary lever directly over the band wheel, which in turn is connected to the third lever, carrying two small poises and a weight counterpoise. The stress applied by the screws and the gears

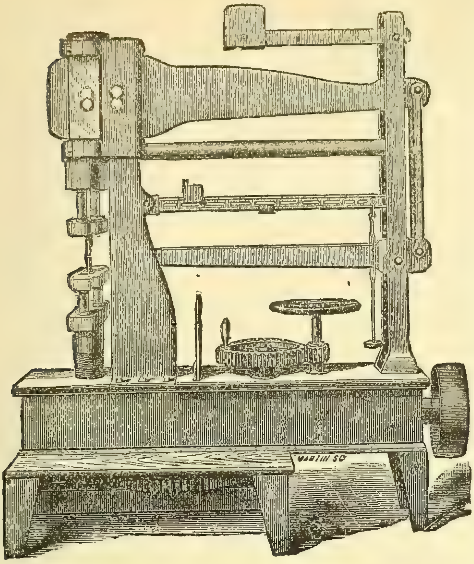


FIG. 3.

to the specimen is transmitted through it to the upper cross-head, then, acting on the poises and second levers, it causes the third, which is graduated as a beam,

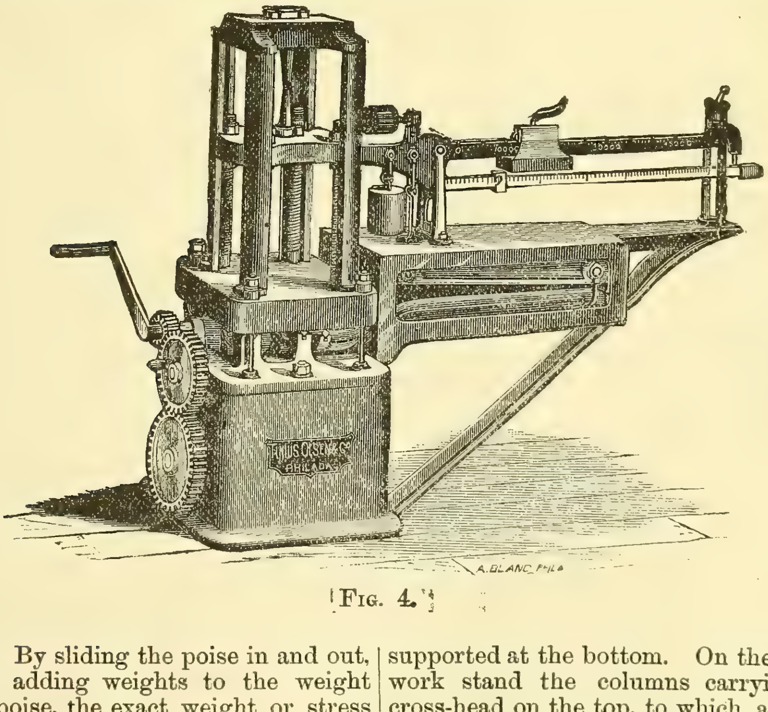


FIG. 4.

to rise. By sliding the poise in and out, and by adding weights to the weight counterpoise, the exact weight or stress applied may be readily ascertained.

In. Fig. 4 may be seen a very compact form of testing machine, built by Olsen & Co., of Philadelphia. Here one of the cross-heads of the machine, to which one end of the specimen is attached is supported on a large cast iron plate by means of four columns. This iron plate rests on the bearings of a large lever seen directly under the frame-work of the machine, which in turn transmits its stress to the beam at the right hand, where, in a similar manner to the previously described machine, the weight may be estimated by sliding the poises to and fro. Directly under the first mentioned cross-head may be seen a second cross-head, carried by means of four screws placed behind the columns supporting the upper head. By means of the crank and the gear at the left hand of the machine, this cross-head may be run up and down at pleasure, producing on the specimen the stress which is transmitted to the weighing apparatus.

Figure 5 gives a style of testing machine adopted by the Riehle Brothers, of Philadelphia. Here the frame-work of the machine carries a hydraulic ram supported at the bottom. On the framework stand the columns carrying the cross-head on the top, to which, as in the previous cases, one end of the specimen

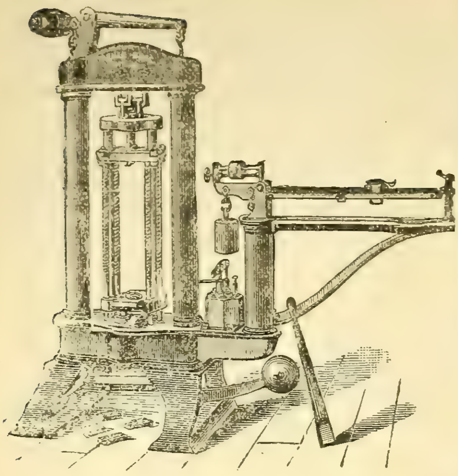


Fig. 5.

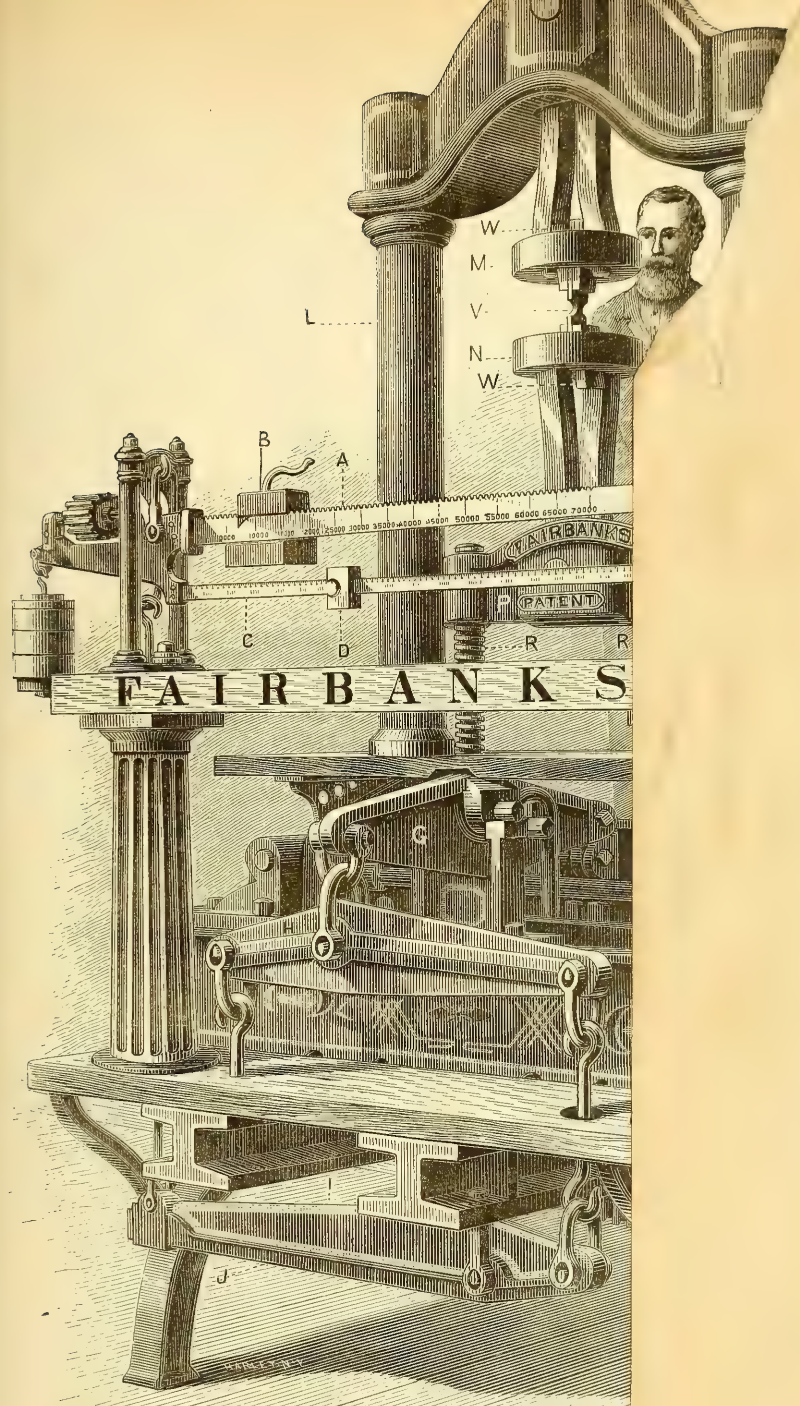
may be attached. Directly underneath the cross-head are four adjusting screws carrying a second cross-head. The object of these screws is simply to furnish a means of adjustability for raising the cross-head independent of the jack, so as to adapt the machine for a great variety of length of specimen. These screws are attached to a main lever concealed in the base of the machine which, as in the previous instance, also transmits its stress to the weighing beam at the right hand, where the stress, as previously, may be estimated by sliding the poises.

In Fig. 6 may be seen a typical illustration of the system employed by Messrs. Fairbanks & Co. A heavy frame-work of I-beams is arranged to carry the straining screws with their driving mechanism, and to support a scale system consisting of 7 levers. Four levers are arranged so as to support quite a large platform. From the ends of these 4 main levers, (technically so-called) the stress is conveyed to the central lever C, extending beneath the platform. From the end of this lever the stress passes to two multiplying levers, simply used for the purpose of reducing the amount of stress that is transmitted to the weighing beam. From the end of the last of these multiplying levers, the stress runs directly to the beam and may be estimated by the poises in the customary manner. On the top of the platform there stand two columns, supporting on their tops a large cross-head, to which one end of the specimen may be attached.

In the preceding machines it wlll be clearly noted that the straining mechanism has no connection at all with that part of the apparatus used for estimating the stress, except such as exists through the piece itself—the specimen forming the only connection between the weighing apparatus and that for producing the stress—consequently, it is obvious that only the stress which affects the piece can by any means (if the testing machine is properly built) produce an effect on the scale beam. Thus it will be seen that all co-efficients of friction, excepting such as may be involved in the levers and pivots themselves, is entirely eliminated from the testing machine, and only the stress used for rupturing the specimen is ever indicated.

The remaining class of testing machines, viz., those that estimate the force applied to the specimen by a hydraulic scale has but a single representative—that of the famous Emery testing machine.

Fig. 7 gives representation of the machine now at the Watertown Arsenal. Here it may be seen that at one end there is an exceedingly large and heavy-weighing mechanism, from whence proceed two screws extending nearly the whole length of the room and carrying on them the hydraulic straining jack with the jaws for holding the specimen. Essentially, the weighing mechanism consists of a large receptacle called a hydraulic support, covered with two thin flexible diaphragms. This receptacle is filled with a fluid made of a mixture of alcohol and glycerine, and connected to a similar receptacle of much smaller dimensions, placed under the weighing beam. According to the elementary principle of hydrostatics, the total pressure on a surface is proportional to the pressure per square inch and the area of the surface. If, consequently, it be supposed that in the main hydraulic support there is an area of one thousand square inches, and if in a similar miniature hydraulic support under the beam end, there is an area of only 1 square inch, a thousand pounds on the main support will only produce 1 lb. under the beam. It will thus be seen that by varying the relative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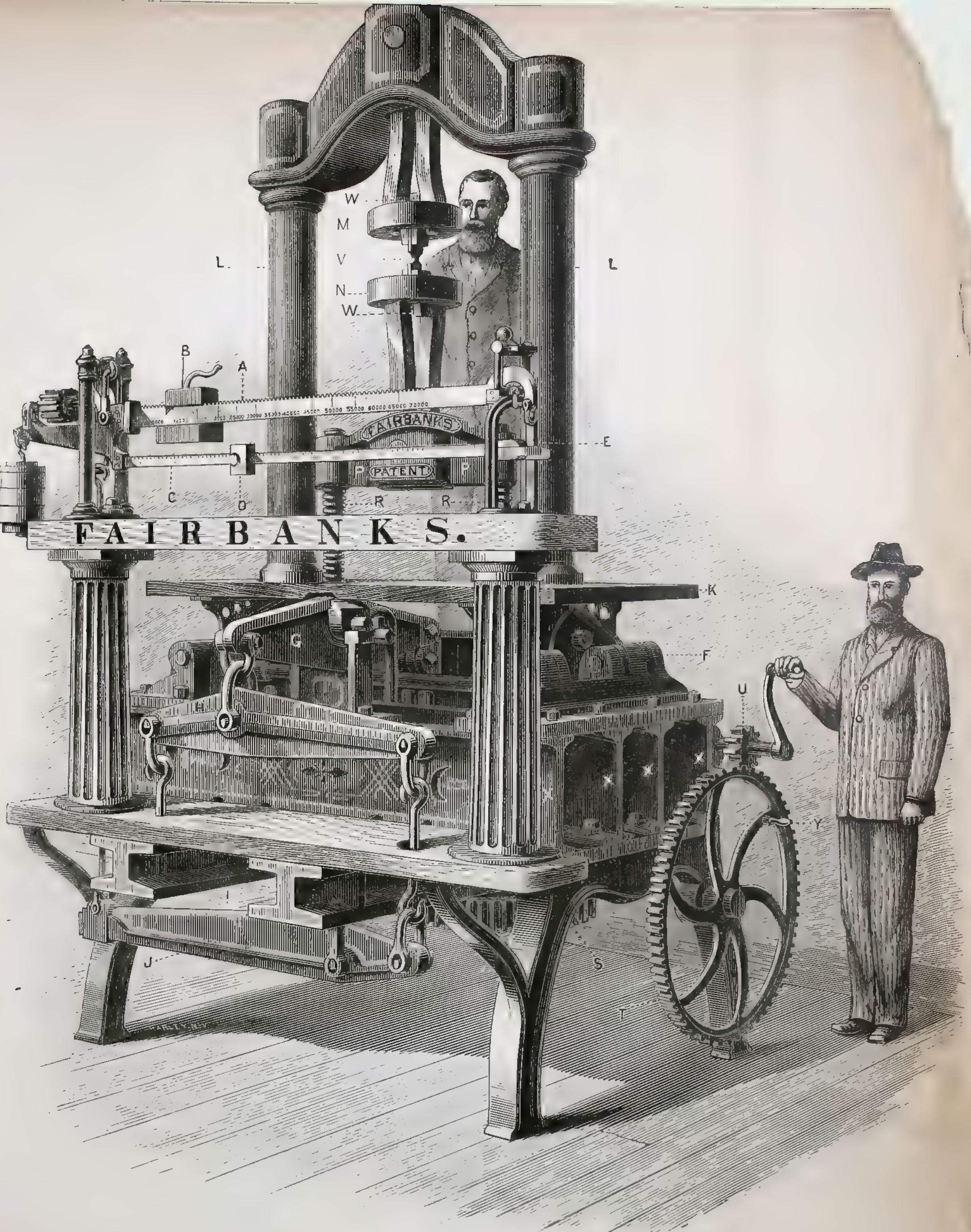

Fig. 6.

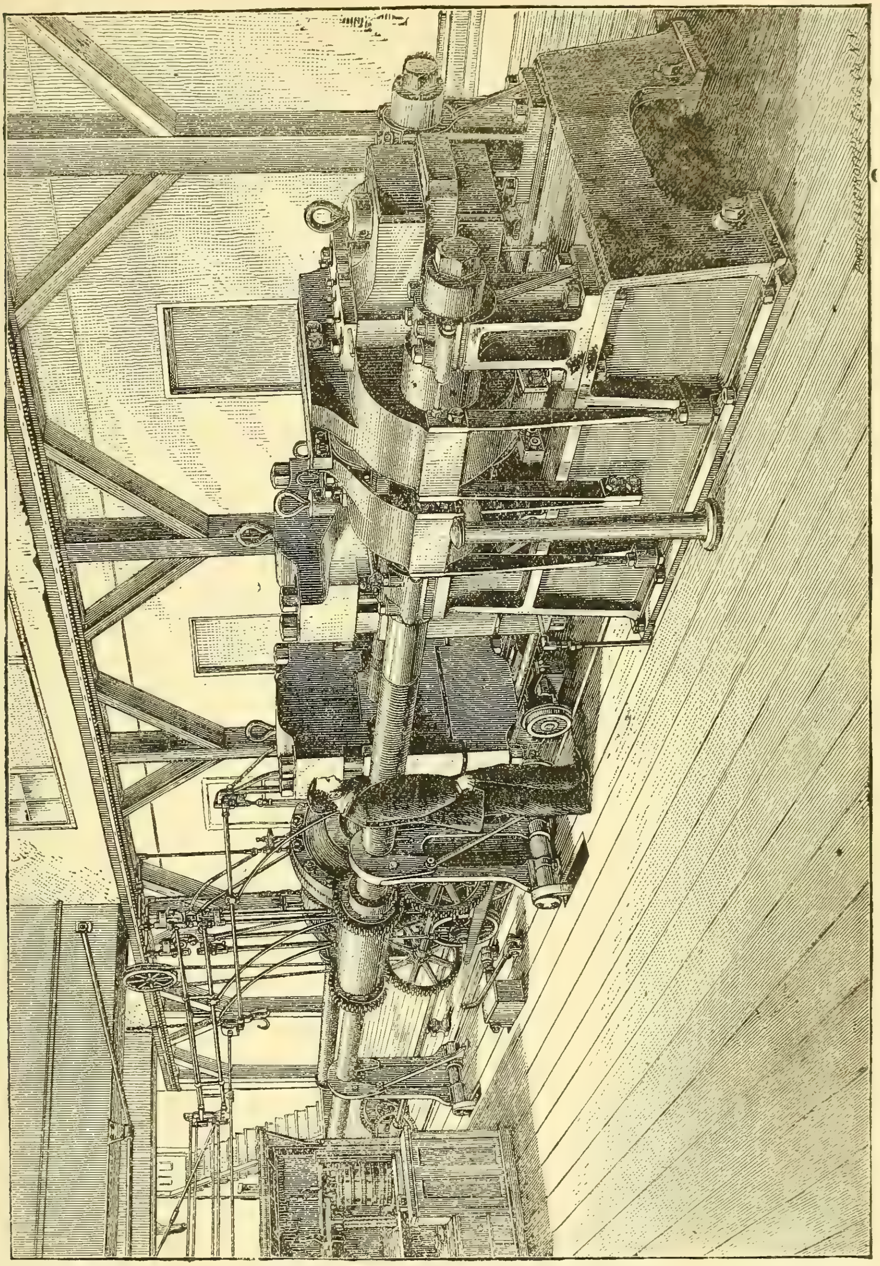

Fig. 7.

sizes of the two hydraulic supports any desired multiplication may be obtained under the beam. The weighing beam, instead of being supported on the customary knife edges, is hung by one or more metal strips. This arrangement will be seen to constitute an absolutely frictionless piece of mechanism, for the pressure coming on the large diaphgram is transmitted by the fluid column to the weighing beam, and simply operates to slightly bend the metal constituting the diaphragm and the suspending strips of the scale beam. Thus, in the true sense of

the word, the weighing is affected by no friction whatsoever, the only opposing force being the molecular resistance to flexure of the diaphragms and the suspending strips. The great merit of this ingenious device is its extreme sensibility, the enormous machine at the Watertown Arsenal being perfectly sensitive to the application of a few ounces of stress between the cross-heads.

ON SECONDARY BATTERIES AND THE ELECTRICAL STORAGE OF ENERGY.

By Professor OLIVER LODGE, M.A., D. Sc.

From the "Journal of the Society of Arts."

The ordinary methods of storing energy at present in use may be divided roughly into mechanical, chemical and electrical. As an example of the first kind, we may take the raising of weights, which is accomplished on a large scale hydraulically, and is applied to the working of cranes and dock-gates; another example is the coiling of springs, as in clocks and musical boxes. An example of a chemical store of energy is gunpowder; another is zinc or other smelted metal; and a third example is vegetation, whence arises all the energy of coal and gas engines. The charging of a Leyden jar, and the decomposition of dilute acid, furnish examples of electrical storage, and the explosive gases derived from the last-mentioned operation could doubtless be used to drive some form of gas-engine.

All these methods of storing energy involve some loss, and many of them are accompanied with a great deal of loss. The advantage of being able to store energy frequently outweighs the disadvantage of a considerable amount of loss of energy in the process; and there are three main cases in which this occurs.

One is when regularity and continuity of supply of energy is needed, and when the source of energy to be utilised is irregular and fitful. For instance, it would be convenient to be able to store wind or wave power, and utilise it in equable and dependable manner.

Another case is when occasional power is needed at times when the source of energy is not acting, or when a very great power is required for a short time, whereas the source of energy at your command has a steady but insufficient power. This would be exemplified in the utilisation of a small engine for lighting purposes.

The third case is when the power at your disposal is so great and so unutilised that the saving of even a fraction of it is a clear gain, no matter what becomes of the rest. This case frequently occurs when natural water-power is utilized. Processes of vegetation also utilize but a meagre proportion of the solar radiation, but the small proportion saved and stored up in the coal fields represents so much clear gain.

The mode of electric storage which has recently excited so much interest involves the use of a secondary battery, which may be defined as an instrument for receiving the energy of an electric current, and for giving it out again in the same form. This is a case of storage without transmutation, such as is also accomplished in the simple operation of winding a clock, where mechanical energy is taken in, to be subsequently given out in the same form. But in the electro-chemical decomposition of acid, the energy of an electric current is stored in such a way as to give sound, and heat, and light, and motion, but not an electric current under ordinary circumstances. There is one arrangement for making the evolved gases directly furnish an electric current, and that is a true secondary battery, viz., Grove's gas battery, or a voltameter.

A primary battery means one in which the plates have been made, or prepared and rendered active, by chemical means; a secondary battery means one in which the activity of the plates depend upon electrical energy having been first expended upon them. Any two conducting bodies, immersed in a conducting liquid

able to chemically attack one of them more than the other, constitutes a battery; and the difference in the attackability of the two bodies or "plates" may be caused either by making them originally of different materials, or else by modifying their surfaces in some way, permitting the main bulk of them to remain identical.

Most run-down batteries can be re-excited by forcing a reverse current through them, and they then become true secondary batteries.

The polarization of voltameter plates was studied by Gautherot, Ritter and Grove, in the earlier years of the century; and more recently and exhaustively by Planté, who found that plates of lead acted extremely well, and retained their energy for a long time.

The gas battery consists of two similar platinum plates, one soaked, or coated, or alloyed with hydrogen, the other with oxygen. But the quantity of oxygen which platinum can store up is very minute; the advantage of using a metal actually oxidizable is manifest. Platinum, and still more palladium, have considerable holding power for hydrogen, but by the use of a salt or an oxide as a recipient of the hydrogen, a far greater amount of reversible action can be stored up than by attempting to store up the hydrogen itself by any power of occlusion.

A Planté cell, from the beginning, furnishes an oxidizable plate on the positive side, but its negative plate has at first no practical occluding power for hydrogen.

Reversing a once charged Plante cell, however, we get metallic lead for the oxidizable plate, and an already oxidized, and therefore reducible, plate to receive the hydrogen; the oxide gradually being reduced to metallic lead, of a porous or spongy consistency. By many reversals of this kind, Planté "formed" his cells, until the lead plates were deeply acted on, and a considerable storage capacity obtained.

The modification of Faure consisted in providing both an oxidizable and a reducible porous material from the very first, by coating the plates with minium or litharge. The plates could then be peroxidized, and reduced respectively to a considerable depth, without any tedious process of "formation." A considerable storage capacity was thus obtained, but difficulties presented themselves connected with the adhesion of the coatings to the plates, and these were not got over until perforated plates were employed to hold the applied substances. Perforated plates, by rendering unnecessary any jacket of cloth or felt to keep the coating on, and by thus permitting a free circulation of the liquid, no doubt are a considerable improvement; and cells, as now made, often work extremely well; at the same time, a layer of uniform thickness spread all over the plate, and not locally distributed in a non-homogeneous manner, would seem theoretically to be preferable, if the practical difficulties connected with such a uniformly spread layer had not seemed too great. Plates made in the old manner did not safely bear transport and rough usage, and a film of insulating sulphate of lead was apt sometimes to creep between the metallic lead and the peroxide coating, and to destroy all electrical connection between them over large patches.

The process of manufacture, as now carried on, is here illustrated by a number of specimens of plates in different stages, kindly lent for the purpose by the Electrical Power Company, of Millwall.

The first operation is the casting of the lead grid, the holes in the plate being from $\frac{1}{4}$ in. to $\frac{3}{8}$ in. square. The positive grid is made a little thicker than that which is to be the negative, for reasons which will shortly appear. The thickness of the grids at present most employed are, positive, $\frac{1}{8}$ in. to $\frac{1}{6}$ in.; negative, $\frac{1}{12}$ in. to $\frac{1}{8}$ in., and they are made in two sizes, one $7\frac{1}{4}$ in. by $5\frac{1}{4}$ in., the other 9 in. by 10 in.; the former being used in the small or "half horse-power" boxes, the larger plate being used in all other sizes, it being found more convenient to vary the number of plates in a cell than to have in stock a number of different sized plates. The weights of the formed plates are, small size negative, 15 oz.; small size positive, 21 oz.; large size negative, 50 oz.; large size positive, 70 oz., about.

The next operation is the filling of the grids with a paste of oxide of lead mixed with dilute sulphuric acid. The negative plates are filled with litharge, PbO, the positive with minium, Pb_3O_4, since there is an evident economy in having to reduce only the lower oxide, and to peroxidize only the higher one.

The plates are then taken to be "form-

ed" by the electric current, for which purpose they are packed alternately in square stoneware jars filled up with dilute sulphuric acid, and a current of about two or three amperes per pair of plates supplied. At the first instant of immersion in acid, a darkening of the minium occurred, owing to a partial formation of peroxide in it by the action of the acid; and this darkening continues, under the influence of the nascent oxygen liberated by the current, until the whole of the composition is converted into peroxide. The litharge is, on the other hand, gradually reduced to metallic lead by the action of the nascent hydrogen produced by the current, the reduced lead having a slightly porous or spongy consistency.

No gas is evolved from the cells for some time; it is all consumed in effecting these chemical changes. A few minute oxygen bubbles do, however, make their appearance on the positive plate, much of it being in the form of ozone. Ultimately, as the surface of the positive plate gets peroxidised over, a good deal of oxygen escapes, and it is only after a long-continued current that the portions of minium in the interior get acted upon. In time all the minium is attacked, however deeply buried; the reason being that less E.M.F. is needed to send the current to an oxidizable substance than to saturated peroxide. The reduction of the litharge is a somewhat slower process, but the plates being thinner, it is accomplished in approximately the same time, which may be taken as between three days and a week. When the process is approaching completion, torrents of both kinds of gas rise from their respective plates. The plates are then removed from the forming pot, rinsed plentifully with water to remove the excess of acid, and then stacked till wanted. Unless the rinsing operation is carried out, the spongy lead plates are found to heat considerably, by reason of the acid in their pores combining with the lead.

In order to keep the plates from touching each other when subsequently used in battery cells, various devices have been tried. The one now most in favor is to punch out the composition from a tolerable number of the holes of each negative plate, and to insert instead a block of vulcanised India-rubber in each. These plugs project nearly a quarter of an inch on either side the face of the plate, and effectually prevent the adjacent positive from coming into contact with it.

Another matter which has given some trouble to determine is the best method of connecting the plates together. All the positive and all the negative plates in a cell have to be electrically connected in the most complete and permanent manner. Clips and clamps of various kinds have been tried; but, to guard against all possibility of corroded surfaces and imperfect connection, it is now thought wise always to use solder, whether in combination with screw clamps, or otherwise, and to protect the joint from the action of acid spray by a varnish, or by a mass of paraffin. It is curious to notice how persistently the junction of the + plates tend to corrode, showing how the oxidizing action creeps up liquid films on the lugs.

When the plates are to be used, they are packed alternately in glass or ebonite boxes, the India-rubber plugs keeping them apart, and wooden supports keeping them off the bottom of the cell. Their lugs are connected together, and the box is filled with dilute sulphuric acid, say, of specific gravity 1.100 or 1.150. A charging current is then supplied, so as to freshen up the plates, and they are then discharged, and charged up again once or twice, to bring them into good condition. It is not a good plan to discharge them too completely, nor to let them remain idle for very long together; neither should they be charged or discharged too rapidly, so as to heat them above a gentle warmth. The more regularly they are worked—as for instance, when they receive a charge every day and discharge every evening—the better they behave. Regularly and carefully worked cells will keep in good order for a very long time, but if they remain idle for weeks together, some of the plates are apt to warp, and otherwise give trouble and require attention. They must not be overcharged very excessively either. When full, they indicate the fact by evolving a good deal of gas, and it is best not to continue the charging power much longer when this stage is fully reached, partly because nearly all the charge then put in is wasted, but principally because a little of the oxygen is absorbed by the lead of the grid, and it therefore gets slowly peroxidized,

so that, after a sufficient lapse of time, it may get actually eaten through, and the plate become friable. When a plate has got into this state, the only plan seems to be to melt it down with a flux, and recover the lead. But the complete peroxidation of a plate by overcharging is a work of time, by far the greater quantity of gas being harmlessly, though wastefully, set free; and it is not necessary to be alarmed at a slight evolution of gas which usually goes on from the positive plate when being charged, more especially as the cell is getting full. Alarm at the appearance of bubbles may lead to the cell never being properly filled up, in which case the battery will fail to work satisfactorily.

The number of plates which are packed into each box depends entirely upon the size of the box, and it is in this way that the different sized cells are constructed, not by varying the size of the plates; except that the smallest cells are made with half-sized plates.

Taking the full-sized plates, we may reckon that each pair is able easily to receive and give out electricity at the rate of four amperes per day. This, of course, reckons both faces of each plate as active. This process may be continued for say 8 hours on the average, consequently the storage capacity of a cell is 32 ampere hours per pair of plates. The E. M. F. of discharge is as nearly as possible 2 volts; consequently the power of a cell is 8 watts per pair of plates; and the total available energy per pair of plates, when full, is 8 watts for 8 hours, or 170,000 foot-pounds; which may be also, though clumsily, expressed as 1-12th of a horse-power-hour per pair of plates. Consequently, a box which is to give 1 horse-power-hour should contain 12 pair of plates; a 2 horse-power box 24 pair, and so on. But each box will not really give a horse-power for an hour, it will rather give an eighth of an horse-power for 8 hours. If a horse-power is wanted, all you need do is to take 8 cells, and then they will give you a horse-power and will last 8 hours; 16 cells will give 2 horse-power for the same time, and so on. By drawing off the power more slowly, the discharge will last longer, and it is more economical thus to work, except as regards first cost of batteries. This latter item it is which usually causes them to be called upon to exert power up to their maximum reasonable capacity. It is true that a cell will give a much higher power for a short time if called upon—thus a single cell might possibly be made to give a horse-power, but then it would not last out its proportionate time. I have found some cells, which I discharged at a very moderate rate, continue discharging an extraordinary time, and give out sometimes even more than was put in, which, of course, means that the effect of previous charges was being drawn upon.

When a cell which is being rapidly discharged has apparently run completely down, a few minutes rest will suffice to recover it a little; and after an hour or two's rest a cell will commonly have recovered sufficiently to enable it to discharge with something of its old vigour for a period comparable with that of the rest. This recuperative power of the cells is in some respects advantageous, but, on the whole, it would be better if they gave out the whole of their energy continuously without needing intervals for refreshment. The more slowly a cell is discharged, the less noticeable is this temporary falling off and subsequent recovery.

When a cell has run down, it is interesting to examine which of its three constituents is the one that has failed; the positive plate, the negative plate, or the acid. Sometimes, especially in cells too crowded with plates, there is not enough acid present; the free acid is absorbed by the lead, and only water is left. The specific gravity of the acid may, however, be easily watched by floating a little hydrometer in the cell. More frequently the negative plate is the first to fall off; as may be shown by removing it and inserting a fresh one, when the cell will go on again. The cause of this, and of several other peculiarities observed in batteries, may be readily made out by experiments on a small scale, on two little plain lead strips, held in a convenient clamp, and immersed in dilute acid.

To a pair of such strips of sheet lead supply a current, from a couple of Groves, say, for half an hour or so. The positive plate is seen to become coated with a pure layer which gradually darkens to black. The other plate seems unchanged. Gas

is evoked from both plates. On now connecting them with an electric bell, it will ring for a minute or more. Now recharge, but this time, instead of discharging the cell, remove the negative plate, and insert instead a bit of clean lead, the same in appearance as the one against which hydrogen has been so long liberated, and then discharge the cell; it will only ring the bell for about two seconds. Depress the plain lead plate further into the acid, so as to expose a fresh surface, and the bell will give a few more strokes. Remove this plate altogether, and re-insert the hydrogenized plate which had been used for charging the cell, and the bell rings vigorously for as long a time as it did in the first experiment, showing that the peroxide plate is perfectly vigorous, and showing also that the nascent hydrogen has modified the lead in some way or other.

The natural supposition is that the lead has occluded hydrogen, but Dr. Frankland has shown that, if such hydrogenized lead be melted down, it gives off no hydrogen worth mentioning. It is not easy to see wherein the change consists. Under the microscope, the hydrogenized plate looks a little darker than the plain plate, but no real porosity is noticeable. The fact, however, is certain, that it behaves in a very different manner to plain lead, and that it retains this singular power long after it has been removed from the liquid and rinsed.

To avoid misunderstanding, it might have been well to state earlier that I use the term "positive plate" to signify that at which the charging current enters, and from which the discharge current leaves. It is that one whose terminal is colored red in the cells of commerce. It is that which must be joined to the + pole of a magneto-machine, whether the latter is to act as a generator or a motor. I make no distinction between positive plate and positive pole, because all such distinctions end in confusion.

The plain lead plate which refused to give a current when opposed to peroxide, did not fail because of its high position in the electro-chemical series, as may be proved by opposing it to the hydrogenized plate, when it is equally inert; it fails simply and solely on account of a film of some non-conducting substance which spreads over its surface the instant it is used as one electrode of a battery, the substance appearing to be sulphate of lead. The internal resistance of the cell thus becomes enormous, and the current ceases. On standing idle for a time, the film has a chance of clearing off—though from a plain lead plate I do not notice much tendency to clear—or it may be wiped off with a cloth at once; but a fresh film speedily forms. If amalgamated lead be used, the white scum can be seen, and it sometimes drops off in a filmy sheet. One remarkable way of instantly clearing off the scum is to touch the lead under the liquid with a scrap of amalgamated zinc. The effect is magical: the scum vanishes, and the bell again rings for several seconds after the zinc has been removed. So long as the zinc is present the current will continue, until indeed the positive plate becomes exhausted.

To examine whether the positive plate also exhibits the temporary falling-off, and to eliminate the concurrent action of the negative, the best plan is to use a piece of amalgamated zinc for the latter; for since sulphate of zinc is soluble, it remains perfectly clear, and all variation is traceable at once to the behavior of the positive. Under these circumstances, the cell, if rapidly discharged, does show a slight recuperative power, recovering somewhat on standing. So far as I can make out, this appears to be mainly due to a temporary exhaustion of the acid close to the surface, and in the pores of the peroxide, so that the power falls until, by rapid diffusion, the absolved acid has been replenished. The insulating scum of sulphate on the negative plate is, however, by far the most powerful cause of the ordinary running down of a cell.

When we come to try the hydrogenized plate employed in charging the cell instead of the plain plate, we find that it also ceases to act before the positive is exhausted, though it is able to go on for a considerable time; it ultimately acquires a scum. On standing, the scum clears off partially, and the cell recovers. A piece of zinc made to touch the plate under the liquid clears it, and in a short time restores it to its original power, having precisely the same effect as a re-application of the charging current. The fact is not surprising, since the zinc naturally causes nascent hydrogen to be

liberated on the plate, just the same as a current would; but the quick and prompt action of the merest scrap of zinc is somewhat striking, and some practical application of the fact would seem to be possible.

Instead of a plain lead plate, lead sponge may be employed for the negative plate, such as is made by Watt, of Liverpool, by blowing high pressure steam through a jet of molten lead, or such as Mr. Desmond FitzGerald has brought me here, made in some other way. The enormous surface of this spongy lead enables it to go on acting for a much longer time than plain lead, without being stopped appreciably by a scum. This effect is due to the extent of surface, and it appears to be owing to a sort of potential hydrogenation caused by the ability of acid to attack spongy lead, or any metal in a fine state of division, more easily than when massive; and the sulphate of lead formed, though it certainly clogs up the pores, is not readily able to form a continuous protecting film.

A thin coat of peroxide formed on a lead plate rapidly becomes lighter in color when the current is stopped, being evidently reduced to a lower oxide by local action with the lead underneath. When the coat is very thin, the changes go on so rapidly that, by quickly pressing the charging battery key down and up, flashes of peroxide appear and disappear on the surface. It is by this local action effect, no doubt, that peroxidation eats deeper and deeper into metallic lead, for otherwise, as soon as a complete layer of peroxide was once formed, all subsequent gas would be given off from its surface (it being a conductor), and the lead beneath would be protected.

The local action proclivity between lead and peroxide has two unpleasant consequences, however; one is that the peroxide gradually disappears on long standing, and the cell thus loses its charge; the other is that the metallic lead of the plate gets gradually eaten into, and corroded away. These two results are modified in practice by the fact that, whereas local action destroys the peroxidation of thin films in an instant, in the deeper layers of thick films it goes on very slowly, partly because fresh acid cannot so easily penetrate, but principally because the sulphate and lower oxides formed, being non-conductors, exert a clogging and protective action.

Instead of using a lead negative plate, a copper-plate may be used, and will give a strong and lasting current. It lasts, of course, because copper sulphate is soluble, and no insulating scum is formed; but that this Sutton cell has a fairly high E. M. F., is a striking proof of the value of peroxide of lead as the electro-negative element of a battery.

Even platinum gives a current when opposed to peroxide of lead, though of course it is a feeble one. But if zinc is used, we have the strongest practicable battery I know, each cell having an E. M. F., of $2\frac{1}{2}$ volts.

I must now say a few words about the methods and instruments for practically measuring the various quantities involved in the storage of energy by secondary batteries. We need current or ampere-meters, potential or volt-meters, quantity or coulomb-meters, and energy or erg-meters. The well-known dead-beat ammeters and volt-meters of Professors Ayrton and Perry are exceedingly convenient instruments, especially the former; the only objection to them is the frequent calibration they require, because their permanent magnet changes in strength. When portability is not essential, I find a high-resistance galvanometer, of the form known on the continent as the "Wiedemann," a most accurate and convenient volt-meter. The needle has a thick copper damper, which makes it almost dead-beat, and a resistance of 30,000 ohms or so has to be added to the circuit, in order to bring the deflection given by one cell within moderate range. It thus taps off an absolutely infinitesimal fraction of the current.

The deflections are immediately interpreted by observing, at the same time, the reading produced by a couple of common Daniell cells in series, each of these being assumed to be 1.12 volts. There is a trifle of uncertainty about the absolute E. M. F. of even a freshly set up Daniell, and this uncertainty will continue until an authoritative standard of E. M. F. is issued by the British Association Standards Committee; but variations in E. M. F., one-thousandth part of the whole, can be readily detected; and a sensitive volt-meter is somewhat important, if fouling cells are to be detected in their

early stages, and before the cell is seriously injured.

As a meter for the quantity of electricity supplied or withdrawn to the cells, I use a large copper drum, suspended in sulphate of copper on the end of a balance arm, in such a way that it can be weighed at any moment without withdrawing it from the solution. Contact is easily made with it by means of mercury, and it is surrounded with a larger cylinder of copper, to act as the other pole. Allowing for the buoyancy of the solution in which it is weighed, every gramme increase or diminution of the drum means 1.04 ampere-hours put into or taken out of the cells. Frequent weighings of the drum enable one to get a very good idea of the cells as regards charge, and they keep an accurate record of the loss, or discrepancy between the charge and discharge quantity.

A copper-drum coulomb-meter may also be made floating, so as to indicate by its height the quantity of electricity which has passed through. If such a vertical-moving quantity indicator carries a sheet of sensitised paper, on which the horizontal spot of light from a volt-meter records a trace, the arrangement will give an indicator diagram of the charge and discharge precisely analogous to the indicator diagram for the steam engine, except that the area enclosed by the curve represents the loss of work instead of the balance of useful work. The useful work is indicated by the area between the discharge curve and the two straight lines of reference.

Many plans can be devised for combining the two traces, the quantity and the E. M. F., at right angles to one another, and the continued use of some such instrument would, I believe, assist in detecting the peculiarity of cells, and in improving their manufacture and system of treatment, almost to the same extent as the indicator throws light on the peculiarties of different steam engines. But even without such an instrument for automatically constructing the diagrams, a curve plotted in this way (with quantity for abscissa and E. M. F. for ordinate) is the readiest mode of exhibiting the results of measurement taken with ordinary meters, and making the value of the cell manifest at a glance.

A highly ingenious quantity-meter has been devised by Dr. Hopkinson, and since it indicates precisely like a gas-meter, it is easily intelligible, and may come into general use. But at the present, I believe, it only works one way, hence it is not so directly applicable to secondary batteries; it would add up both charge and discharge in one continuous record, drawing no distinction between them. The simplest power or watt-meter is the Siemens dynamometer, arranged with one series and one shunt coil; and it indicates at any instant the power which is being supplied or withdrawn from the cells.

An erg-meter, or energy integrator, can be made in various forms. The one I have here to show is Ayrton and Perry's modification of an eight-day clock. They substitute a fine coil of wire for the bob of the pendulum, and surround this with another larger and thick coil. A current sent in the same direction round two nearly concentric coils, causes the smaller one to move to the centre of the large one. Thus, if the pendulum is oscillating, and the clock keeping good time, a current sent round the two coils will cause the clock either to gain or lose, according as the currents are opposed or agree in direction. If the two coils are both in series in the circuit, the gain or loss of the clock will indicate the coulombs passed through. But if the small fine coil is a shunt, while the other is a series, the force acting on the bob will be a function of the power, and the gain or loss of the clock will indicate the energy in some more or less arbitrary way, which can be made simple proportion by empirically adjusting the relations among the parts.

To determine the internal resistance of a battery cell, or of a whole battery, the plan is to measure its electromotive force when driving two different strengths of current, as different as possible and both known. One strength of current is most conveniently zero; the other must be measured, say, by an am-meter in the circuit. A volt-meter, such as the high resistance Wiedemann, is connected to the terminals of the battery and read. Its indication will be a scrap higher if the current was being supplied by the cells, but lower if the current was being supplied to them. The difference of the indication of the volt-meter, when a current is passing and when no current is passing, is proportioned to the resistance

of the battery, and for a secondary battery cell in good order, the difference is surprisingly small, so much so that an ordinary volt-meter will often fail to detect it. To get the resistance of the battery in ohms, you must divide the difference in the readings of the volt-meter (interpreted into volts) by the strength of the current passing in the main circuit, reckoned in amperes. A moment's interruption is sufficient with a good dead-beat volt-meter, and the current may be immediately switched on again before the disturbance in the circuit has had time to show itself, except indeed where the current is being used for lighting purposes, in which case no tricks may be played unless there are several batteries in multiple arc, and one of them can be disconnected without detriment. This, indeed, is a good plan of connecting batteries, as it allows measurements, examinations and repairs to go on while the current is being used. The resistance of one of the small sized (half horse power hour) cells with nine pair of plates averages .002 ohm. Large cells have a still lower resistance, of course, and it is to this extremely low internal resistance that the secondary battery owes much of its great practical value. The current from an ordinary Grove's battery is by no means proportionate to the external resistance of the circuit unless this is high; in other words, the driving power of a cell, or the difference of potential between its terminals, falls off greatly when strong currents are demanded. But a secondary battery, from its enormous area of surface, not only gives a higher E. M. F. than a Grove ever does, but it keeps this two-volts difference of potential between its terminals for pretty nearly any reasonable strength of current. Hence the current does within wide limits vary with the external resistance, and the current which such a cell will drive through a piece of thick copper wire is simply tremendous.

I must now say a few words about the uses to which secondary batteries can be applied; and, first of all, for general laboratory purposes they are invaluable. A small gas engine and shunt dynamo are necessary to charge them up occasionally, but they then retain their charge for days or weeks, according to the demand made upon them, and they are ready at any moment to produce a current, strong or weak, according to wish. They thus save a deal of trouble in setting up Grove batteries, and, moreover, the current they give is much steadier, for unless they are run too near their falling-off point, their E. M. F. continues very constant, and no compunction is felt at leaving them connected up to the circuit for a long time together—for such a time as would make Grove cells heat violently and boil over. When they are run down, a few days' charging replenishes them again.

As regards durability, the cells repay cleanliness and attention. I mount them in front of a window, on a slab of thick glass raised above the bench; one can then see all over and through them, the light being reflected from below the glass slab by an inclined bit of looking-glass which can be moved about. It ought also to be possible to get on both sides of the bench, so as to be able to detect and remove any attempt at contact between the plates of a cell.

If a lump of composition tumble out and bridges across from one plate to the next, it can be removed with a paper-knife. Plenty of light, easy access, and clear vision through between the plates of each cell are highly desirable. The glass cells and the slab should also be kept clean, and free from acid splashes and spray, for all this moisture promotes leakage, and causes the cells to lose their charge more rapidly than they would by ordinary and unavoidable local action.

When cells are stowed away in dirty dark cellars, on wooden shelves, like wine bottles, the leakage must often be considerable, and the cells may get into any state of dilapidation without detection or attempt at remedy. A light attic or glass house, with crockery slabs at a convenient height, is a much more suitable place for secondary batteries than a wine cellar.

It is important to be able to detect an incipient flaw in a cell before it has had time to damage the cell perceptibly. A sensitive volt-meter may do this, but an ordinary one will not show any difference for a long time, and if the cell is in series with others, as soon as it begins to run down in E. M. F., the others rush it down to and past zero, and begin to charge it up wrong way. This reversal is very bad for a cell, besides its very weakening effect upon the main current.

It is very important to keep all the cells of a series as exactly alike as possible. A bad short circuit often causes a hissing noise which can be heard with a stethoscope applied to the cell, even when it is not loud enough to call your attention, as it sometimes is. But the most delicate detector of an incipient short circuit is the thermometer. All the cells heat by their proper current, but any abnormal action going on in a cell, whether it be short current or local action, or any other dissipation of energy, must result in heat, and the thermometer is the natural detector of this. Any cell warmer than the others should be examined, and set right or else replaced.

The lugs and contacts should be wiped clean occasionally from the acid spray which collects on them, and they may also be recoated with paraffin and shellac. This acid spray is thrown up by the bubbles of gas which rise when the cell is getting full. The hydrogen bubbles throw up largish globules, which bound two or three inches into the air, and fall all over the slab and terminals; but when hydrogen comes off freely, the cell is commonly quite full. The oxygen which comes off slightly during the greater part of the charging throws up much more minute globules, which float about as an acid spray, very irritating to the nostrils and very provocative of leakage. The spray may be very greatly diminished by a layer of oil spread over the surface, in which the bubbles collect and break in a different way, and it also checks evaporation; but the oil introduces difficulties of its own when the plates have to be lifted out of the cell, though indeed it may be first flooded off. The contacts of the positive plates are very apt to be corroded by the spray and creeping film, together with the per-oxidizing action of the current crawling up wet lugs, and they require occasional attention. Unless absolute uniformity of contact is secured, some of the plates will take nearly all the current, and get peroxidized to pieces, while the others are nearly idle.

I use pans of very dilute ammonia, or perforated boxes of carbonate, to neutralize the acid spray. The atmosphere is then much improved, and instead of acid films, white crusts of ammonia sulphate collect, which are less harmful and may be cleared off. Another precaution is always to keep the cells filled with acid above the level of the plates, so that they are completely immersed, else the part above the liquid will be exposed to the influence of non-concentrated acid, from the evaporation of the water out of the acid film covering it. It is important to remember that all these exposed layers of acid are likely to become pretty concentrated, and hence, that they may be expected to corrode more rapidly than the mass of the solution.

Another advisable precaution is to stir or agitate the liquid occasionally, more particularly during charging. The bubbles of gas, fortunately, do this to some extent, if the cells are well filled up electrically; but if no such agitation is excited, the acid at the bottom of the cell is apt to become very much more concentrated than that at the top. Not that gravity is able to pull down strong acid out of once thoroughly mixed dilute acid. Gravity cannot undo diffusion like this. But it is the acid freshly formed during re-charging, by decomposition of the sulphate of lead on both plates—it is this acid which falls to the bottom and accumulates there, frequently corroding the bottoms of the plates. One great advantage of the present cells over the old flannel-swathed Faure, is that they permit free circulation of the liquid. To make the cells answer various laboratory requirements, I have a sort of resistance-box, made with uncovered spirals of German silver wire, in a stoneware water trough, so that the wires do not melt, and I can then throw in any resistance from a quarter ohm upwards, otherwise the current would be frequently too strong. For working a Serrin lamp no extra resistance is necessary, and from eighteen to twenty-two cells work it admirably. Even when one drives the lamp direct from the dynamo, the battery is still connected as a shunt circuit, to act as a regulator and steady the current. Moreover, it has the great advantage of preventing the engine from racing away when one puts out the lamp. A well-governed engine, of course, ought not to do this in any case, but I have not found a gas-engine behave well, when work is thrown on and off it in such an irregular and fitful manner as a lamp is commonly used in a lecture. It is much better for it to be pumping into the cells

whenever the lamp is put out. Moreover, one can get a far more powerful current through the lamp in this way than from a small dynamo direct. Another advantage of having the battery connected is, that it will not permit the locking of the carbons, and thus the short circuiting of the field magnets of the shunt dynamo, which is a not infrequent occurrence when a shunt dynamo alone is used to feed an arc lamp.

If, however, only a regulator is wanted, and not a store of energy, a set of zinc and lead plates can be easily arranged, which will do all that is wanted in the way of regulation; and as the lead gets gradually more and more "formed," the battery will gradually acquire more and more actual storage capacity. The cells were very useful once when the gas-engine was being repaired. I took the dynamo over to a neighbouring steam-engine, and laid its comparatively full current with spans of common wire to the battery. From the battery thus charged, the powerful occasional currents needed for some magnetic researches then going on were readily obtained. To bring such currents from the distant dynamo would have necessitated the use of very thick and expensive cable, and would in fact have seriously damaged its armature.

The battery thus acted as a transmuter of weak currents into strong ones, and this illustrates on a small scale one of its very important future functions, viz., a receiver of energy transmitted from a distance, as Sir Wm. Thomson has pointed out. Currents generated at a distant station, to be conveyed to a distance, ought not to be strong; they ought to be feeble currents at a high potential. They can be received by a great number of batteries in series, and the batteries so charged can be used for any local purposes, and made to give any reasonable desired current by altering their connections. Moreover, they can be coupled up to a number of independent circuits, so that irregularities in one shall not interfere with the others. The great advantage of a set of batteries as a receiver, over a set of dynamos in series, is that the latter would need such a high internal resistance to give the necessary back E. M. F., whereas a battery, even of small cells, can give 1,000 volts E.M.F., with an internal resistance of only 2 ohms. Hence the waste in heating the receiver is next to nothing. There is another kind of waste, no doubt, for the work got out of a battery can never be anything like that put in. The E.M.F. of charge is about 2.3 or even 2.4 volts, while that of discharge is scarcely above 2; besides this, there is a slight loss or discrepancy between the quantity supplied and that afterwards withdrawn. Notwithstanding this waste, they must come into use when made in a satisfactory and quite dependable manner, for their great convenience and steadiness. And when the power at the distant station is water or wind-power, as I doubt not it often will be, waste becomes of secondary importance, and a convenient method of storing is the first consideration.

So plainly is high E.M.F. pointed at as the requisite for the economical transmission of energy, that I cannot help thinking that, in some cases, a use will be found for electrostatic machines as generators instead of dynamos. Not exactly a Holtz machine—which has a high internal resistance, and must dissipate energy, from its use of non-conductors and sparking intervals—but some gigantic form of Thomson's replenisher with metallic contacts. A large machine is needed to give a current of one ampère, but not outrageously large, and a clear separation of the parts by a millimetre will enable 4,000 volts E.M.F. to be obtained. No great horse-power is here represented, but it would be transmitted by a well insulated wire with very small loss.

In using a waterfall running continuously to light a town for, say, eight hours, batteries are evidently desirable. Three sets might be used, each charging for eight hours—one or other of them always charging, and two of them discharging during the eight hours the light is required. Or two sets would be sufficient if the water-power itself could be used for lighting direct. But the use of three sets of batteries would enable a reasonably distant waterfall to be used, and would, for the eight hours the light is needed, give an actually greater horse-power than the fall itself possesses.

For locomotion in cases where the current cannot easily be supplied to the moving carriage by metallic conduction,

as in tram-cars on common streets, batteries carried by the tram-car afford an obvious, and, moreover, an economical form of motive power. It is sincerely to be hoped that electric tram-cars may speedily come to perfection, and that the excessive wear and tear of horseflesh in this monotonous and severe labor may be checked.

[A number of motors were exhibited, some lent by the Electrical Power Storage Company, and some by Professors Ayrton and Perry. A De-Meritens motor, driven by 18 storage cells, was used to drive a small Siemens' dynamo by a strap, the current from this being used to drive another motor, which worked a lathe, or was used to light four small Swan lamps. A Pilsen arc-lamp was shown, working very steadily with the current from the battery, and a number of Swan lamps were also excited, so as to show how entirely any one was unaffected by the turning out of the others, a battery behaving in this respect like a very good compound wound dynamo. Diagrams of the mode of connecting up cells and dynamos for transmission of power and curves representing the charge and discharge of cells, as tested by Dr. Hopkinson and Messrs. Ayrton and Perry, were also exhibited, but it is thought unnecessary to reproduce them here.]

For driving boats electrically, secondary batteries are obviously necessary, because the laying on of a current by a wire is not only inconvenient but impossible. The storage cells will act as ballast, and can be charged while the boat is moored. For swift short voyages, and for pleasure vessels, nothing better than electrical propulsion is likely to be devised. For long voyages and big ships, the weight of secondary batteries would need all the floating power the ship possesses, and perhaps more. The energy consumed in propelling the present Atlantic liners is something almost incredible.

When motors are driven by secondary batteries with very short connecting wire, the question arises, is high E.M.F. still desirable, as it is when transmission across great distances is concerned, or may powerful currents be now used with advantage? The answer is that, neglecting the resistance of battery and connecting wires, there is neither gain nor loss in using powerful currents, provided the dynamo is properly wound, and has brushes that will not burn up.

But if we do not neglect the resistance of the batteries—a thing we have no right whatever to do—the answer is, that high E.M.F. is of very great advantage. A large number of cells in series, driving a high E.M.F. dynamo at its highest possible speed, and with as small a current as is needed for the power, is by far the most economical arrangement. Now, a screw for a ship has always needed a high speed for economy, and in the old days gearing used to be employed between the steam-engine and the screw, because no high-speed engine was sufficiently economical. I understand that slow engines are still the best, but the disadvantages of gearing are so great that high-speed direct-acting engines are always used. But a high speed is absolutely essential to economy in an electric motor, and no screw, as ordinarily made, would work at such a speed; consequently there is a temptation to use gearing the other way, and to gear down the dynamo to suit the screw. I hope the attempt will be resisted, and high-speed screws made of small size, smooth surfaces, and low pitch, so that the electric motor axle may be coupled direct to the screw shaft. The direct rotatory action of a dynamo used as a motor, as opposed to the oscillating action of a steam-engine, is a great simplifier of mechanism, and will be found extremely convenient.

For all kinds of short transit locomotion, electric power will, probably, prove to be the most suitable, and the improvement in the atmosphere of the Underground Railway, which would result from the adoption of such a means of propulsion, would be far more remarkable than anything which ventilators, whether ornamental or the contrary, are able to effect. In fact, there can be little doubt that the practical development of electrical appliances in the near future will be something extraordinary, and the consequence of this, and of the extensive use of gaseous fuel, will be, I most sin-

cerely hope, and not unreasonably believe, not only the abatement, but the compulsory abolition, of smoke and all other artificial pollution of the atmosphere, and such an improvement in the air of towns as will change them from stifling purgatories into wholesome and refreshing centres of life and work.

THE ALGEBRA OF ALGEBRAIC NUMBERS.

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I.

1. The object of the following paper is the discussion of negative quantities as found in algebra, or rather the finding a logically developed system, which shall include such quantities as special cases, and thus tend to the great generalization of problems and theorems.

In the older algebras the fundamental theorems were established on the supposition that the various letters, used as symbols, denoted pure *arithmetical* or *absolute* numbers, and the results were *assumed* to be general or true for the so-called negative quantities as well as ordinary numbers. But there are so many objections to a method without a rational basis, even though it gives true results, admitting of a proper interpretation, that of late years attempts have been made to substitute an algebraic series of numbers, involving both plus and minus quantities, for the ordinary series, so that a definite meaning may be given to a minus number, and the ordinary algebraic processes be given a rational basis for all cases. We shall endeavor in what follows to deduce the ordinary laws of addition, subtraction, multiplication and division, for such a series, calling especial attention to difficulties which naturally occur to any one in the development of the common system, and pointing out how these difficulties are overcome by the "symbolical algebra," as it has been termed.

It will conduce to a better appreciation of the subject if we first give very briefly the usual method of deducing the ground rules for arithmetical numbers and point out their limitations.

We shall first then regard the letters used as symbols to denote ordinary numbers.

2. *Addition.*—Let it be required to add $(8a-9b)$ to $(5b-4a)$.

It is tacitly assumed that $(8a-9b)$ is positive and denotes a real number, also that $(5b-4a)$ is positive and denotes an ordinary arithmetical number, whole or fractional. That is, we assume that if the proper numerical values are substituted for a and b, that $8a$ is greater than $9b$, and that $5b$ is numerically greater than $4a$.

Now, if we simply add $5b$ to $(8a-9b)$, we have $(5b+8a-9b)$, but this sum is too great by $4a$, as we had to add $5b$ diminished by $4a$, so that we must subtract $4a$ from this first result to get the correct sum, which is therefore,

$$5b+8a-9b-4a.$$

In order to reduce this expression to its fewest terms, we have to make use of the *law*, that is easily proved for numbers, that the order in which we combine the terms is immaterial. Thus, if from $(5b+8a)$ we subtract $9b$, the result is $(8a-4b)$, since we evidently reach the same value by adding $5b$ to $8a$ and then subtracting $9b$, as in simply subtracting $4b$ from $8a$. From $8a-4b$, we have now to subtract $4a$, giving $4a-4b$ for the correct answer.

In practice we set down the terms and add thus:

$$\begin{array}{r} 8a-9b \\ \underline{-4a+5b} \\ 4a-4b \end{array}$$

But it must be distinctly understood that $-4a$ by itself means nothing, and that we have only written for convenience like terms under each other, so that $(-4a+5b)$ must be interpreted $(5b-4a)$, which is agreeable to the law mentioned.

From a consideration of such examples we deduce the law in addition: Combine like terms by adding those of like sign and prefixing the common sign, and when of unlike signs take the difference of the sum of the positive and the sum of the negative terms and prefix the sign of the greater.

3. *Subtraction.*—Suppose we have to subtract

$$5b-4a \text{ from } 8a-9b.$$

If we take $5b$ from the minuend, the indicated result is

$$8a-9b-5b;$$

but we have taken away too much by $4a$, for we had only to subtract $5b$ less $4a$, so that we must increase this result by $4a$, giving for the correct answer

$$8a-9b-5b+4a=12a-14b.$$

Combining the terms as mentioned above by adding $8a$ and $4a$ and subtracting $14b$, which is the same thing as first subtracting $9b$ and then $5b$. This result is briefly represented thus:

$$\begin{array}{rl} \text{From} & 8a-9b \\ \text{Subtract} & -4a+5b \\ \hline \text{Remainder} = & 12a-14b \end{array}$$

which is thus equivalent to the rule; change the signs of the subtrahend and proceed as in addition.

We again note that the minuend and subtrahend are tacitly assumed to be real positive numbers, whole or fractional, and further that the subtrahend is numerically in value less than the minuend. There is, besides, no sense in subtracting $-4a$ from $+8a$ by itself, for $-4a$ has no existence by itself. Neither can we subtract $5b$ from $-9b$ since the last term is an absurdity by itself in arithmetical algebra. Neither can any meaning be attached to adding the same terms ($-4a$ to $8a$ or $5b$ to $-9b$), in article 2, though we get so accustomed to using the well-known rules in apparently adding or subtracting such single terms that we lose sight of our tacit assumptions, that it is only when combined into two expressions, both numerically positive and in the case of subtraction, the minuend greater than the subtrahend, that we can apply such rules at all.

4. *Multiplication.*

$$(a+b)c=ac+bc$$

$$(a-b)c=ac-bc$$

$$(a+b)(c+d)=(ac+bc)+(ad+bd)$$

$$(a-b)(c+d)=(ac-bc)+(ad-bd)$$

These four results are obvious enough since $(a+b)c$ means the number $(a+b)$, repeated c times, and is equal to a (one part), repeated c times, added to b (the other part), repeated c times. Similarly we go through. In the next case we have to multiply $(a+b)$ by $(c-d)$ or repeat $(a+b)$ c times and diminish the product by $(a+b)$ repeated d times

$$\therefore\ (a+b)(c-d)=ac+bc-(ad+bd)$$
$$=ac+bc-ad-bd$$

Similarly,

$$(a-b)(c-d)=ac-bc-(ad-bd)$$
$$=ac-bc-ad+bd$$

Now, in place of going through the successive steps given here, we shall reach the same result by multiplying each term of the multiplicand by each term of the multiplier, noting a rule that *like* (prefixed) *signs give plus, unlike minus,* in the product. It is again tacitly assumed that both multiplicand and multiplier are positive (arithmetical) numbers.

There is no sense, *per se*, in

$$-b\times c=-bc$$

or in

$$-b\times -d=+bd$$

It is only in connection with the other terms that such results have any meaning so far as expressions containing only arithmetical numbers are concerned.

5. *Division.*—In division the aim is to find a factor called *quotient* that multiplied by the divisor will give the dividend, so that the rules of signs and limitations hold as in multiplication.

6. We see, therefore, that with such an *algebra of ordinary numbers,* connected by addition and subtraction signs, that we are very much circumscribed, for we have seen that the expressions we treat must be essentially positive, whereas, in numerous investigations, it is impossible to say whether all the expressions we combine by addition, subtraction, multiplication or division, are positive or negative, so that we are, perforce, obliged to apply our rules to negative quantities

without having defined them or proved any rules with respect to them.

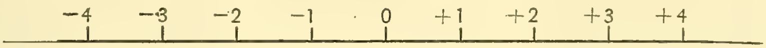
In certain problems, too, the unknown quantity is found with a negative sign before it, and we have to go back and vary the statement of the problem to make the result intelligible, when that is possible.

The great value that lies in the conception of the true, real, negative quantities, consists in the wonderful generality given to general formulæ, which may be deduced, say, on the supposition that all the letters denote plus quantities, when a simple substitution in the final result of the actual values (in any case) of the letters (for that case) will give a correct formula.

In the applications of algebra to geometry we shall find still stronger reasons for an early consideration of + and — quantities.

We thus see that it is not only necessary in certain cases but highly important that the greatest generality should be given to algebraic processes, and we shall now proceed to the consideration of the algebraic series of numbers, which, together with an extension of meaning given to the signs plus and minus, will enable us to give the generality desired.

7. The algebraic system of numbers is supposed to increase from zero in the opposite directions as in the scale below.



Those numbers to which are prefixed the + sign are called positive numbers, those with the negative sign minus numbers; so that (—a) means a quantity or number of *real* units, but measured in a direction opposite to the positive one. There is no meaning in the statement that (—a) is "less than zero" without a new definition of "less than," so let the reader bear in mind that in dealing with ($-a$) he is dealing with a real arithmetical number, though taken in the opposite sense to ($+a$).

For the present it is well to indicate an algebraic number by enclosing it in a parenthesis.

The + and — quantities above may be conceived as measuring distances right and left from 0, so that the line drawn will be considered as divided into equal parts and may be called the scale of numbers. In this case direction to the right is called +, that to the left −, but we can just as well assume the positive direction to the left when the negative way of counting will be to the right.

As illustrations of quantities having opposite directions, we may mention motion forward and backward, or degrees above and below zero on a thermometric scale. Thus, if a moving point, starting at 0 in the scale, moves 5 units of distance to the right and 9 to the left, we reach (−4) on the scale which thus indicates that the final position of the moving point is 4 units to the left of the starting point or zero of the scale.

Again, if it moves 5 units forward, then 9 units backwards, then 6 units forward, its final position will be 2 units from 0 in a forward direction, or to the place denoted by (+2). If direction forwards is called +, then direction backwards will be — as we see illustrated in this case.

8. *Addition of Algebraic numbers.—Rule.* To add two algebraic numbers, *we start at the point in the series indicated by the first number and count in the direction given by the sign of the second number the number of absolute units in that number.*

This operation is indicated by putting the + sign between the algebraic numbers; thus to add (+3) to (—5) we start at (+3) in the scale and count in the negative direction 5 units, thus bringing us to (—2). The symbolical representation is as follows:

$$(+3)+(-5)=(-2).$$

Similarly, in "adding" any number of algebraic numbers, we add the first two as just explained, then the third to the previous result by the same rule, and so on.

Thus,

$$(+3)+(-5)+(+3)=(+1).$$

Here we start at (+3), go 5 units to left to (—2), then 3 units to right to (+1), the result of the *algebraic addition.*

We see that the rule for adding two algebraic numbers is consistent with the

definition, *that* $(-a)$ *means absolute units measured in the opposite direction on the scale of numbers to the plus direction, so that when combined by the sign of addition with* $(+a)$, *the result is zero.*

9. By the consideration of examples like the following:

$$(+3)+(+2)=(+5);$$
$$(-3)+(+2)=(-1);$$
$$(+3)+(-2)=(-1);$$
$$(-3)+(-2)=(-5;$$

We easily deduce the rules:

I. To add two numbers with *like* signs, find the arithmetical sum of their absolute values and prefix the common sign to the sum.

II. To add two numbers with unlike signs, find the arithmetical difference of their absolute values and prefix the sign of the greater.

Where there are many numbers to add of different signs, we place the + numbers in one column, and the − numbers in another column, add separately and combine the sums. It is evident that the result is the same in whatever order we add algebraic numbers, since the total number of units we go to the right, by the successive steps, equals the sum of the absolute values of the positive numbers. Similarly for the minus numbers; so that the final position is given by the relation between the two sums.

10. Let a and b denote the absolute values of any two numbers, then the four cases given in article 9 can be generalized and written in the following forms.

$$(+a)+(+b)=a+b;$$
$$(-a)+(+b)=-a+b.$$
$$(+a)+(-b)=a-b;$$
$$(-a)+-b)=-a-b.$$

The interpretation to be given to the right members must follow from some convention, and this convention is, that when a quantity, as a, *has no sign before it, the plus sign must be understood;* so that a is the same thing as the algebraic number $(+a)$; likewise, that *if a quantity* a *has a + or − sign before it, that sign will indicate whether it is a + or − algebraic number*, to be combined by addition with any other numbers in the same expression.

Thus, $a-b$ is the same as $(+a)+(-b)$, and $-a-b$ the same as $(-a)+(-b)$.

This convention enables us to dispense with parentheses and a multiplicity of signs when desired; though for the sake of clearness we shall retain them in establishing fundamental rules, whilst indicating, at the same time, abreviated forms. However, if an expression is given in the abbreviated forms, shown by the right members of the above equations, we can always express it in the manner shown by the left members, when desirable.

II. We will now show how the use of algebraic numbers enables us to generalize by expressing all the cases of a certain problem by one equation.

We must always premise by stating what class of quantities are to be regarded as +, and which (those opposed to the + one) minus. Thus, in the case of the moving body in art. 7, let us count distances forward +, those backward, minus.

Then, if a person walks say 5 feet forwards from a given origin, then 9 feet backwards along the same straight line produced when necessary, we can say he moves successively $+5$ and -9 feet, thus stopping 4 feet back of the origin, or at -4. Thus, if we call X his final distance in feet from the origin, + if forwards, − if backwards, we can write,

$$X=(+5)+(-9)=(-4),$$

or, more simply,

$$X=5-9=-4.$$

Again, if the person moves 5 feet forwards, then 9 backwards, then 6 forwards, we have his ultimate position,

$$X=5-9+6=+2;$$

or two feet in front of the origin.

Now, no matter how many movements forwards and backwards are made, we see that we can state generally that the final position of the moving body is the algebraic sum of his successive movements, so that,

$$X=(a)+(b)+(c)+ \quad . \quad . \quad .$$

where a, b, c, . . . represent the successive movements which are essentially negative when backwards, otherwise positive, as written.

Again, as to dates, let dates reckoned

after Christ be designated as +, those anterior −, and let any interval of time be regarded as + if reckoned forwards, − if reckoned backwards; then having given a date D and the interval I to another date D′, we can find the letter by the formula,

$$D+I=D'.$$

What is the date 20 years after 240 B.C.?

$$(-240)+(20)=-220,$$

or 220 B.C.

What is the date 20 years before 240 B.C.?

$$(-240)+(-20)=-260,$$

or 260 B.C.

What is the date 40 years after 20 B.C.?

$$(-20)+(+40)=(+20), \text{ or } 20 \text{ A.D.}$$

Julius Cæsar died A.D. 14, aged 77 years; when was he born?

$$(+14)+(-77)=-63, \text{ or } 63 \text{ B.C.}$$

Similarly for any series of successive intervals we shall always have,

$$D+\text{sum of intervals}=D'.$$

Thus one formula comprehends every case. Other illustrations of such generalizations can be made in the case of quantities that stand in opposition, as *assets* and *debts*, *gain* and *loss*, &c. The algebraic sum of assets taken as plus, and sum of debts as minus, gives total assets if the result is +, or total indebtedness if −, &c.

Examples of a far more complicated character will be given further on, as we deduce rules for subtraction, multiplication, &c., of algebraic numbers.

Subtraction.

12. We have previously only used the + sign before an *algebraic* number, having clearly defined the process of addition intended by it. We shall similarly give a definition of a *minus sign* before an *algebraic number*, consistent with the use already made of it to indicate a negative number, and will remark that everything that follows will be strictly deduced from the consequences of these definitions.

Definition.—A minus sign before an algebraic number indicates that the number is to be counted in an opposite direction to that indicated by its sign, or it indicates a reversal of direction of the line representing that number.

Thus,

$$-(+5)=(-5),$$
$$-(-5)=(+5).$$

This process involves no absurdity for single numbers; for the results (−5) and (+5) in the two cases are perfectly intelligible; meaning algebraic numbers, the one plus, the other minus, that will cancel each other if added together. From the definition, a − sign before an arithmetical number causes it to be counted in the opposite direction to the usual or + direction, as the number may be regarded as a + number.

13. When this minus sign is placed between two numbers, as $(+8)-(+5)$, it indicates what is called *algebraic subtraction.* The quantity before which it is placed is called the subtrahend, the other one the minuend.

From the definition, it follows that to subtract one quantity from another, *we change the sign of the subtrahend and add.*

Thus,

$$(+3)-(+1)=3-1=2,$$
$$(+3)-(-2)=3+2=5,$$
$$(-3)-(+1)=(-3)+(-1)=(-4),$$
$$(-3)-(-1)=(-3)+(+1)=(-2).$$

We have left the parentheses around certain minus quantities in the right numbers, though it is not necessary if we attend to the meaning given in art. 10.

It is seen from a consideration of these cases that the *absolute number* given as the result of the algebraic subtraction of two algebraic numbers, *gives their distance apart on the scale of numbers, and that the prefixed sign is the direction from the subtrahend to the minuend.* Hence the result is called the *algebraic difference.*

As in art. 10, we can write briefly,

$$(+a)-(+b)=a-b\ ; \qquad (-a)-(+b)=-a-b,$$
$$(+a)-(-b)=a+b\ ; \qquad (-a)-(-b)=-a+b.$$

In the right numbers of these equations the a's and b's are to be taken with the sign prefixed to each, or understood according to the convention given

in art. 10, and added by the rules given in art. 9.

14. We see from the foregoing that we employ the same rules exactly as in addition and subtraction of polynomials in the arithmetical algebra, and that when + single numbers are taken, the algebraic sum or difference of any two of them is exactly as in the arithmetical algebra. As before explained, arithmetical algebra cannot handle single minus numbers or expressions that are essentially minus; but now there are no limitations as to signs, for the operations on negative quantities are just as intelligible and as clearly defined as on positive quantities.

15. On comparing the formulæ just given in art. 13 with those in art. 10, we notice the relation:

$$(+a)+(+b)=(+a)-(-b),$$

$$(+a)+(-b)=(+a)-(+b),$$

or

$$a+b=a-(-b),$$

$$a+(-b)=a-(+b)=a-b.$$

So that algebraic additions can always be written as algebraic subtraction by changing the sign of the term, which is to appear as the subtrahend. We can thus go from one form to the other at pleasure. In fact, from the comprehensive definition given to a minus sign before an algebraic number (article 12), we can write,

$$a+b=a-(-b)=a-[-(+b)]$$
$$=a-[-(-(-b))], \text{ etc.}$$

Similarly,

$$a-b=a-[-(-b)]$$
$$=a-[-\{-(-(-b))\}].$$

There is thus choice in the form in which a given value can be expressed.

16. The principles deduced for algebraic numbers apply equally to algebraic expressions, composed of several numbers combined in any way, since, if the numerical values are assigned to the letters and the indicated operations performed, the expression reduces to a single number when the rules apply.

Thus to subtract $5b-4a$ from $8a-9b$, we have,

$$8a-9b-(5b-4a)=$$
$$8a-9b-5b+4a=$$
$$12a-14b.$$

Compare the result in art. 3, where it was premised that $8a$ was greater than $9b$, and that $5b$ was greater than $4a$, also that the subtrahend $(5b-4a)$ was less than the minuend $(8a-9b)$. By the use of algebraic numbers there are no limitations of this kind whatever. As in this example, we shall generally, in what follows, leave off the parentheses to indicate the algebraic numbers. From a consideration of the results in art. 15, the expressions can be transformed so as to contain them when desired.

Multiplication.

17. In multiplying any number by a, we understand that the number is to be repeated a times, so that we only have to define what is meant by multiplying by $+a$ or $-a$; in other words the significance of the + or — signs before a multiplier.

Now, *since* a + sign before an algebraic number does not reverse the direction of counting (art. 10), and a —sign does (art. 12), we shall define,

1. Multiplication of an algebraic number by $(+a)$ means that the number is to be repeated a times, the direction of counting being unchanged.

2. Multiplication of an algebraic number by $(-a)$ signifying that the number is to be repeated a times, and the direction of counting reversed.

Thus,

$$(+a)\times(+b)=+ab,$$

$$(-a)\times(-b)=+ab,$$

$$(+a)\times(-b)=-ab,$$

$$(-a)\times(+b)=-ab.$$

From the first two results we deduce the rule, "*like signs give plus*," and from the last two the rule, that "*unlike signs give minus.*"

18. Since *division* may be defined as finding the factor called quotient, when the other factor (the divisor), and the product of the two factors, called the dividend, is given, it is seen that the same rules for signs hold for division.

Thus,

$$\frac{(+ab)}{+b}=a;\frac{(+ab)}{-b}=-a;$$

$$\frac{(-ab)}{-b}+a;\frac{(-ab)}{+b}=-a.$$

19. Since a fraction may be considered an indicated quotient, the same laws as to signs hold for fractions.

20. Having defined multiplication of single numbers by plus and minus quantities, we must now ascertain if these rules can be utilized for the multiplication of polynomials by polynomials.

Now,

$$(a-b)n=(a-b)+(a+b)\ \ldots\ n \text{ times},$$
$$=a+a+\ \ldots\ n \text{ times}, -b-b-\ \ldots\ n \text{ times},$$
$$=na-nb.$$

This is called the distributive law. Similarly, to multiply $(a-b)$ by $-n$, we get the same result by *definition*, except that the direction of counting of the result is changed, so that

$$(a-b)\times -n=-na+nb.$$

Hence the rule: multiply each term of the polynomial by the monomial, according to the laws laid down for single numbers, and add the results. Next suppose the multiplier, $(+n)$ or $(-n)$, is itself expressed as a polynomial. Thus, if 10 is to be multiplied by $8-3$ or 5, it is evident that we get the same result by repeating the 10 8 times in the positive direction and 3 times in the negative direction, and adding the results as by multiplying the 10 by 5.

This operation is recorded thus:

$$\begin{array}{l} 10 \\ +8-3 \\ \hline +80-30=50 \end{array}$$

That is we multiply the multiplicand by each term of the multiplier according to the laws for single numbers, and add the partial products.

Similarly we multiply 10 by $-8+3$ or -5,

$$\begin{array}{l} 10 \\ -8+3 \\ \hline -80+30=-50 \end{array}$$

Here the direction of the 10 is reversed on multiplying by -8, giving -80; but not reversed on multiplying by the $+3$, which gives $+30$, which added to -80 gives -50; exactly what we should obtain by repeating the 10, 5 times in the negative direction. There is no difference in principle in multiplying a negative multiplicand by a polynomial.

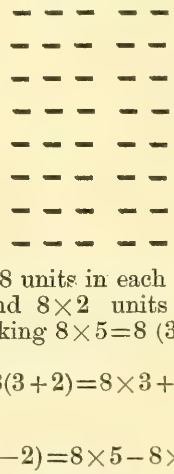
In fact we are not limited, as in the case of arithmetical numbers, but both multiplicand and multiplier can be essentially negative, and the results are as perfectly intelligible as in the case of absolute numbers. The laws just deduced, by combination enable us to apply the usual rules to the multiplication of any polynomial by another polynomial. Thus to multiply $(-a-b)$ by $(c-d)$, we obtain,

$$\begin{array}{l} -a-b \\ \quad c-d \\ \hline -ac-bc+ad+bd. \end{array}$$

That is, we multiply the $(-a-b)$ first by c, by the distributive law, without reversing, and then by d, reverse the last result, and combine with the other.

21. The rules for the division of polynomials by polynomials, are deduced from these in the usual manner.

22. The rule for multiplying by a polynomial may be illustrated graphically as follows: Let it be required to multiply 8 by $(3+2)$, or generally m by $(a+b)$.



There are 8 units in each vertical row, and 8×3 and 8×2 units in the two columns, making $8\times5=8\ (3+2)$ units in all.

$$\therefore 8(3+2)=8\times3+8\times2.$$

Similarly,

$$8(5-2)=8\times5-8\times2;$$

for 8×5 gives the whole number of units and $8+2$ those of the right column, so that their difference equals the number in the left column $=8\times3=8(5-2)$.

23. We shall note a special case of multiplication, $(-a)^2$ as the form often occurs. Now by the rule, article 17, we have

$$(-a)^2=(-a)\times(-a)=\times a^2-$$

for $(-a)^2$, means that $(-a)$ is to be repeated a times and the direction of counting reversed.

If this result is again multiplied by $-a$, by the same rule, we obtain,

$$(-a)^3=+a^2\times -a=-a^3$$

Similarly

$$(-a)^4=(-a^3)\times -a=+a^4\ ;$$

so that a negative quantity raised to an even power gives a + result; to an odd power a — result.

24. We shall group together some of the fundamental formulae so far deduced, and note an important fact resulting from the comprehensiveness of the definitions relating to the influence of signs.

The following formulae have been drawn from articles 10, 13, 17 and 18; and it is to be observed that the left members indicate the addition, subtraction, multiplication and division of algebraic numbers, and the right members are intended to show the indicated operation *performed*, as far as this can be done where numerical values are not assigned to a and b.

We have put parentheses around the algebraic numbers, in the left members, as before, for without them, in the case of $a-b$, we might be in doubt whether we are to include it under the rule of addition and write it, $(+a)+(-b)$ or to consider it a case of subtraction and write it $(+a)-(+b)$. The parentheses around the plus quantities as well as the sign + enclosed, can be omitted, if desired, since a quantity is assumed as +, when it has no sign before it, so that $a+b$ would generally be understood as $(+a)+(+b)$; but as it fixes the attention more precisely upon the algebraic operations involved, the parentheses were retained in the left members throughout.

The right members are, of course, algebraic members, only under a different form from the left numbers (see article 10), the results of the combinations, according to the rules, in both members, being precisely identical.

The formulae are as follows:

$$1.\ \begin{cases}(+a)+(+b)=a+b; & (-a)+(+b)=-a+b;\\ (+a)+(-b)=a-b; & (-a)+(-b)=-a-b\ ;\end{cases}$$

$$2.\ \begin{cases}(+a)-(+b)=a-b; & (-a)-(+b)=-a-b;\\ (+a)-(-b)=a+b; & (-a)-(-b)=-a+b;\end{cases}$$

$$3.\ \begin{cases}(+a)\times +b-+ab; & (+a)\times -b=-ab;\\ (-a)\times -b=+ab; & (-a)\times +b=-ab;\end{cases}$$

$$4.\ \begin{cases}\dfrac{(+ab)}{+b}=a; & \dfrac{(+ab)}{-b}=-a;\\[2ex] \dfrac{(-ab)}{-b}=a; & \dfrac{(-ab)}{+b}=-a.\end{cases}$$

Now note, that if in the left members we substitute $(-a)$ everywhere for a and perform the indicated operations, as far as may be, that the result is precisely the same as if we substitute $(-a)$ for a in the corresponding right members and reduce according to the laws established.

Thus put $(-a)$ for a in both members of the first formulae of each series, we obtain

$$(-a)+(+b)=-a+b$$
$$(-a)-(+b)=-a-b$$
$$(-a)\times(+b)=+(-a)b=-ab$$
$$\frac{(-a)b}{+b}=(-a)=-a.$$

Thus having deduced a formula, as $a\times b=ab$, we can ascertain the result for $(-a)\times b$ by substituting $(-a)$ for a in the right member, or performed multiplication, giving $(-a)b=-ab$.

25. Again to multiply $(a+b)$ by a, we indicate the process as follows:

$$\begin{array}{l} a+b \\ a \\ \hline a^2+ab \end{array}$$

This may be called a general formula, true whether b is + or —; for if b is minus, so that we have to multiply $a+(-b)$ by a, we have only to change b to $(-b)$ in the result and reduce by the laws of signs, giving the correct answer, a^2-ab. This follows from the principle just noted.

In the formula,

$$n(a+b+c)=an+bn+cn,$$

if we put $a=-x$, $b=-y$, in both numbers, the results must be identical by the principle or law that was found to exist.

$$\therefore n(-x-y+c) = -xn-yn+cn.$$

In the multiplication or division of polynomials by polynomials, the same general principle as to substitution exists, for the results are derived from the multiplication, division, addition and subtraction of monomials according to the rules affecting single numbers.

Thus by actual multiplication, we deduce the formula,

$$(a+b)^2 = a^2+2ab+b^2$$

By substituting $(-b)$ for b in this formula, both sides, and reducing, we find the correct formula,

$$(a-)^2 = a^2-2ab+b^2.$$

We can put the actual multiplications side by side to make this plainer:

$$\begin{array}{ll} a+b & a+(-b) \\ a+b & a+(-b) \\ \hline a^2+ab & a^2+(-ab) \\ \quad +ab+b^2 & \quad +(-ab)+(-b)^2 \\ \hline a^2+2ab+b^2 & a^2-2ab+b^2 \end{array}$$

Thus if in the partial product $(a+b)\times a = a^2+ab$ we substitute $(-b)$ for b, we get the first partial product on the right. Similarly for $(a+b)\times b$. Now since, by article 15, we can write $+(-ab) = -ab$, and by article 23, $(-b)^2 = +b^2$, the result on the right is $a^2-_2ab+b^2$, just what we find by putting $(-b)$ for b in the final result on the left.

Now it is readily seen that all this follows from the general principle enunciated above, so that it is not necessary to analyze every case in this way, but we may consider that the principle of substituting $(-a)$ for a on both sides of any formula giving identical results as established.

Again having deduced the general formula,

$$(a+b+c)^2 = a^2+b^2+c^2+2ab+2ac+2bc,$$

if in any particular example b and c say are minus, we have at once without the necessity of multiplication, by substitution only,

$$(a-b-c)^2 = a^2+b^2+c^2-2ab-2ac+2bc.$$

26. The principle in question is likewise evident if the minus quantity is always kept in a parenthesis, thus simply taking the place of the corresponding + quantity throughout. Thus to cube $(a-b)$, we may multiply as follows:

$$\begin{array}{l} a+(--b) \\ a+(-b) \\ \hline a^2+a(-b) \\ \quad +a(-b)+(-b)^2 \\ \hline a^2+2a(-b)+(-b)^2 \\ a+(-b) \\ \hline a^3+2a^2(-b)+a(-b)^2 \\ \quad +a^2(-b)+2a(-b)^2+(-b)^3 \\ \hline a^3+3a^2(-b)+3a(-b)^2+(-b)^3 \\ = a^3-3a^2b+3ab^2-b^3. \end{array}$$

If we had formed the product $(a+b)^3$ along side of this, we should have noted the identity throughout, except that b is everywhere simply replaced by $(-b)$, so that the final result can be obtained from that pertaining to $(a+b)^3$ by simply changing b to $(-b)$ and reducing according to the laws of signs.

Similarly from the developments of $(a+b)^n$, we deduce the formula for

$$(a-b)^n = a^n - na^{n-1}b + \frac{n(n-1)}{1.2}a^{n-2}b^2 -$$

&c.

27. Numberless illustrations may be given of the application of the rule. We shall content ourselves with mentioning two more general formulæ: the first pertaining to simple quadratic equations, the other to simultaneous equations of the first degree with two unknown quantities.

From the general equation of the 2d degree

$$x^2+px+q=o,$$

we deduce

$$x = -\frac{p}{2} \pm \frac{1}{2}\sqrt{p^2-4q}$$

Now if we have given the equation

$$x^2-x-20=0,$$

on comparing it with the first one we note that

$$p=(-1) \text{ and } q=(-20),$$

hence, substituting these values in the expression for x, we find the value in this case.

$$\therefore\ x=-\frac{(-1)}{2}\pm\frac{1}{2}\sqrt{(-1)^2-4(-20)}$$

$$\therefore\ x=\tfrac{1}{2}\pm\tfrac{9}{2}=5\ \text{or}\ -4.$$

Again take the equations

$$\left.\begin{array}{l} ax+by=c \\ a'x+b'y=c' \end{array}\right\}$$

By elimination we obtain

$$x=\frac{cb'-bc'}{ab'-ba'}$$

$$y=\frac{ac'-ca'}{ab'-ba'}$$

In case any of the numbers a, b, c, a', b', c' are minus, in any particular example, we can at once write the values of x and y from the last two equations by a simple substitution, without the necessity of an actual elimination process.

This is evident, because in this process in the general case we should have only to substitute $(-a)$ for a, etc., throughout, to get the corresponding expressions where a, . . . were minus.

We have now deduced all the fundamental rules of algebra from a few comprehensive definitions relating to the meaning of + and − signs in connection with numbers, combined in various ways, and have shown the wonderful generality given to formulæ by the use of the algebraic negative quantity.

28. We shall now discuss some problems where we shall similarly reach the greatest generality by the same means. In fact the use of negative quantities is necessary in certain problems, where although $(a-b)$, to take an illustration, may be assumed as plus at the beginning, yet it may turn out in the end that $a<b$, so that we have supposed an impossible subtraction if a and b are regarded as absolute numbers. By the use of the algebraic series, however, all difficulty disappears, not only in the work but also in the interpretation of the answer when minus; though here it is necessary to state in the beginning the positive sense, when the negative will be just the reverse.

29. *Problem* 1.—A's age is now a years, and B's age is now b years, how many years before or after this epoch to the period when A's age is m times B's.

Let x=number of years, regarded as + if reckoned in the future, − if in the past; then in any case

$$a+x=m(b+x).$$

We see that if x turns out to be plus, the formula is correctly stated, as well as when x is minus, or when the period antedates the present.

Solving we at once find

$$x=\frac{a-mb}{m-1}$$

Now, if $a=40$, $b=15$, $m=2$, then $x=+10$ years; *i. e.*, in 10 years A's age will be double B's.

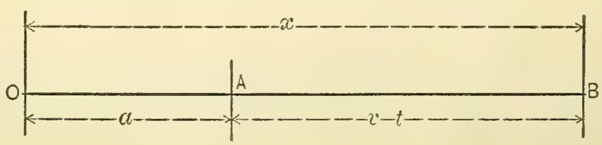
But if $a=35$, $b=20$, $m=2$, then $x=-5$, that is, A was twice as old as B 5 years ago. It is not necessary to make a new statement of the problem, so that x shall come out +, as is generally done. The result is perfectly intelligible as it stands.

30. *Problem* 2.—The celebrated problem of the couriers. Two couriers, at a certain instant of time, are at certain points on a straight road, and moving towards or from each other at certain uniform velocities. When were they together, or when will they be together?

It seems impossible, at first glance, that one solution can cover all the cases that can be imagined, but we shall find it to be so.

In the figure below the first courier is supposed at the point A, distant a from O, *the same instant* the second courier is at some other point A′ (not shown in the figure), distant a' from O.

Now, the two couriers are supposed to meet at B, distant x from O, either t hours before or t hours after passing the two points A and A′; in the first case t will be called minus, in the second case plus. Similarly,



x, a and a' will be + if measured to the right of O, otherwise −.

The rate per hour, or velocities v and v', at which the couriers travel, will be called + if the motion is to the right, − if to the left in either case.

We are thus carrying out principles already employed in article 11, that distance to the right will be called +, if distance to the left is minus; time after is +, if time before is −, and motion to the right + when motion to the left is called minus.

Where all the quantities are plus, we can at once write the formula for the first courier,

$$x=a+vt \quad . \; . \; . \; . \; . \quad (1).$$

But, by art. 11, this formula holds in all cases, for $x=a+vt$ is the algebraic sum of two numbers, and thus gives the distance of the courier from the origin O at the time of meeting, this distance being measured to the left if x is minus, otherwise to the right.

This can be made plain by noting that if v is + and t +, the product vt=distance AB is +; if the courier is going to right still, but he met the other one + hours before reaching A, then + is −, and vt is −, as it should be, since B is to the left of A.

Now, for the two cases where the motion is to the left, or v minus, t may be + or −; if t is +, or the time of meeting was t hours after the courier passed A going to the left, then the term vt will be minus in the formula; when t is − also, vt will be plus, for now the point of meeting is to the right of A.

The reader can readily illustrate the various cases, dependent upon the signs of a, v and t, by diagrams. For the second courier, we should have the analogous formula for the distance OB,

$$x=a'+v't \quad . \; . \; . \; . \quad (2),$$

whence, placing the two values of x equal, we deduce,

$$t=\frac{a'-a}{v-v'} \quad . \; . \; . \quad (3),$$

$$x=\frac{va'-av'}{v-v'} \quad . \; . \; . \quad (4).$$

At the same instant of time the two couriers are respectively at the distances a and a' from O, so that $(a'-a)$ in (3) represents their distance apart at that instant (article 13). Likewise $(v-v')$ is the arithmetical difference or sum of their velocities according as they are going in the same or opposite directions; hence we know eq. (3) to be correct independently of previous considerations.

As a particular case, let $a'=10$ miles, $a=(-20)$ miles, $v=4$, and $v'=6$ miles per hour, then from (3) we find,

$$t=\frac{10-(-20)}{4-6}=\frac{30}{-2}=-15.$$

That is, they met 15 hours before the time first courier was at A, distant 20 miles to the left of O.

Now, in 15 hours the first courier travels $4\times15=60$ miles, so that they met $60+20$ or 80 miles to left of O, which we likewise find from formula (4),

$$\therefore\ x=\frac{4\times10-(-20)6}{-2}=-80.$$

For the special cases when $v=v'$, (3) gives,

$$t=\frac{a'-a}{o}=\infty;$$

or they will never meet, as is evident, since the assumption is, they have the same velocity and are going in the same direction.

If, besides, $a'-a=o$, or they are together at the start, they will be together at all times, so that t can be anything. Here the equation takes the singular form,

$$t=\frac{o}{o}$$

31. We have now given the theory and a sufficient number of illustrations of the use of the algebra of algebraic numbers to show the generality obtained by its use. The theory has been developed by giving comprehensive meanings to the signs + and −, so as to include arithmetical algebra as a special case when the numbers or symbols representing them are all plus quantities.

It is not proposed to develop the theory further, except to note an extension of meaning given to "greater than" or "less than," with a necessary caution.

Thus an algebraic quantity is said to be *greater than* another when it lies in the positive direction from it.

Thus, we say that $5<-1$, and $-2<$

-3. This is a very useful convention in generalizing results, but it must not be inferred from it that there is such a thing as a *ratio* between a + and a − quantity.

Before the algebraic series of numbers, with the use to be made of it, had been brought forward, there was considerable difficulty in the comprehension of negative quantities as they appeared in analytical investigations, especially where a minus quantity was defined or understood, in some fashion, to be less than zero.

Thus Carnot follows D'Alembert in quashing the whole business as follows:

Consider the proportion.

$$1 : -1 : : -1 : 1$$

If -1, is less than zero, then we have a greater is to a less as a less is to a greater, which is an absurdity ; and yet the product of the extremes is equal to the product of the means, so that the four quantities should be in proportion apparently. Q. E. D.

But we have tacitly assumed here that there is a ratio of 1 to -1, and it is just here that the absurdity is introduced, for from the original definitions of × and − quantities we see that they are of such opposite character that they can have no ratio, though by the technical rule for division, (which is deduced from that for multiplication) the quotient $\frac{1}{-1}=(\frac{1}{1}$ and the direction reversed) giving -1 for the result, is the same as $-\frac{1}{1}$.

If we choose to *extend* the ordinary arithmetical meaning of *ratio* to include algebraic numbers, well and good ; but with such merely conventional definitions, it would be next in order to *ascertain*, if in any proportion, we must always have,

greater : less : : greater : less.

And we should soon find, by the consideration of this very example in fact, that such a criterion does not exist.

32. This example should serve as a valuable caution to the student, against assuming anything that may turn out to be inconsistent with the definitions. The algebraic series of numbers is essentially much more comprehensive, in most respects, than the natural series, so that in using it as the basis of an algebraic system, we should be all the more careful to give few and comprehensive definitions that are consistent throughout, and deduce the laws of the new algebra entirely from these definitions and obvious elementary principles.

I have fault to find with writers of text books in this very respect, that a number of definitions or rules pertaining to the fundamental operations are given, which to the student seem not to have the proper connection, or in fact may be conflicting; so that he cannot give his full assent, or at least reserves his judgment until the whole ground can be carefully reviewed and the bearing of each case upon every other closely analyzed.

We have endeavored, in what precedes, to deduce the laws from obvious principles, after giving precise, though comprehensive definitions of the meaning of + and − whenever used, whether in multiplication or division, addition or subtraction.

There must be but one meaning to each of these signs, and that meaning is very simple : plus meaning counting forwards, or in the usual direction ; minus counting backwards or in the reverse direction. And yet from these simple definitions, we devise the algebraic series of numbers ; and then deduce, with definitions consistent with the first, all the fundamental ground rules of addition, subtraction, multiplication and division for both simple and compound expressions.

Thus we have not to give new definitions of + and − under the various heads enumerated, and perhaps leave the student to find out if these meanings be consistent, but we agree in the first instance that wherever + or − occur, we shall understand but one meaning to each of them. Then there can be no inconsistency ; for a sign, as −, before either a single term, a product, a quotient or any compound expression, means simply but one thing, that the indicated result must be counted in the reverse direction to that assumed for the positive direction.

Again, supposing the ground rules established in a logical manner, illustrations should follow, without which the student of algebra often wonders why isolated negative quantities are introduced at all ; and then again whilst the

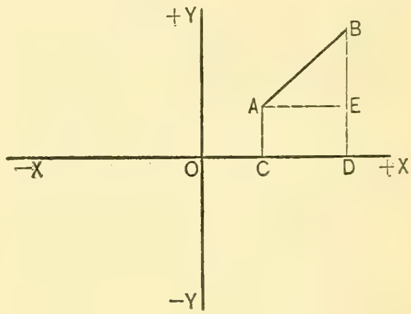
principle of substitution in a general formula, as illustrated in previous articles, is universally made use of, yet I am unacquainted with any attempt at demonstrating the universal correctness of the process.

The truth is the books give too little, and that in too disconnected a form, for the student unaided, to grasp the entire subject. Especially is this true in the transition from multiplication of simple to that of compound expressions.

33. It would lead us far beyond the scope of this article to take up the numberless applications of the algebraic numbers to trigonometry and the higher analysis. Besides in trigonometry, modern text books have already shown the generality of all the formulae by the use of negative numbers, so that little remains to be said. In analytical geometry, though, there is a singular lack in demonstrations going to show the universality of the formulae deduced, to all angles and quadrants; and yet this very generality is often assumed, as if either, sufficiently evident, or following from some sort of general principle previously demonstrated. Now, neither of these suppositions are correct. No student finds the generality assumed, either self-evident or following from principles already demonstrated. In fact, it is absolutely necessary to show, *for each case*, that the formulae deduced is general, for problems can easily be given, in the application of algebra to geometry, where this generality cannot be attained. This is an important difference between purely algebraic formulae and those used in the solution of some geometrical problem. Further, it will often happen that a formulae deduced for a certain case, may include other cases of a different kind, where the algebraical transformations have been such as to increase the number of the roots, over those originally contemplated. Thus in squaring an equation and then reducing and solving, we can introduce double the number of roots intended originally; but generally the false solutions are easily detected.

34. It will be found, too, as a rule, that the generality of the formulæ of analytical geometry and trigonometry rests on the tacit assumption that the rules of signs, as laid down in algebra, are to be observed, although there may be no meaning attached to some of the operations in themselves in certain cases. To make my meaning clear to the students of analytical geometry, let us find the analytical expression for the distance between two points whose co-ordinates are respectively x_1, y_1, and x_2, y_2.

Let A and B in the figure, represent the points, so that drawing AC and BD parallel to OY, we have $OC=x_1$, $AC=y_1$; $OD=x_2$, $BD=y_2$.



Then, by the theorem that "the square on the hypotenuse of a right triangle is equivalent to the sum of the squares on the other two sides," we have,

$$\overline{AB}^2=(x_2-x_1)^2+(y_2-y_1)^2.$$

To ascertain if this formula is general or true, no matter what the co-ordinates x and y, let us first suppose y_1 and y_2 to remain constant, so that A and B will both be above the axis of X, and remain at the same distance from it. Then if B lies to the left of A in the first quadrant, (x_2-x_1) will be negative, but its square $(x_2^2-2x_2x_1+x_1^2)$ will be the same as before, so that the formula is unchanged.

Again, if A and B are both to the left of OY, then x_1 and x_2 are both minus, but $(x_2-x_1)^2$ is equivalent to the square of the projection of AB on OX, as in the former case. Similarly, when A and B are on different sides of OY; for now (x_2-x_1) will no longer be an arithmetical difference, but a sum, or minus a sum, which is the projection of AB on OX, so that $(x_2-x_1)^2$ will be equal to the square of this projection, as before. It is, moreover, evident that the term $(x_2-x_1)^2$ will remain of the same form whether A and B are both above, both below, or one above and the other below OX.

We can prove in an exactly similar manner that $\overline{BE}^2=(y_2-y_1)^2$, no matter in what quadrants A and B may be found, or what the relative values of y_2 and y_1. So that the *one* formula is true for every supposable case, with the understanding, implied as it is, that the rules of signs as deduced in algebra are in force here, and of course the signs of x and y as given in analytical geometry. Thus, to take a numerical example, let

$$x_2=-5,\ x_1=+2,\ y_2=+4,\ y_1=+6,$$

then,

$$\overline{AB}^2=(-5-(+2))^2+(4-6)^2$$
$$=(-7)^2+(-2)^2,$$
$$=49+4=53,$$
$$\therefore\quad AB=\sqrt{53}.$$

Now, the point that I have to make is this, that the formula in the form,

$$\overline{AB}^2=(-7)^2+(-2)^2$$

means nothing, though it leads to a true formula,

$$AB^2=(7)^2+(2)^2,$$

which carries out the theorem that the square on the hypotenuse equals the sum of the squares on the other two sides; whereas $(-7)^2$ cannot be said to represent the square on a side, for $-7\times-7=(-7)^2$ conveys no such meaning as the square on a side. Hence, although the general formula, *as it stands*, does not seem to carry out the principle of the square on the hypotenuse, &c., for *any* values of the co-ordinates, yet for any such values at pleasure, it can be at once transformed into a formula which is an analytical expression of this theorem, so that the formula is said to be general.

We note, further, that on extracting the square root, in the example above, according to the laws of signs, that we get two solutions,

$$AB=\pm\sqrt{53};$$

but the minus result must be discarded, since AB was tacitly assumed as positive from the beginning. This illustrates another point noted above.

THE CONDITION IN WHICH CARBON EXISTS IN STEEL.

From "The Engineer."

An inquiry into the condition in which carbon is present in steel as it is left by the cold-rolling—as well as in its hardened, annealed and intermediate states—has been made by Sir F. Abel and Mr. Deering for the Committee on Steel of the Institution of Mechanical Engineers. Two series of experiments were made, the earlier ones showing difference in the behavior of the hardened steel as compared with the cold-rolled and annealed steel, and in the amount of carbide of iron left by the oxidizing solution; and the second series were devoted to ascertaining the limits of strength of the chromic solution, within which the same percentage of carbide of iron would be obtained.

First Series.—Discs of steel, weighing about 6.5 grammes, were employed, about 2.5 in. diameter and 0.01 in. thick. Twelve were used, all of which were cut from the same strip of metal, the odd-numbered discs—1, 3, 5, etc.—being cut from one side of the axis of the strip, and the others—2, 4, 6, etc.—from the other side. The discs 1, 4, 7 and 10 were as received for the cold-rolling; Nos. 2, 5, 8 and 11 were annealed; and Nos 3, 6, 9 and 12 were hardened. The discs to be hardened were placed between two cast iron blocks, one being recessed to receive the plates, the other being quite flat. These blocks were equally heated to a bright red, a disc was then placed between them and allowed to remain till thoroughly heated; it was then instantaneously removed, and as quickly as possible caught and pressed between two cast iron surface plates. The discs to be annealed were bolted between wrought iron plates, $\frac{3}{8}$ in. thick, and so enclosed in a thin sheet iron box, 5 in. square and 2 in. deep. This was enclosed in a cast iron box about 15 in. by 6 in., and the intervening space filled up with flue dust—

thoroughly burnt soot—and the whole was then raised in an annealing furnace to a bright red heat, sufficient to scale but not peal the iron of the box. The fire was then slackened off, banked up with ashes, and the box left undisturbed in the furnace for twenty-four hours. From certain estimations made, the steel discs in contact with the wrought iron plates appear to lose carbon during the annealing. Of the annealed discs Nos. 2 and 11 were those which were in contact with the wrought iron plates. Disc No 6 was used for estimation of silicon, which was found to amount to 0.2 per cent. Total carbon was estimated in one disc of each kind, but an inside disc of the annealed series was examined to compare with those which had been annealed in immediate contact with the wrought iron. Total carbon was, as usual, estimated by decomposing the metal with cupric chloride containing sodium chloride. The filtration was conducted in the combustion tube itself, so that no loss could ensue from transferring the filtering bed and the carbonaceous matter. The discs were in all cases rubbed with fine emery and cleaned with ether before being used. The total carbon was found to be:

		per cent. carbon.
Disc No. 1 (cold-rolled)	gave	1.108
" 3 (hardened)	"	1.128
" 5 (annealed, inside disc)	"	0.924
" 11 (annealed, outside disc)	"	0.860

An estimation of the so-called uncombined carbon in three of the discs was made by generally heating them with hydrochloric acid of specific gravity 1.10. The annealed and cold-rolled discs dissolved much more rapidly than the hardened disc, the cold rolled disc furnishing the largest amount of dark-colored residue. The residue, collected on asbestos in combustion tubes, washed successively with water, alcohol, ether and water, were dried, and the carbon estimated by combustion:

		per cent. carbon.
Disc No. 7 (cold-rolled)	gave	0.096
" (annealed, inside disc)	"	0.052
" (hardened)	"	0.935

Of the remaining four discs, three were submitted to the action of an oxidizing solution—potassium bichromate with sulphuric acid—made by adding to cold concentrated solution of bichromate one-twentieth of its volume of concentrated sulphuric acid. The discs were placed on sieves of platinum gauze in the center of 500 cubic cm. of the liquid with the following result: No. 4 disc (cold-rolled) solution of metal began at once with rise of temperature, and a very slight evolution of gas. Black particles in small quantity remained at the end of five days on the sieve, they were attracted by the magnet and appeared spangly under the microscope. No. 2 disc—annealed—solution did not commence till after the lapse of five hours; afterwards solution proceeded slowly; scaly residue left on the sieve resembled the above. No. 12 disc—hardened—metal at once attacked with considerable evolution of gas. At the end of five days a little buff-colored matter remained on the sieve, as well as spangles; the light-colored matter was probably silica. The residues which remained on the sieve were placed in the liquid for a further thirteen days. Finally collected and washed, dried, and burnt in oxygen as usual; the iron estimated after the experiment.

The following quantities, calculated on 100 parts of the discs, were found:

	Carbon. Per cent.	Iron. Per cent.
No. 4 (cold-rolled)	1.039	5.87
No. 2 (annealed, out-side disc)	0.830	4.74
No. 12 (hardened)	0.178	0.70

It will be seen that very nearly the whole of the carbon for the cold-rolled disc is left in the form of a carbon-iron compound; and from the annealed disc still more nearly the whole of the carbon. Thus:

	Total carbon. Per cent.	Carbon in residue from chromic treatment. Per cent.
No. 1 disc (cold-rolled)	1.108	—
No. 4 " "	—	1.039
No. 11 (annealed, outside disc)	0.860	—
No. 2 " " "	—	0.830

On the other hand, only about one-sixth of the total carbon of the hardened disc was left in the solid residue of the chromic treatment. In the latter case, too, the ratio of carbon to iron in the residue was greater than in the residue for the other two discs, thus:

	Carbon.	Iron.
No. 4 (cold-rolled)	1	5.64
No. 2 (annealed)	1	5.72
No. 12 (hardened)	1	3.93

It is interesting to observe that in the case of the annealed and cold-rolled discs the ratios correspond very closely; they also correspond closely with the proportion of the elements of the iron carbide having the formula $Fe_6\ C_5$. The last disc was used to see whether the iron carbide would resist the action of a chromic acid solution containing a large excess of sulphuric acid. There was left 0.84 per cent. of carbon and 1.104 per cent. of iron per 100 of disc. With this large excess of acid the carbide broke down.

Second Series.—The strength of the solution of chromic acid and the amount of sulphuric mixed with it was varied, and the different kinds are described as Preparations 1, 2, 3 and 4. From these points it was desired to ascertain whether its composition is independent, with rather wide limits, of the strength of the chromic solution employed; whether, within these limits, a constant quantity of carbide is obtained per 100 of steel; and how much of the carbon of this carbide, upon treatment with hot hydrochloric acid, would remain unconverted into hydrocarbons. The chromic acid solution used may be conveniently referred to that used for Preparation 2, containing 99 grammes of salt per 1,000 cub. cent. solution, sulphuric acid being added in the proportion of 0.9 gramme of acid to 1 gramme of the bichromate. The solution used in obtaining Preparation 1 was a little weaker, being 0.8th strength of the solution of preparation 2. Preparation 3 was produced with a much weaker chromic solution, its strength being 0.44. For Preparation 4 a hot solution of bichromate was mixed with the requisite quality of sulphuric acid, and the strength aimed at was double that of Preparation 2. The mode of treatment with the chromic solution was in all instances alike, and the experiments were made at ordinary laboratory temperatures.

Preparation 1.—Four pieces of the steel—from 7 to 7.5 grammes each—were exposed in separate vessels to the chromic acid solution; 1,000 cub. cent. to each piece of steel. At the end of two days there only remained small quantities of a black grey powder which was worked off into the liquid and exposed for several days to the action of the chromic solution, and were then collected together and treated with some fresh chromic acid. In a similar manner other portions of steel were treated with Preparations 2, 3 and 4, which led to the following results:

	Preparation 1.	Preparation 2.	Preparation 3.	Preparation 4.
Carbide obtained per 100 of steel	13.25	14.16	153.4	4.66
Composition per 100 of carbide				where 400 is Fe_3 C.
Carbon	7.31	7.21	6.84	11.77
Iron	90.42	90.64	91.50	80.57
Water	2.37	2.27	1.63	5.57
Atomic ratio	$Fe_{2\cdot65}\ C_1$	$Fe_{2\cdot694}\ C_1$	$Fe_{2\cdot867}\ C_1$	—
Parts of carbon obtained in form of carbide per 100 of steel	0.969	1.021	1.049	0.266
Parts of carbon unconverted into hydrocarbons by treatment of carbide with H Cl:				
Per 100 of carbide	$\begin{cases}1.410\\1.238\end{cases}$	1.269	0.836	
Per 100 of carbon in carbide	$\begin{cases}20.87\\16.93\end{cases}$	17.60	12.22	

An examination of the foregoing results suggests the following conclusions:

1. The two chromic solutions used for Preparations 1 and 2 gave very similar results both in respect of the percentage of product obtained from the steel and of the percentage composition of the product. The third, a much weaker solution, furnishes results which, allowance being made for the smaller quantities of substance dealt with and inherent analytical difficulties, must be regarded as

closely resembling those obtained with the other two solutions.

2. The results obtained with Preparation 4, the strongest chromic solution, indicate that the limit of concentration of the oxidizing solution which the separated carbide is capable of resisting has here been exceeded. Not only has there been in this case a very considerable loss of carbon as hydrocarbons—or possibly as a soluble product of oxidation—but the iron in the separated carbide has also been to a considerable extent attacked, and only a relatively small proportion of the carbide remains, together with separated carbon, the latter partly in a hydrated form, and possibly also in some partially oxidized insoluble form.

3. The small amounts of water obtained by the combustion of Preparations 1, 2 and 3 may indicate that, in these also, small quantities of carbohydrate are present with the iron carbide. This may result from the action of the chromic solutions on the carbide first separated, and may account for the not very definite, though on the whole uniform, atomic ratio of iron to carbon in the products of Preparations 1, 2 and 3.

4. If the carbon, unconverted into hydrocarbons by treatment of the products with hydrochloric acid, be deducted from the percentage of total carbon in the products of Preparations 1, 2 and 3, the results exhibit a uniformity which, if accidental, is somewhat remarkable. Thus:

	Preparation 1.	Preparation 2.	Preparation 3.
Percentage of total carbon.........	7.31	7.21	6.84
Less carbon unconverted into hydrocarbons.....	1.38	1.27	0.84
There remains of carbon per cent.	5.93	5.94	6.00

The atomic ratio of this residual percentage of carbon to iron is as 1 to 3.270 of iron.

5. The carbon separated in the solid form as carbide and carbohydrate more nearly approaches the total amount (1.144 per cent.) of carbon contained in the steel in the case of No. 3, when the weakest chromic solution was employed—a result which was anticipated.

These results serve to confirm the view that the carbon of cold-rolled steel exists, not simply diffused mechanically through the mass of the steel, but in the form of an iron carbide, a definite product capable of resisting the oxidizing effect of an agent which exerts a rapid solvent action upon the iron through which the carbide is distributed. It is to be hoped that opportunity may be found to continue these experiments with unfused cementation steel, and with other ingot steels in the same and in different conditions of temper, using the weakest chromic solution which gave the most favorable results.

MOTION ON THE SURFACE OF A SPHEROID.

By J. E. HENDRICKS.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

As several articles have been published in this Magazine during the past year, in which the deflecting force of the surface of a revolving spheroid, as the earth, for instance, has been considered, and as the results obtained by the respective writers differ, and mine, at least, is defective and incorrect, I beg leave to substitute for my papers, published in the October and January Nos., the following proposition and discussion:

Proposition:—"When a body rotates about an axis, it is in consequence of this rotation, simultaneously rotating about any other axis which passes through the same point, with an angle of rotation which is represented by the projection upon this new axis of the line which represents the original angle of rotation." (See Peirce's Analytical Mechanics, Sect. 25).

Let P represent the point where the new axis pierces the surface of the spheroid, and let λ denote the latitude of P; then, by the above proposition, the tangent plane will rotate about P with an

angular velocity $= V \div R \sin\lambda$. Hence a body moving over the point P, in any direction, with a velocity v, will, by the rotation of the tangent plane, be constrained to describe in space the spiral of Archimedes, whose equation is $u = a\,\theta$; and, when $\theta = 2\pi$, $u = v$ multiplied by the time occupied by the tangent plane in making one complete rotation about P, $= 24\,v \div \sin\lambda$; therefore $a = 24\,v \div 2\,\pi\,sin\,\lambda$, and $\frac{1}{2}a = 6v \div \pi\,sin\,\lambda$.

Now the deflecting force at P is represented by the centrifugal force due the velocity v at the origin of the spiral, which is equal to v^2 divided by the radius of curvature at P; but the radius of curvature at the origin of the spiral of Archimedes is known to be $\frac{1}{2}a$, hence

$$f = v^2 \div \tfrac{1}{2}a = \tfrac{1}{6}\,v\,\pi\,\sin\lambda.$$

Because $V = \frac{1}{12}\pi R$, and $V^2 \div R = \frac{1}{289}\,mg$,

$$\therefore\ f : \tfrac{1}{289}mg :: \tfrac{1}{6}\,v\pi\sin\lambda : V^2 \div R,$$

whence, substituting for V^2, we get

$$f = \frac{\frac{1}{289}mg24\,v\sin\lambda}{\pi R}.$$

No account is taken here of the centrifugal force, resulting from the relative motion of the body about the earth's polar axis, as was done in my former papers, for the reason that this force affects only the position of the origin of the spiral, and not at all the elements of the spiral, and, therefore, not at all the deflecting force at P.

If we suppose the velocity v to be 60 ft. per second, which is equivalent to about 40 miles per hour, and for $\lambda = 45°$ and $R = 3956$ miles, we get

$$f = \tfrac{1}{5188}\,mg,$$

that is, the lateral pressure will be $\frac{1}{5188}$ of the weight of the moving body.

This result agrees with that obtained by Mr. Ferrell in the *Math. Monthly.*, Vol. II., p. 380, but is obtained by a more simple analysis than that employed by Mr. Ferrell.

FIRE PROTECTION IN THE DRY GOODS DISTRICT.

By FRANCIS B. STEVENS.

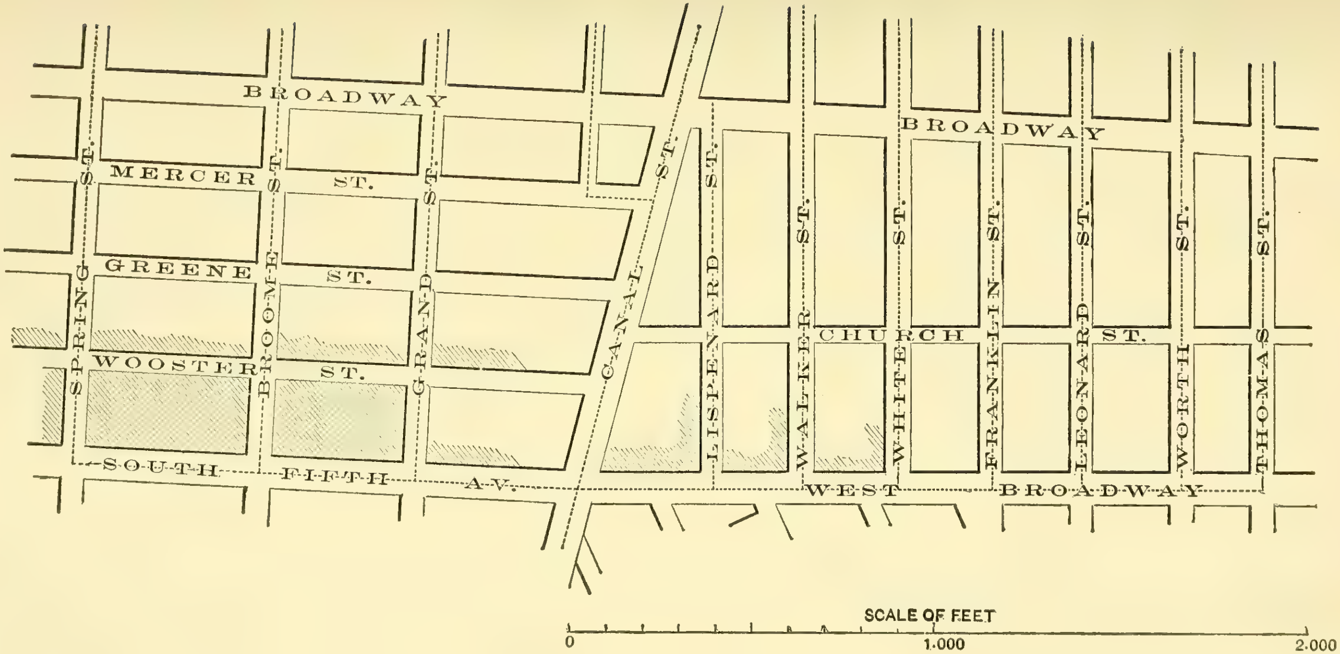
Contributed to Van Nostrand's Engineering Magazine.

The *Herald* has thoroughly shown the great danger threatening the City of New York from what has aptly been termed "the coming conflagration." Notably, its editorial of August 28th sums up the risk, and the consequences of a great conflagration in the dry goods district

The list of uncontrollable fires in cities is a long one, but is so, for the very obvious reason that improvements in the methods of extinguishing fires have never kept pace with increased difficulties and dangers to be met.

By the great fire in London in 1666, the number of dwelling-houses destroyed, exclusive of other buildings, was 13,200. These houses were low, built generally of wood, or of wooden timbers filled in with brick; and the only means then relied on to extinguish fires were water buckets passed from hand to hand. This fire led, almost immediately afterwards, to the introduction of fire engines and leather hose from Holland.

By the great fire in New York in 1835, six hundred and seventy-four buildings were destroyed. This fire occurred in intensely cold weather, accompanied by a high wind, and the means relied on to subdue it were a succession of hand fire engines extending from the river to the fire; one engine pumping into the other, and the last one delivering a feeble and intermitting stream. These engines did not produce the slightest effect on the fire, which, fanned by a furious northwest wind swept everything before it—broadening as it progressed—until it reached the East River. The destruction of some of the buildings by gunpower did not arrest the fire, but merely restrained it in a limited degree from spreading laterally. The causes assigned for the destruction of three hundred and two buildings in a portion of the same district—in fair weather in the summer of 1845, by a fire spread by an explosion—were the deficiency in the capacity of the water pipes, and the want of a sufficient number of proper hydrants.



THE DRY GOODS DISTRICT.

The dotted lines show the water pipes. The Mains are led from the river at West Street, through Canal Street to its intersection with West Broadway and South Fifth Avenue; and through these two streets to the distance shown.

The Branch Pipes run from these Mains through each of the cross streets. shown.

The Fire Department of New York, with its present efficiency, system, and appliances, could certainly have controlled and arrested the fires of 1832 and 1845; for the buildings were low and in great part altered from dwelling-houses; but the experience of the Boston fire shows that the means at present available in New York—although its fire department is probably the best in the world—would be insufficient to control with any certainty a great fire in the dry goods district, even with an independent supply of water from the Central Park Reservoir. The completion of the new aqueduct cannot alter the case materially, as far as this district is concerned, for the Reservoir now contains many times the quantity of water that could be used on any fire.

The danger threatening the dry goods district arises from the great height of the buildings, combined with the great quantity of combustible material stored in them up to this great height; and combined also with the great number of these buildings—occupying about one hundred acres—and their contiguity, front and rear.

It has been held, that it would be impossible to arrest a great conflagration in the dry goods district excepting by the use of explosives; but this cannot be taken for granted, on the contrary it may be that the opinion of engineers best qualified to judge, would be against relying on the use of explosives to isolate a fire by the destruction of a sufficient number of buildings; and that with the modern improvements and appliances, water would be the better means to rely on. Since the Chicago and Boston fires, a great advance has been made in fire protection, both by the improvements in the methods and appliances, and by the treatment of the subject by able engineers. Prominent among the improvements are those in the application and extension of the stationary system by which water under pressure is delivered directly on a fire, without the intervention of movable engines—a system that in its most simple form, the application of water led from a reservoir placed on an elevation, was in use by the Romans. The introduction of the steam pumping engine, and its many improvements, has made this system applicable to any locality, and has given the means of increasing both the volume and the pressure of the water used to any extent. The difficulty of making a complete application of this system arises from the fact, that a pressure sufficient for the purpose would be too great for the strength of the existing water mains and plumbing work, and also for general convenient use. But while this objection, on account of the expense involved, would preclude its general adoption in New York—excepting, perhaps, in the far future—it may be found not unwarrantably expensive to apply it to certain parts of the city, where the risk of an uncontrollable fire is greatest.

The following plan, based on the stationary system, is submitted in an endeavor to show that the risk of a great conflagration, either originating or spreading in the dry goods district, could be made as small as in any other part of the city, at a cost within the repaying limit.

The objects sought are, 1st: That in the case of an ordinary fire, there shall always be a certainty of having water in sufficient quantity and under sufficient pressure, ready for instantaneous use; and that the time elapsing between the alarm and the application of water shall be reduced to the lowest possible limit; so that the damage done by fire and water may both be diminished; 2d, that by the machinery kept always in reliable working order for effecting the first object, additional means be added; by which a great fire either originating in the district or threatening from without, may be controlled or repelled.

The map annexed shows the district. It occupies compactly the whole space shown, with the exception of a small portion on West Broadway and on South Fifth avenue, shown by shaded lines; and extends in length from the center of the block between Duane and Thomas streets to Spring street, a distance of about 3,350 feet; and in breadth supposing it to reach 300 feet east of Broadway, about 1,300 feet, containing including the streets about 100 acres. The highest point is at the intersection of Leonard street and Broadway, which on the street level is 35 feet above high-water mark; and the lowest point is at Canal street and West Broadway, and is 7 feet above high-water mark. The po-

sition of the water mains and branch pipes are shown by the dotted lines.

These mains and pipes are to form an independent system, connected to steam pumps, located on West street, at any available place within three or four hundred feet from Canal street. The water is to be taken directly from the river, whenever it may be necessary to do so; and a connection is also to be made with the Croton pipes in Washington street, that fresh water may be used if desirable, and if only a limited quantity is wanted. The maximum pressure at the pumps available in emergencies, is to be 173 lbs. per square inch, equivalent to a head of 400 feet of fresh water; and for ordinary use a lower pressure—about equal to that generally kept on the steamer engines—is to be always maintained in the pipes and at the hydrants, and subject to regulation there. This pressure is to be increased immediately at the pumps when necessary, up to the maximum pressure. The maximum quantity of water delivered, is to be 12,700 gallons a minute, equal to the quantity that would be delivered from forty-eight steamer engines; supposing each to deliver continuously, and without intermission, 265 gallons a minute. This maximum quantity of 12,700 gallons a minute is to be always available for use within four minutes after call; and the quantity kept available at any hydrant in the district for instantaneous use under a pressure of about 100 lbs. use is to be equal to that delivered by four steamers.

The water mains are to be 24 inches in diameter, duplicated, and to run in pairs from the pumps in West street to West Broadway; thence south through West Broadway to Thomas street, and north through South Fifth avenue to Spring street. The branch pipes are to be 20 inches in diameter, and are to run, duplicated in pairs, from these mains through each of the eleven cross streets shown on the map, to a distance of 300 feet east of Broadway; and at every intersection of two streets throughout the whole district, are to connect by pipes 8 inches in diameter with four hydrants—one being at or near the corner of each street. The branch pipes are also to connect with additional hydrants placed in the cross streets at a distance of 100 feet apart. All the hydrants are to have four nozzles for hose delivery.

By the means described, no spot in the district would be farther than 300 feet from 12 hydrants, capable of delivering the whole maximum quantity of 12,700 gallons a minute, and under a pressure —if necessary—of 173 lbs, a square inch. And as the greatest distance of any building from a hydrant in the eleven cross streets would not be over 50 feet—in the three longitudinal streets below Canal street would not be over 100 feet, and in the five longitudinal streets above Canal street would not be over 220 feet; the interval of time elapsing between an alarm and the certain application of water, could be reduced to the lowest limit, supposing the management to be equal in discipline and efficiency to that now maintained by the Fire Department.

But these means—powerful as they are—would not be sufficient to depend on in case of a great fire in the district, originating either within it or outside of it, if spread by a combination of accidents, and fanned by a high wind.

In such a case, water could not be thrown from the street level to the necessary distance from the leeward; and on account of the narrowness of the streets and the height of the buildings, it could not be directed to cover any given point off the direct line of the streets.

The means proposed to control such a conflagration are, that at or near the intersection of every two streets, two wrought iron stand pipes, each 8 inches in diameter, are to be placed, and are to run upwards on the front of one of each of two corner buildings to its top. These pipes, similar in construction and position, but larger than those now common for fire protection on the fronts of many buildings, are each to connect with one of the 20-inch branch pipes, and each are to terminate in a play stationary pipe, arranged to be pointed in any direction, having a nozzle three inches and three-eighths in diameter, and estimated to throw, from an elevation eighty feet above the street level, with an available head of two hundred and twenty-eight feet, 3,175 gallons of water a minute, to a horizontal distance—in calm weather—of four hundred and four feet. The especial use of such heavy streams would be to arrest from the leeward, either directly or obliquely,

a great fire urged onward by a high wind, and although the horizontal distance reached against a high wind would be very much less than four hundred and four feet, it is submitted that the streams from the elevation of their delivery, from their pressure, and from their size and weight, would be sufficient to arrest any fire. No spot in the district would be distant over 270 feet from four nozzles, throwing unitedly 12,700 gallons a minute, and the district, by the plan described, would be fenced around its whole circumference by stand pipes arranged that the streams from four of them would be available to repel at any point, a fire originating outside of the district. Of course, such streams should not be thrown excepting in a case of extreme necessity, smaller ones could be used when sufficient.

The management of using the water should be placed absolutely under the control of the Chief of the Fire Department, that the stationary system might not work in conflict with the present one.

Estimated Cost.

Pipes and hydrants..................	$360,000
Engine house, engines, pumps and boilers..........................	140,000
Eighty stand pipes and attachments..	60,000
Contingencies.....................	60,000
	$620,000

Maintenance.

Yearly cost of attendance, fuel and repairs..............................	$75,000

A fair time for executing the work would be five months. The steam pumps and pipes are of merchantable sizes, and the boilers could be made within three months.

The 24-inch mains and the 20-inch branch pipes are of sufficient diameter to allow the plan to be extended by continuing the mains from Thomas street, through West Broadway, Church, Morris and Whitehall streets, to the foot of the latter street; and by running branch pipes from these mains to the eastward of Broadway and to the westward of West Broadway.

The plan proposed may be criticised for overdoing the matter, for having available for use more water and greater pressure than necessary, for the large size of the pipes, for the nearness of the hydrants, and for the use of streams of the exaggerated size of three inches and three-eighths in diameter from elevated stand pipes. The answer is, that the case to be provided for is exceptional, both as to the amount involved and to the impending danger, and history of great fires teaches, that the means employed should be in excess rather than deficient.

Any plan that would accomplish the object, must be far in advance of existing methods, and as it would be applicable only to a particular districts where extraordinary precautionary measures should be taken, it is unlikely that it could be carried into effect unless by an organization of the owners of the district to be protected, aided by the insurance and acting under legislative authority.

Memoranda of Detail and of Estimates

ENGINE HOUSE.

The engine house is to be placed on any available lot about 25 feet front, and 100 feet deep, on West street near Canal street. The building to be of brick, plain, two stories in height, and without a basement or partition.

The lower story is to be on the street level.

SUCTION PIPES.

From a well sunk in the engine house to a depth of 13 feet below high water mark, two suction pipes, each 24 inches in diameter, are to run side by side to the face of the bulkhead, a distance of 200 feet, and are to be continued at the same level, suspended under the pier selected, nearly to its end, a distance of about four or five hundred feet, for the purpose of obtaining clear water, and are to terminate in a strainer capable of being raised for examination. Two suction pipes, each 10 inches in diameter, and each with water gates, are also to lead from the rear of the engine house and to connect with the Croton main in Washington street.

DELIVERY PIPES.

The 24-inch delivery mains are to be 1.04 inches thick, and the 20 branch pipes are to be 0.92 in. thick. The pair of 20-in. branch pipes, running through each of the cross streets are to be connected at their extremities—300 feet east of Broadway—by a semicircular bend pipe 10 inches in diameter, that the water may be

forced through one pipe of the pair of mains, and returned through the other pipe; so that a circulation may be maintained in winter or when necessary. In each of these connecting pipes a 10-inch water gate is to be placed, so that one pipe of the pair throughout the whole system may be always available for use, in case the water should be withdrawn from the other pipe for repairs, or to clean or to paint the interior surface.

No other water gates than the 10-inch ones mentioned are to be placed anywhere on the lines of mains and branch pipes.

The mains are to be emptied and the suction pipes are to be flushed, by connecting in the engine house, each 24-inch main and suction pipe by a 10-inch pipe having a 10-inch water gate.

HYDRANTS.

The hydrants, 300 in number, are to be connected alternately to one of each of the pair of 20 branch pipes by an eight-inch supply pipe. Each hydrant is to have a main water gate at its bottom, and an independent water gate at each of the four nozzles for hose delivery.

STEAM PUMPS AND ATTACHMENTS.

To be eight direct-acting horizontal steam pumps, such as are now sold here by at least four reliable makers.

Each pump to be capable of delivering 1,588 gallons of water, under a head of 400 feet. These engines are to be placed on the first floor of the engine house at about the street level. The eight pumps are to be divided into sets of four pumps each, and each pump, in each one of the two sets, is to be connected in the engine house to one of each of the pair of 24-inch suction pipes by a 12-inch pipe having a 12-inch water gate, and to one of each of the pair of 24 delivery pipes by a 10 pipe having a 10-inch water gate. A relief valve with an area of 200 square inches and loaded to 173 pounds to the square inch, is to be placed on each of the 24-inch delivery pipes in the engine house, and each is to be connected to a 12-inch pipe discharging into the river at the face of the bulkhead, to prevent over-pressure in the pipes. Also, when a circulation is to be maintained in the pipes, one of these valves is to be raised by a hand attachment to allow for the discharge.

BOILERS.

To be eight locomotive boilers, of the consolidation pattern, each 25 feet long, and 54 inches in diameter, and each to have its grate surface enlarged to measure 36 square feet. These boilers are to be placed on the second floor of the engine house. The fires are to be urged by the contraction of the blast to the same extent as in locomotives. Steam is to be held on all of the boilers day and night, by means of the fire kept in the furnace of one; the fire boxes of the other seven being filled with kiln—dried wood ready for instantaneous ignition; the kindling being aided by a fan blast. The net horse power required to deliver 12,700 gallons of water a minute, under a pressure of 173 lbs. per square inch, is 1280; and these boilers would be ample for that service. Fresh water should be used in the boilers.

ESTIMATE OF AVAILABLE HEAD.

The pressure at the pumps being maintained at 173 lbs. per square inch, equivalent to a head of 400 feet of fresh water, the head on the street level at the highest point of the district, viz., at the intersection of Broadway and Leonard street would be 31 feet less, or 369 feet; and the head at the top of the eight-inch stand pipes, supposing the water at rest and no losses to occur by its movement, would be 369 feet, less the height of the stand pipe—80 feet—equal to 289 feet.

The losses of head resulting from the motion of the water in delivering 12,700 gallons a minute through the mains, branch pipes and four stand pipes, would be as follows:

	Ft.
Loss by friction in the two 24-inch mains, each 4,000 feet long	16.20
Loss by friction in the two 20-inch branch pipes, each 1,000 feet long	10.10
Loss by friction in the four 8-inch stand pipes, each 110 feet long	27.10
Loss at the intersections of the 24 and 20-inch pipes	0.65
Loss at the intersections of the 20 and 8-inch pipes	6.35
Loss at the 8-inch bends	0.60
Total loss	61.00

Deducting this from the static head of 289 feet, there remains 228 feet available

head at four nozzles—each three and three-eighths inches diameter—in the highest part of the district, estimated to throw the water a distance of four hundred and four feet.

THE USE OF FRESH WATER.

The damage done by salt water over that done by fresh water, in extinguishing a fire, cannot be very great.

Salt water was exclusively used in New York up to the year 1842; and it is believed that the introduction of fresh water made no marked change in the damage by water. But if it should be deemed advisable to use fresh water for an ordinary fire, the Croton main in Washington street would be sufficient for that purpose. In the case of a great fire the difference in the damage, by salt over fresh water, would be too small for computation in comparison with the loss of property. To bring the maximum quantity of 12,700 gallons a minute from the Central Park Reservoir would require an independent main 36 inches in diameter, costing over $300,000, without allowance for excavation in rock. A smaller pipe would not answer, for the whole head of the Reservoir above high water mark of 119 feet would be absorbed by the frictional resistance of 12,700 gallons of water a minute, led in a pipe 30 and 3-4ths inches in diameter, and 24,500 feet—the estimated distance—long.

LOCATION OF ENGINE HOUSE.

If no available lot for the engine house could be had near Canal street, any lot on West street, between Duane and Spring street, would answer; and the mains could be led thence directly to West Broadway.

DETAILED ESTIMATED COST.

THE 24-INCH PIPES.

Under the pier, 500 feet long; in Canal Street, 2,600 feet long; in West Broadway, 3,400 feet long; and this duplicated is 13,000 feet for the pair, which, at $5.70 a foot, including manholes and outlets, and at $1.80 a foot for laying, is..............	$97,500
Additional contingent cost for laying the suction pipes across West Street	13,500
Strainers and attachments at the ends of the suction pipes...............	2,000
Total........................	$113,000

THE 20-INCH PIPES.

The total length in the eleven cross streets is 15,000 feet, which duplicated, and taken at $4.50 a foot for pipes and $1.50 feet for laying, is..	$180,000
Cost of mains and branch pipes	$293,000

THE OTHER PIPES.

Two 12-inch relief pipes, with relief valves........................	4,000
Two 10-inch pipes connecting with the Croton......................	1,500
In the engine house, eight 12-inch suction pipes, eight 10-inch delivery pipes, and two 10-inch pipes connecting suction and delivery for flushing........................	3,500
Connecting the branch pipes and hydrants, three hundred 8-inch pipes, each 30 feet long................	6,000
Laying and connecting them........	5,000
Cost of all water pipes........	$313,000

WATER GATES.

In the engine house, ten 12-inch and fourteen 10-inch water gates; and at the terminations of the branch pipes, eleven 10-inch water gates	$3,000

HYDRANTS.

Three hundred hydrants............	44,000
Cost of pipes and hydrants ...	$360,000

ENGINE HOUSE, STEAM PUMPS AND BOILERS.

Land, 25 feet by 100 feet............	$25,000
Brick building......................	38,000
Eight horizontal direct-acting steam pumps, each to deliver 1,588 gallons a minute, at $2,500 each..........	20,000
Connecting, and cost of steam pipes, feed pumps, valves, gauges, and tools	18,500
Eight locomotive boilers, [consolidation pattern.........................	25,000
Main chimney, eight feet in diameter and 80 feet high, and eight connecting chimneys, all of No. 8 iron	3,500
Boiler attachments..................	10,000
Total........................	$140,000

STAND PIPES.

Eighty 8-inch stand pipes, 85 feet long made of steam pipe, at one dollar and thirteen cents a foot net, costing each............................	$96
Erecting each......................	54
Play-pipes, with nozzles three and three eighths inches in diameter, and attachments for direction, costing each	150
Connecting each stand-pipe and the 20-inch branch pipe....	100
Two water gates for each pipe, one below the surface of the ground, and the other at the nozzle, at $50.00 each	100
Cost of each pipe and nozzle.........	$500

Add to each pipe for contingencies....	$250
Cost of the eighty pipes erected complete, with nozzles................	$60,000

YEARLY EXPENSE FOR MAINTENANCE.

Pay roll of 15 men stationed night and day in the engine house...........	$20,000
Coal to keep steam constantly on the boilers, night and day, at three tons of anthracite a day.................	6,000
Fuel for extinguishing fires, estimated at the maximum at 200 tons of Cannel coal and 500 cords of pine wood.	5,000
Repairs..............................	10,000
Contingencies........................	34,000
Total........................	$75,000

RECAPITULATION.

Pipes and hydrants.................	$360,000
Engine house and pumps............	140,000
Stand pipes........................	60,000
Contingencies......................	60,000
	$620,000
Maintenance per annum.............	$75,000

ON THE PHYSICAL CONDITION OF IRON AND STEEL.

By PROF. D. E. HUGHES, F. R. S.

A Paper read before the Institution of Mechanical Engineers.

In a paper read before the Royal Society, May 5th, 1879, entitled "On an Induction Currents Balance, and experimental researches made therewith," the author showed that this instrument was extremely sensitive to all molecular changes in metallic bodies. Finding that its powers were remarkably suitable for researches upon the molecular change which takes place in iron and steel when tempered, he made with it a series of researches to determine the cause of tempering in steel. The results of these the author laid before the Institution of Mechanical Engineers (Proc. 1883, p. 72) in a paper "On the Molecular Rigidity of Tempered Steel." In this paper the author advanced the theory that the molecules of soft iron were comparatively free as regards motion amongst themselves, whilst in hard iron or steel they were extremely rigid in their relative positions.

The author has since widened the field of research so as to embrace all the physical changes which occur in iron and steel through chemical alloys, mechanical compression or other strains, annealing, and tempering. The results of these researches he now embodies in the present paper. Believing it necessary that we should be able to tell the physical state of any piece of iron, without destroying or changing that state, the author has sought for and tried several methods, which gave any hope of success in this direction. The physical state of iron has a marked influence upon its electrical conductivity. The differences thus indicated, however, are not wide enough to be appreciated except with metal in the form of wire; and in order to perceive small changes, such as small differences of temper, we should require a wire at least 250 yards in length. The author has found, however, that by the application of certain phenomena belonging to magnetism we are enabled to perceive clearly the slightest change in the molecular structure of iron or steel, through all degrees of annealing to the finest differences in tempering, and this with pieces of any form or dimensions.

It is already known that soft iron will take a higher degree of magnetism, and retain it less, than steel; and that tempered steel retains magnetism more than soft steel. Consequently we might expect, that by the aid of an instrument which could give correct measurement of degrees of magnetism, we should be able to include all varieties of iron and steel, between the two extremes of softness as in annealed iron, and hardness as in highly tempered cast steel. The author soon found that this was not the case when pieces of iron were magnetized to saturation, or even partially so.

In a recent paper upon the theory of magnetism* the author said: "During these researches I have remarked a peculiar property of magnetism, viz., that not only can the molecules be rotated through any degree of arc to its maximum, or saturation, but that, whilst it requires a comparatively strong force to overcome

* Society of Telegraph Engineers, May 24th, 1883.

its rigidity or resistance to rotation, it has a small field of its own through which it can move with excessive freedom, trembling, vibrating, or rotating through small arcs with infinitely less force than would be required to rotate it permanently on either side. This property is so marked and general that we can observe it without any special iron or apparatus."

The author has found, by employing extremely feeble magnetizing powers, such as a weak current of electricity only just sufficient for measurement (or the current from one Daniell cell reduced, as found best for the dimensions of the iron, by passing it through resistance-coils varying from 10 to 100 ohms), that the following laws hold with every variety of iron and steel:—

1. The magnetic capacity is directly proportional to the softness or molecular freedom.

2. The resistance to a feeble external magnetizing force is directly as the hardness, or molecular rigidity.

The author has proved this to be the case with sixty different varieties of iron and steel furnished direct from the manufacturers. And he has found that each variety of iron or steel has fixed points, beyond which annealing cannot soften, nor tempering harden; consequently, if all varieties were equally and perfectly annealed, each variety would have its own magnetic capacity, or its specific dedegree of value, by means of which we could at once determine its place and quality.

If in place of several varieties we take a single specimen, say hard-drawn Swedish iron wire, and note its magnetic capacity, we find that its value rises rapidly with each partial annealing, until an ultimate softness is obtained, being the limit of its molecular freedom. We are thus enabled to study the best methods of annealing, and to find at once the degree of softness in an unknown specimen.

Similarly, when we temper annealed iron and steel, we find that we can follow out each degree of temper up to ultimate molecular rigidity; and we may thus appreciate in an unknown specimen of unknown temper the degree of its hardness.

We have thus in each piece of iron or steel a limit of softness and hardness. In soft Swedish iron, tempering hardens but 25 per cent. on the scale adopted, whilst mechanical compression, such as hammering, hardens it 50 per cent. In cast steel, tempering hardens it 400 per cent., whilst mechanical compression gives but 50 per cent. Between cast steel and Swedish iron we find a long series of mild steel, hard iron, &c., varying in their proportionate degree between the two extremes just mentioned.

The theory which the author has advanced, of molecular freedom as in soft iron, and molecular rigidity as in cast steel, fully explains all the changes which we are enabled to perceive and measure; but it is not absolutely necessary to accept the theory in order to appreciate the results. For, leaving theoretical considerations aside, we have one proved fact, viz., that the magnetic power or capacity of a piece of iron, under the influence of an external limited magnetizing power, depends upon its softness; and that the retention of magnetism, when the external power is withdrawn, depends upon its hardness. The same degree of temper or annealing, upon the same iron or steel, gives invariably the same readings; but the slightest change—say from a straw-colored temper to a blue—gives very wide differences.

Description of Apparatus.

The instrument which the author has constructed and used in these experiments, and which he has named a "Magnetic Balance," consists of a delicate magnetic needle, suspended by a silk fibre; it is 5 centimetres in length, and its pointer rests near an index having a single fine black mark for its zero. The movement of the needle on either side of zero is limited to 5 millimetres by means of ivory stops or projections. When the north end of the needle and its zero index are north, the needle rests parallel with its index; but the slightest external influence, such as a piece of iron 1 millimetre in diameter placed at 10 centimetres distance deflects the needle to the right or left, according to the polarity of its magnetism, and with a force proportionate to its magnetic power. If we place on the opposite side of the needle, and at the same distance, a wire possess-

ing absolutely the same polarity, of similar name and force, the two balance each other and the needle returns to zero; and if we know the magnetic value required to balance the first piece of iron we know the magnetic value of both.

The iron, B (which may be in the form of wires, rods, bars, plates, or any shape or size desired),* is placed at a fixed distance (preferably 10 or more centimeters) resting against a fixed brass stop. The centre of the iron should be in a line with the centre of the needle, and it should be placed at right angles to the needle, lying horizontally east and west, so as to be free from the directing influence of the earth's magnetism.

The compensator, placed upon the opposite side of the needle, and at a distance of 30 centimetres, consists of a powerful steel bar-magnet, 3 centimetres width, 1 centimetre thick, and 6 centimetres long. This turns upon its axis, carrying with it the pointer to indicate its degree of angular displacement on the graduated circle. Generally this bar-magnet is parallel with the needle, the pointer of the compensator and the needle being at zero; but when we wish to measure the amount of magnetism in the piece of iron the bar-magnet is made to pass through an angular displacement necessary to balance this force, and its index readings on the graduated circle are taken as the comparative values.

In order to magnetize the iron by an electrical current, a coil of insulated copper wire is placed near the needle, the iron then becoming the core of an electro-magnet.

Now as this coil, independently of its iron, acts upon the needle, this action must be balanced by an opposing coil, G, on the opposite side. The position and power of these two coils can be adjusted by means of the lever H, which allows us to find a position where the two coils completely neutralize each other. If we introduce iron in the coils on either side, the balance is destroyed, and we have solely the magnetic influence of the iron core, whose value we find by an equal opposing magnetism brought into play by the rotating magnetic compensator.

A reversing key serves to change the direction of the current, and thus any difference between north and south polarity in the iron core can be observed. One Daniell cell is all that is required as a battery; but great care must be taken that its electromotive force is a constant, otherwise all variations in the battery would be read as variations in the quality of the iron itself; and we need in addition a series of resistance coils from 10 to 100 ohms, in order to reduce the current sufficiently to bring the whole series, from soft Swedish iron to cast steel, into range. Separate and finer determination can then be separately made by an extremely weak force for soft iron, and full or increased battery power for tempered steel. A series of different sized coils is necessary, whenever we vary greatly the diameter of the core. The first size, with an internal core-opening of one centimetre, will test bars and rods of wire, from one centimetre diameter to the finest needle; but for larger bars, plates, &c., coils must be used which allow free passage for the iron into the core. Great care and some practice is necessary in the use of the instrument, so as to ensure that the iron is placed in a neutral field; but when we have really obtained the necessary conditions we can take several readings in a single minute, with an invariable result for the same kind of iron.

All irons and steel have some traces of remaining magnetism; it is therefore necessary that a double reading (north and south) should be taken by means of reversed currents. In this case the quadrant is divided into 360° on each side of zero; and the total value of north and south polarity added together is that given in the following tables of magnetic capacity.

Several methods of observation can be employed with the magnetic balance, the usual one being that already described; but there are many others, such as magnetizing all specimens to the same value and noting the amount of current required. We may also observe the remaining magnetism after the cessation of the current; the influence of a weak current after the passage of a strong, &c. Many of these methods give interesting facts, particularly useful to those making researches upon the cause of magnetism.*

* The smallest rods yet tested have been fine sewing needles, and the largest bars of 5 centimeters diameter, 1 metre long.

* The author has not patented this instrument, giving it freely to the scientific and manufacturing world.

By means of this instrument the author has tested sixty brands of iron and steel, mostly in the form of wires. A wire one millimetre diameter and ten centimetres long was the standard size used, as we can more readily temper small wires than large rods. In all comparative experiments between iron of different grades, we must have one standard form to which all the rest must be similar in form and size. Thus we could not compare a square or flat bar with a piece of wire; but if all pieces have the same form, then any difference observed between them must be due to their comparative softness, from which we can deduce the quality and place of each on the line ranging from soft iron to cast steel.

Influence of Annealing upon the Molecular Structure of Iron and Steel.

The magnetic balance shows that annealing not only produces softness in iron, and consequent molecular freedom, but it entirely frees it from all strains previously introduced by drawing or hammering. Thus a bar of iron drawn or hammered has a peculiar structure, say a fibrous one, which gives a greater mechanical strength in one direction than another. This bar, if thoroughly annealed at high temperatures becomes homogeneous in all directions, and has no longer even traces of its previous strains, provided that there has been no actual mechanical separation into a distinct series of fibres.

Table I.

INFLUENCE OF ANNEALING UPON SWEDISH IRON, SAMPLE G.

	Approximate Temperature.		Degrees of softness indicated upon the Magnetic Balance.
	Cent.	Fahr.	
Wire hard-drawn as furnished by maker....	—	—	230°
Annealed at black heat....................	500°	950°	255°
Annealed at dull red.	700°	1300°	329°
Annealed at bright red	1000°	1800°	438°
Annealed at bright yellow.................	1100°	2000°	507°
Annealed at bright yellow white...........	1300°	2300°	525°

From Table I. we see that a regular increase of softness occurs as the temperature at which Swedish iron is annealed increases, the maximum being at a point under that of fusion.

Some difficulty was experienced in annealing all wires to the same standard. The method employed at first was to place the wires in an iron tube heated to the desired temperature; but the temperature of the tube was extremely variable, and also it was found that an interchange of carbon takes place between the tube and wires. Steel wires rapidly lose their carbon, and thus become softer at each successive annealing; whilst the purest iron absorbs carbon, until it contains exactly the same proportion as the tube itself. It is well known that iron wires at red heat, placed in a porcelain tube through which a current of carbureted hydrogen is passing, will absorb sufficient carbon to become hard steel.

Experiments regarding the time required for perfect annealing showed that whilst hard steel required several hours, soft iron might be cooled in a few minutes without losing its degree of softness; consequently, knowing the great value of high temperature, the author adopted the following method. The tube was heated to a white heat or otherwise, the iron wires to be annealed were introduced quickly, and the instant they had the same temperature, they were withdrawn and simply allowed to cool in the air. The wire employed being 1 millimetre diameter, the whole operation was complete in two minutes. This is not suggested as the best practical method of annealing, although in the case of these wires it produced the best result; but the experiments show that, whatever method is employed, the heating should be as rapid as possible to a high degree of temperature, and that the wire should cool in a completely neutral medium or atmosphere.

The facts regarding annealing, as pointed out by the measurement of the magnetic capacity of iron wires, have no doubt been in a great measure perceived by ordinary mechanical methods. The results of the author's researches may be thus formulated:—

1. The highest degree of softness in any variety of iron or steel is that obtained by a rapid heating to the highest temperature less than fusion, followed by cooling in a medium incapable of changing its chemical composition.

2. The time required for gradual cooling varies directly as the amount of carbon in alloy.

Thus in absolutely pure iron rapid cooling, as in tempering, would not harden it, whilst steel might require several hours or days, even for pieces only one millimetre diameter. Slow cooling has no injurious effect upon iron, when cooled in a neutral field: consequently, where time is no object, we may employ slow cooling in every case.

A wire or piece of iron thoroughly annealed must not be bent, stretched, hammered, or filed; the hardening effect of a bend is most remarkable, and the mere cleaning of the surface by sand-paper hardens that surface by several degrees on the scale.

The following Table shows the effect of annealing upon a series of wires, kindly furnished expressly for these experiments by Messrs Frederick Smith & Co., of Halifax.

TABLE II.

Mark.	Description.	Magnetic Capacity.	
		Bright as sent.	Annealed.
		Degrees on Scale.	Degrees on Scale.
G	Best Swedish charcoal iron, 1st variety......	230	525
F	Best Swedish charcoal iron, 2d variety......	236	510
T	Best Swedish charcoal iron, 3d variety......	275	503
S	Swedish Siemens-Martin iron...............	165	430
H	Puddled iron, best best.....................	212	340
Y′	Bessemer steel, soft........................	150	291
Y	Bessemer steel, hard.......................	115	172
Z	Crucible fine cast steel.....................	50	84

From the above Table it will be seen that annealing had a great effect on the iron wires, doubling their value, and that Swedish iron stands far in advance of puddled iron; consequently, for the cores of electro-magnets in telegraph instruments—as in fact for all electro-magnets—Swedish iron is the most suitable, and the magnetic balance may find a field of practical utility in measuring each core before it is used in an electro-magnet, and may also aid us by its measurements in finding the best methods of annealing.

TEMPERING.

The influence of tempering upon the magnetic retentivity, or molecular rigidity, has been shown in every piece of iron or steel yet examined. Swedish iron hardens but 10 to 20 per cent. by tempering, whilst cast steel hardens 300 per cent.;* the molecular rigidity of tempered steel being 18 times greater than that of soft iron. The influence of different methods of tempering on crucible steel is shown in Table III., ranging from its ultimate molecular rigidity to its ultimate softness when annealed.

* For instance, in Table IV. below, the figure for Swedish iron No. 7 annealed is 525, tempered hard 415. On the other hand, the figure for cast steel annealed is 84, tempered hard 28. The reciprocals of these figures give what may be called a scale of hardness.

TABLE III.

Crucible Fine Cast Steel Tempered.	Mark.	Magnetic Capacity.
Bright yellow heat, cooled completely in cold water..........	A	28
Yellow red heat, cooled completely in cold water.............	B	32
Bright yellow, let down in cold water to straw color.........	C	33
Bright yellow, let down in cold water to blue................	D	43
Bright yellow, cooled completely in oil....................	E	51
Bright yellow, let down in water to white....................	F	58
Red heat, cooled completely in water........................	G	66
Red heat, cooled completely in oil..........................	H	72
Annealed..	J	84

We may from this represent graphically a diagram which includes all methods of tempering; and another diagram which shall include all varieties of iron, from the softest iron to the hardest steel, intermediate qualities of hard iron and mild steel finding their place between the two extremes.

The numerous specimens of wires tested have been forwarded direct from the manufacturers, at the request of the author's friend, Mr. W.H. Preece, F. R. S., Electrician to the General Post Office. The chemical analyses of most of these wires have not been furnished; but Messrs. Frederick Smith & Co., of Halifax, not only supplied a beautiful series of wires, but had them specially analyzed by Mr. Henry S. Bell, of Sheffield, in order that the results should be as exact as it was in their power to make them. The author therefore neglects in this paper all other samples except those of Messrs. Frederick Smith & Co.: they all stand between, or are included by the two extremes, of Swedish iron and cast steel.*

Table IV. on next page gives the complete results of the mechanical, chemical, and physical tests upon these wires. The tensile strength and electric conductivity are as furnished by Messrs. Frederick Smith & Co., the chemical analyses are as given by Mr. Henry S. Bell, and the magnetic capacities of the bright hard-drawn wires, as of the annealed and tempered wires, were determined by the author with the aid of the magnetic balance.

* The author does not desire that Swedish iron should be considered as the softest of all possible irons, or tempered cast steel as the final limit of hardness. They are simply the limits found during these researches, but they may possibly be widened by a mere extended series of irons and steels.

Table IV. will aid us in drawing several conclusions. Taken in conjunction with Table III., it shows—

1st. That the degree of temper in cast steel is dependent jointly on the heat to which it is raised and the degree by which this is lowered in rapid cooling; the extremes in Table III. giving the relative molecular rigidity of the softest and hardest steel.

2nd. That a peculiar mild and homogeneous temper is obtained in oil.*

3rd. That the tempers or degrees of hardness, when steel is let down through the various colors, vary with the kind of steel tempered, as well as with the heat from which it has been let down..

In these experiments the author has noticed that the highest degree of temper has not been obtained with wires containing the relatively highest proportion of carbon. The maximum thus far was obtained with but 0.62 carbon; whilst in a series of steel wires, made expressly for these experiments, but in which the manufacturer stated only the amount of carbon, the results were as in Table V. on page 256.

It will be seen that the hardness as indicated in column "tempered" is not directly as the proportion of carbon; a marked example being the wire with 0.75 carbon, which is far softer than that with 0.62. The author might here have doubted the truth of the magnetic balance, if he had not previously verified its results by mechanical tests. In order hower to test the accuracy of the results, the wires S^5

* The author has found, by a method more complicated than here described, and by the use of the induction balance, that all tempers heretofore tried (excepting these in oil) give a steel not homogeneous; and a temper let down to straw or blue has external strains differing from those of the interior.

TABLE IV.

Brand	Quality.	Electrical resistance per mile of .040 diam.	Tensile strength per square inch.	Magnetic Capacity. Bright hard.	Magnetic Capacity. An-neal'd.	Magnetic Capacity. Tempered hard.	Chemical Analysis. Carbon.	Silicon.	Sulphur.	Phos-phorus.	Manga-nese.	Copper.	Iron.
		Ohms.	Tons.	Deg.	Deg.	Deg.							
G	Best Swedish charcoal iron, No. 1.	191.52	28	230	525	435	0.09	trace	trace	0.012	0.06	trace	99.69
F	Best Swedish charcoal iron, No. 2.	198.40	30	236	510	415	0.10	trace	0.022	0.045	0.03	trace	99.70
T	Best Swedish charcoal iron, No. 3.	199.62	31	275	503	395	0.15	0.018	0.019	0.058	0.234	trace	99.44
S	Swedish Siemens-Martin iron......	226.32	34	165	430	390	0.10	trace	0.035	0.034	0.324	trace	99.60
H	Puddled iron, best best.........	259.92	30	212	340	328	0.10	0.09	0.03	0.218	0.234	0.015	99.11
Y′	Best homogene's soft Bessemer steel	266.52	35	150	291	255	0.15	0.018	0.092	0.077	0.72	trace	98.74
Y	Best " hard Bessemer steel	312.69	50	115	172	60	0.44	0.028	0.126	0.103	1.296	trace	98.20
Z	Fine crucible cast steel..........	350.08	55	50	84	28	0.62	0.06	0.074	0.051	1.584	trace	97.41

and Z were bound together, heated together to the same temperature, and plunged together in cold water; this was repeated several times, with the invariable result that the wire Z with 0.62 carbon was glass-hard and could not be marked by a file, whilst the wire S^5 with 0.75 carbon could be easily cut by the same file. Again we notice that in Table IV. the wires T, called Soft Swedish iron, contain precisely the same amount of carbon (0.15) as those Y′ in Table V. called Bessemer soft steel: but that whilst Y′ is comparatively hard when tempered, it does not become greatly softened by annealing. This is due probably to its greater proportion of some other ingredients. Similarly the wire S is much softer than H in Table IV., both having a similar amount (0.10) of carbon. The hardness of H when annealed is probably due to its greater proportion of phosphorus or some other substance.

It may be too soon to try and correlate the physical changes occurring in tempering with the corresponding chemical analyses: but the author believes that he has shown reason to hope that we may eventually obtain, by uniting chemical with physical analysis, a more clear insight into the mysteries of iron and steel.

DIVIDING LINE BETWEEN IRON AND STEEL.

Mechanical tests, as well as chemical analyses, have failed to find any distinct line of separation between the numerous varieties of iron and steel. The physical method which the author has employed shows clearly that there is no dividing line between iron and steel. If we glance at Table IV. we see that we have a continous series from the softest iron to the hardest steel, and between them we have every variety of intermediate quality. In point of fact the sixty brands which have been tested fill up all the gaps: and by their means we could choose irons gradually hardening into steel, or steel gradually softening into iron. Thus ordinary iron is physically a soft steel, and steel a hard iron. All are hardened by temper; all are hardened by mechanical treatment, as hammering and rolling; all are hardened by strains and stresses of any nature whatever: the difference, though large, is only in degree. At the extreme end towards iron, mechanical hardening has a greater effect than tempering. At

TABLE V.

	Mark.	Magnetic Capacity.		Carbon.
		Annealed.	Tempered.	
		Degrees.	Degrees.	Per cent.
Bessemer soft steel..........	Y′	291	255	0.15
Steel made for these Experiments........	S′	348	206	0.40
" " " "	S^2	250	160	0.55
" " " "	S^3	209	133	0.60
" " " "	S^5	195	107	0.75
" " " "	S^4	144	61	0.65
Bessemer hard steel....................	Y	I72	60	0.44
Fine crucible cast steel.................	Z	84	28	0.62

the steel end, tempering has a greater effect than mechanical hardening. We might here suppose we could find a physical dividing line: but the author has found some mild steels to stand just on that dividing line, which had previously appeared the most satisfactory. We are thus forced to adopt an arbitrary line. Neither the mechanical nor physical methods will suffice to overcome the difficulty. Mechanically a certain tensile strength has been proposed—the objection to which is that unless we take note of the physical conditions (such as whether soft, tempered, &c.) we shall have very different magnetic readings for what would stand as the same material. The addition of the ultimate elongation might to some extent weaken this objection, but would not remove it. The physical method would allow us to fix upon a certain molecular rigidity, or difference in the readings of the same metal annealed and tempered, as the boundary; it would have, however, all the objection of being a purely arbitrary line. Chemical analysis also fails to show a dividing line, as the same proportion of carbon is accompanied by very different physical results, if sulphur, phosphorus, &c., are present. In the author's researches he has adopted the plan of simply reading an unknown piece of iron or steel in its annealed state: if the figure stands above 400° it is classed as iron, if below as mild or hard steel, according to its magnetic capacity. This happens to agree with the general classification at present in use, and suffices as a general division.

RELATIONS OF PHYSICAL FORCES IN IRON AND STEEL.

Iron is by far the richest of all metals in its physical nature. It stands almost alone in its magnetic qualities, as well as in its tempering properties, and, while there is an evident relation between capacity for temper and loss of magnetism when tempered,* so these experiments show an intimate if not absolute relation between the electrical conductivity of iron, and its magnetic capacity. In Table IV., in the column of electrical resistance as given by Messrs. Smith & Co., we find a progressive increase of resistance, just as we find a progressive decrease in magnetic capacity. And there is an exact correspondence between the two variations. The molecular rigidity, observed by the author as the cause of hardness, gives at once decreased magnetic capacity, and increased electrical resistance; so that from the magnetic capacity we might deduce its electrical resistance, and *vice versa*. A very remarkable phenomenon is that this only holds true in the limited sphere of elastic rotation, which the author has already described.

This demonstration the author believes to be of great theoretical value, and in a future paper, upon the theory of magnetism, its importance will be shown. In this paper the author has tried as far as possible not to bring theoretical considera-

* This is shown in Table IV. where the proportion of magnetism lost by tempering is seen to increase markedly as we pass from soft iron to hard steel.

tions forward; in the results presented we are dealing with proved facts.

Another extraordinary relation of physical to mechanical tests may be mentioned. In Table IV. the tensile strength bears no relation either to the magnetic or electric qualities. On increasing the electromotive force in the magnetic balance, all the readings became confused; there was no longer any fixed relation as to hardness, nor any other quality. But on again forcing the magnetism to a very high point, the figures for magnetic capacity were found to bear exactly the same relation to each other as those for tensile strength. This, however, may have been only an accident, as it only seems true at present in relation to the wires in Table IV.; but it gives hope that by a new method we may some day be enabled, not only to deduce electrical conductivity from magnetic capacity, but also tensile strength. Already in Table IV. we notice a close relation between molecular rigidity, as indicated by the figures for the annealed wires, and tensile strength.

Leaving aside all theoretical considerations and hoped—for improvements in the methods of observation, the author believes that he has demonstrated clearly that, by the aid of the instrument and methods described, we can at once determine the physical state of iron, as influenced by tempering and mechanical hardening, from the ultimate degree of softness to that of hardness; that we can at once determine the best iron for electro-magnets, and the best methods of softening it, as well as the best steel for permanent magnets, and the best temper to be given to it. He therefore ventures to hope that the Magnetic Balance will prove an aid of no small value in all researches into the physical state of iron and steel.

THE UNIFICATION OF LONGITUDES AND OF TIME—RESOLUTIONS OF THE INTERNATIONAL GEODETIC COMMISSION.

From "Science."

The seventh general conference of the International Geodetic Association held at Rome, and at which representatives of Great Britain, together with the directors of the principal astronomical and nautical almanacs and a delegate from the Coast and Geodetic Survey of the United States, have taken part, after having deliberated upon the unification of longitude by the adoption of a single initial meridian, and upon the unification of time by the adoption of a universal time, has agreed upon the following resolutions:—

1°. The unification of longitude and of time is desirable as much in the interest of the sciences as in that of navigation, of commerce, and of international communications. The scientific and practical utility of this reform far outweighs the sacrifice of labor and the difficulties of rearrangement which it would entail. It should, then, be recommended to the governments of all the interested states to be organized and confirmed by an international convention, to the end that hereafter one and the same system of longitudes should be employed in all institutes and geodetic bureaus, for general geographic and hydrographic charts, as well as in astronomical and nautical almanacs, with the exception of those made to preserve a local meridian; as, for instance, the almanacs for transits, or those which are needed to indicate the local time, such as the establishment of the port, etc.

2°. Notwithstanding the great advantages which the general introduction of the decimal division of a quarter of the circle in the expressions of the geographical and geodetic co-ordinates and in the corresponding time-expressions is destined to realize for the sciences and their applications, it is proper, through considerations eminently practical, to pass it by in considering the great measure of unification proposed in the first resolution.

However, with a view to give satisfaction at the same time to very serious scientific considerations, the conference

recommends, on this occasion, the extension, by the multiplication and perfection of the necessary tables, of the application of the decimal division of the quadrant; at least, for the great operations of numerical calculations for which it presents incontestable advantages, even if it is wished to preserve the old sexagesimal division for the observations, for charts, navigation, etc.

3°. The conference proposes to governments to select for the initial meridian that of Greenwich, defined by a point midway between the two pillars of the meridian instrument of the observatory of Greenwich; for the reason that that meridian fulfills, as a point of departure for lcngitudes, all the conditions wished for by science, and because, being at present the best known of all, it offers the most chances of being generally accepted.

4°. It is suitable to count the longitudes, starting from the meridian of Greenwich, in the sole direction from west to east.

5°. The conference recognizes for certain scientific wants, and for the internal service in the great administrations of routes of communication—such as the railways, steamship-lines, telegraphic and post routes—the utility of adopting a universal time, along with local or national time, which will continue necessarily to be employed in civil life.

6°. The conference recommends as the point of departure of universal time and of cosmopolitan dates, the mean noon of Greenwich, which coincides with the instant of midnight or with the commencement of the civil day, under the meridian situated twelve hours, or a hundred and eighty degrees, from Greenwich.

It is agreed to count the universal time from 0 hour to 24 hours.

7°. It is desirable that the States which, with a view to adhere to the unification of longitudes and of time, find it necessary to change their meridians, should introduce the new system of longitudes and of hours as soon as possible.

It is equally advisable that the new system should be introduced without delay in teaching.

8°. The conference hopes that, if the entire world agrees upon the unification of longitudes and of hours by accepting the meridian of Greenwich as the point of departure, Great Britain would find in this fact an additional motive to make on its part, a new step in favor of the unification of weights and measures by adhering to the *Convention du metre* of the 20th of May, 1875.

9°. These resolutions will be brought to the knowledge of the governments, and recommended to their favorable consideration, with an expression of a hope that an international convention—such as the government of the United States has proposed—for confirming the unification of longitudes and of time should be decided upon as soon as possible.

REPORTS OF ENGINEERING SOCIETIES.

American Society of Civil Engineers.—The last issue of the transactions contains:

No. 266. The cost of steam power, by Chas. E. Emery.

No. 267. The Shubenacadis Canal, Edward H. Keating.

No. 268. The Nasmyth Pile-driver, Don J. Whittemore.

No. 269. The effect of passing trains on iron bridges, masonery, etc., James L. Randolph.

No. 270. An economical and efficient railroad bridge floor, W. Howard White.

Engineers' Club of Philadelphia.—Regular meeting January 19th, 1884.

Col. William Ludlow, President, in the chair.

After calling the meeting to order, President Ludlow said:

"I deeply regret, gentlemen, that almost my first official duty as your President is to appear as a harbinger of ill tidings, and to announce to you the death of our past President, Mr. Strickland Kneass; a man whom you all knew, and knowing, held in the highest esteem—a typical good citizen and good engineer, faithful in every relation of life, full of industry and conscientiousness, devoted to duty, warm-hearted, clear-headed, capable and honorable.

A special meeting of the Board was held on Thursday, for the consideration of the best method by which our appreciation of Mr. Kneass, and our deep regret at his loss, might be expressed for transmission to his friends and incorporation into the records of the Club.

It was thought fitting to substitute for the drafting of the ordinary resolutions, which bear, perhaps unavoidably, a somewhat perfunctory character, the preparation of a Memorial, containing such a recital of the history of Mr. Kneass as should more effectively set forth his distinguished services and example, both to the profession and to the community.

The Special Committee to whom this duty is assigned, consists of Messrs. Graff, Chairman, DuBarry, Worrall, McClure and Dye, the gentlemen best qualified to discharge it."

Mr. Wilfred Lewis read a paper upon the "Resilience of Steel," reviewing some of the

means employed for the storage of energy, and showing the place occupied by steel among them.

Among the means now employed, compressed air, hot water and the storage battery were cited from an English writer as being about equal in value, and as giving out about 6,500 ft. lbs. of work per pound of material used.

Steel springs, according to the same writer, were said to yield about 18 ft. lbs. per pound. In this connection, the project of using steel springs as a motor for street cars was referred to as the most hopeless of all possible means of locomotion.

To test the accuracy of this statement in regard to steel, several experiments were made by the writer upon tempered specimens, both for tension and flexure. Contrary to expectation, the highest results were shown by the flexure of a small spiral clock spring weighing 2040 grains, which gave out, when wound up, about 45 ft. lbs. of energy, or in other words, 154 ft. lbs. per pound.

The transverse strength of this steel within the elastic limit was found to be about 300,000 lbs. per sq. in., and its modulus of elasticity about 30,000,000. Such extraordinary strength, with such a low modulus, was so far beyond conjecture that it seemed to give a new hope for the success of the project referred to; but after making the necessary allowances for weight of car and efficiency of driving mechanism, it was found that not more than about 20 ft. lbs. per pound of car would be available for locomotion. It was therefore improbable that such a car could ascend a hill over 20 feet high.

It was also a matter of doubt whether larger springs could be made to show results which would even approach these figures, and on this account the experiments about to be tried might be looked for with some interest.

Mr. H. C. Luders presented a description, illustrated by photographs, of the ancient ship found near Sandefjord, in Norway.

He also exhibited specimen of rolled and annealed phosphor-bronze of maximum ductility, and consequently of minimum tensile strength, and submitted the following data of the test thereof: length, 2″: diameter, 0.57″; subjected to a strain of 13,620 lbs., equivalent to 53,400 lbs. per sq. in.; elongation, 70.5 per cent.; reduced area at point where fracture would occur, 0.3″; elastic limit, about 18,000 lbs. per sq. in. Hard rolled rods tested without turning off the surface, have shown a tenacity exceeding 90,000 lbs. per sq. inch.

The Secretary presented, for Mr. Louis C. Madeira, Jr., the Record of American and Foreign Shipping, containing an interesting set of drawings for the details of construction of iron ships.

Mr. Percival Roberts. Jr., gave some account of the results of experiments, now being conducted by Mr. James Christie, at Pencoyd, upon the relative elasticity of iron and steel structural shapes.

Regular Meeting, February 2, '84.

President Wm. Ludlow in the chair.

Mr. Wm. Lorenz presented a comprehensive discussion of steel and iron railroad ties, illustrated by numerous full-sized models and drawings.

Mr. C. J. Quetil, introduced by Mr. J. J. de Kinder, gave an illustrated description of his wire truss railway.

President Wm. Ludlow exhibited specimens of obstructed water pipe, discovered in his practice, and explained the chemical causes which led thereto.

Mr. J. Milton Titlow exhibited and described a full set of general and detail drawings for the proposed stone bridge over the Schuylkill River at Market Street, Philadelphia. As the cost thereof is estimated at $1,200,000, in the discussion which followed, President Ludlow suggested the alternative of building two $600,-000 iron bridges therewith—one at Market Street and one at Walnut Street, over the Schuylkill, the latter bridge being very greatly needed at this time.

Mr. E. F. Loiseau presented a description of the process of manufacture of Portland cement by the application of his solidifying apparatus.

Mr. Robt. W. Lesley, visitor, supplemented Mr. Loiseau by explaining the difference between the old and the above-mentioned methods in actual practice.

ENGINEERING NOTES.

The Brown Hoisting Apparatus.—Briefly described, the Brown bridge tramway is as follows: It is a trestle bridge supported on movable piers. In the rear of the docks the piers are placed upon double tracks and so arranged as to be transferred easily along the docks. One great improvement in the application of this latest patent is the ability to move either the rear or the front piers separately or with each other. The tramway over the front pier is arranged with a "U" brace, on a ball and socket movement, thus allowing any position of the bridge for the convenience of loading or unloading without causing a strain upon any of its parts. The front pier is run upon a single track for this purpose. This pier is thirty feet high to the bridge, which slopes gradually upward to a height of forty-five feet over the rear piers, which are erected through an engine-house 40x16 feet in dimensions at their base. In the plant now erected are three bridges, two of which are connected with the engine-house, as it is usually unnecessary to move them more than a few feet in front to accommodate themselves to the rear hatches of a vessel. A third bridge is entirely detached and is used in emptying the fore hatch. The total length of the stringers is 210 feet, and the bridge has a clear span of 180 feet. Its bottom cords are of wrought iron; the top cords and stringers of wood, amply braced for its own weight and the wind pressure, besides the weight of the load, and in such a manner as to permit of a free movement of the bucket. The apron extending over the ship is raised to escape masts and rigging when not in operation. The distinctive feature of the apparatus and one not possessed by any other hoisting and conveying machines is the ability of the operator to hoist and lower at the upgrade end. Its operation s

are performed in a positive and automatic manner without aid from springs by the direct action of the hoist rope, and is thus placed under the perfect control of the operator. The capacity of the machine is limited only by the speed with which the men on ship board can load the buckets, but with the usual speed now attained, and, with three buckets for each hatch, it is an easy matter to get out 1,000 tons of ore in ten hours.

The Cleveland Institution of Engineers held the third meeting of the present session at Middlesbrough on Monday last. Mr. G. J. Clarkson's paper on the new patent law, read at the previous meeting, was thoroughly discussed, and subsequently Mr. Lowe, of Haswell Colliery, read an interesting paper on a new "mechanical coal-getter." The object of this apparatus is to supersede the use of gunpowder in breaking down the coal after it has been undercut. Holes must be made at the top of the seam as usual. Then the instruments—which are long and narrow—are introduced, one into each hole. By turning a crank attached to the outer end, the collier has the power of thrusting apart two loose blocks near the inner end of the apparatus, by forcing a wedge between them. The result is to separate the whole block of coal under treatment from the strata immediately overlying it. By means of this invention far less small is made than where gunpowder is employed; and besides this advantage, there are the still greater ones of avoiding any vitiation of the air, and of running no risk of firing any combustible gases which may have accumulated in the vicinity.

The subject of fuel economizers was before the members of the Manchester Association of Employers and Foremen, at their usual fortnightly meeting on Saturday. The question was introduced in a paper read by Mr. Daltry, of Manchester, who urged that nothing like the best possible results had yet been attained by the economizers at present in use. If they supposed that with any economizer the temperature at which the issuing gas entered the chimney was as low as 400 deg., there was still a deal more of heat wasted than if it were possible to let them emerge at the temperature of the feed-water. By a rough calculation he estimated that this would give an increase of economy of 15 per cent. of the coal burnt; and supposing that with a forced draught and a suitable economizer this result could be attained, nothing could better it. A somewhat animated discussion followed. Mr. Nasmith expressed the opinion that the present form of economizer was not a perfect one, nor did he think that any form of economizer would be perfect until they got a more efficient system of heating their boilers. They would have to adopt some method other than the use of green fuel before they could apply any system of economizing the waste heat with the best results, and in his opinion they would only be able to effect this by the use of gaseous fuel. There was a wide field not only for a radical alteration in the fuel economizer itself, but also in the method of raising and applying the heat. Mr. Marmock did not see that much advantage was to be gained by the use of gaseous fuel; in some cases the use of gaseous fuel had given worse results than direct firing with coal. Mr. Baldwin was of opinion that under certain conditions gas fuel might be used with great benefit in the heating of boilers. The chairman—Mr. Thomas Asbury, C.E.—thought there were many points in connection with fuel economizers that would have to be considered all round; the whole subject of fuel economy was one about which they had been rather careless, and in his opinion engineers had been rather remiss in taking advantage of every possible point. With regard to gas fuel, he thought the time was fast hastening when this method of raising heat would become pretty general.

The Forth Bridge.—Considerable progress is now being made with the works of the great bridge across the Firth of Forth, which includes two spans of the unprecedented width of one-third of a mile each, or about four times that of any existing railway bridge. Some £30,000 per month is the present expenditure in temporary and permanent works, at which rate the bridge will be completed in the anticipated period of five years from the date of the contract. Only four classes of materials will be used in the permanent works of the bridge, and these being steel, granite, whinstone, and Portland cement, they will be of the most durable and trustworthy character. Some idea may be formed of the magnitude of the undertaking from the statement that the materials required for the Forth Bridge would fill 1,000 goods trains of average length and capacity. This enormous weight will, of course, require to be handled several times, the whinstone alone being found on the spot, the granite being shipped from Aberdeen, the steel from Glasgow and South Wales, and the Portland cement from different places in England. As it is intended to manufacture the steel superstructure of the bridge on the spot, very extensive works have been constructed at Queensferry, and the plant provided includes about fifty steam engines of various classes, and a large number of specially designed hydraulic appliances, drilling machines, and other tools for dealing with the 45,000 tons of steel which will be used in the bridge.

At the present time ten of the smaller piers have been founded and carried up to varying heights, and the rock has been prepared by the aid of diamond drills and rock drills of other types for the founding of some of the main piers. Each of the three main piers will consist of a group of four cylindrical masses of granite and whinstone, ranging from 70 feet to 60 feet in diameter at the base, and founded either upon rock or upon boulder clay of an exceptionally hard character. Where the bed of the river is of rock, it will be leveled and otherwise prepared for the piers by means of a large diving bell of special construction, wherein are a series of rock drills driven by compressed air, the whole being lighted by incandescent electric lights. Where the foundation is on clay, a considerable thickness of mud and silt has to be cut through, and preparations are now complete for putting in the first of the

piers by what is known as the pneumatic process. A caisson, or diving bell, 70 feet in diameter, and 65 feet in height, will be sunk to the required depth by a large number of men working in the bell, which will, of course, be constantly fed with compressed air, to prevent the water from flowing into the chamber, and to supply the men with the required amount of oxygen. Many bridge piers and other subaqueous works have been successfully carried out on this plan, but in the present case the appliances for removing the earth and the details generally are of a novel and interesting character.

Simultaneously with the building of the piers, the manufacture of the superstructure of the bridge will be proceeded with, and several hundreds of tons of steel plates are now on the ground. One of the peculiar features of the Forth Bridge is that all the important members subject to compression will be of a tubular form—a form which the instinct of an ordinary person and the deductions of theory and experiment alike indicate as best adapted to resist compressive stress, but which difficulties of manufacture have excluded from nearly all but American bridges heretofore. About three miles of steel tubes, ranging from 12 feet to 5 feet in diameter, and from $1\frac{1}{4}$ inch to $\frac{1}{2}$ inch in thickness, will be required for the Forth Bridge, and this great quantity justifies the expenditure which has been incurred in the special plant for the manufacture of the tubes. The proper plant being provided, it is as simple to make a tube as any other shaped member. Thus the steel plates will be heated in gas furnaces, and, when red hot, be stamped to the desired curvature in a 2,000 ton hydraulic press; the edges will next be planed all round and the plates be temporarily clamped together to form a tube about 400 feet in length. Traveling drilling machines will then traverse the tube and drill all the holes required to rivet the plates together, but this riveting will not be done until the bridge is being erected, plate by plate, across the Forth. All of the machinery required to commence the manufacture of the tubes is on the ground, and has been found to work very satisfactorily, which reflects credit on the designer, Mr. Arrol, one of the contractors of the bridge.

To fix the position of the piers of a bridge having two clear spans of 1,700 feet, and two of 700 feet, across a channel 200 feet in depth, is not so simple a problem as may appear to some persons. In the case of the Forth, the banks are more or less precipitous, and the country is undulating, so that it was difficult to get a base line for a trigonometrical measurement. At the request of Mr. Fowler, the Ordnance Department kindly sent a party of sappers to recover the old stations used in the Ordnance Survey of Scotland, and to take the necessary additional observations to determine the width across the Firth of Forth. The calculated result differed about 1 foot from that obtained by Messrs. Fowler and Baker's engineers, which is not much, perhaps, in a difficult measurement of over a mile across deep water, but enough to make it desirable that it should be set right. Mr. Baker held the view that it was possible to measure the 1,700 feet span to the eighth of an inch if necessary, and the measurement has just been made without any difficulty whatever. A length of 1,700 feet was first measured with what may be termed absolute exactitude along a straight piece of the North British Railway, and posts of a certain height were firmly fixed at each end. A steel pianoforte wire was then hung from these posts with a sag or droop of 24 feet at the center, and marks were fixed to each end of the wire to indicate the 1,700 feet length. The wire was then coiled on a roller and taken on board a steam launch, from which it was laid like a submarine telegraph cable across the channel of the Forth. It was then hauled up above the surface of the water till it hung with the original droop of 24 feet, when, assuming the temperature to be the same, the marks put on the pianoforte wire should indicate the required span of 1,700 feet. This process was repeated several times, and the wire taken ashore to the railway to test whether it had stretched, but all the observations were in exact accord. The sensitiveness of this simple mode of measuring the span with a wire weighing only 8 lb. in all was quite astonishing. Thus, when Mr. Baker slacked out the wire only $\frac{1}{4}$-in. in the 1,700 feet, Sir Thomas Tancred, one of the contractors, who was in a boat in the center of the Forth, at once signaled there was too much droop in the wire. At another time he signaled that it was too high, but a reference to the thermometer showed that the temperature had fallen 2 deg., so that the wire proved itself a most delicate indicator of changes of temperature. The officers and crew of Her Majesty's ship *Lord Warden*, the ironclad guardship anchored immediately above the site of the bridge, have always taken much interest in the works, and while the measurement was being made, they very kindly manned their boats and regulated the traffic past the spot. Engineers and contractors engaged in carrying out a giant bridge across a stormy estuary and reaching to a height twice as great as that of the topgallant masts of the *Lord Warden*, could desire no better neighbors than Captain Kennedy and his well-disciplined and willing crew of 300 bluejackets.—*Times*.

IRON AND STEEL NOTES.

Blowholes in Steel.—The winter session of the Manchester Association of Employers, Foremen and Draughtsmen was opened on October 13, when a well-attended meeting was held in the Mechanics' Institution. An interesting paper on "The Manufacture of Steel" was read by Mr. W. Annable, of Govan, Glasgow. The paper was mainly devoted to a discussion of the best methods to be adopted in overcoming one of the serious difficulties—the existence of blowholes—in the manufacture of steel. Mr. Annable, having quoted several authorities to prove that gases were occluded as well as mechanically mixed with the metal, said they were told that the mechanically mixed gases tried to escape during the time the metal was passing from the fluid to the solid state, but as steel set so rapidly, a goodly portion of this

gas was entrapped and formed blowholes. This theory appeared very feasible, as cavities were more numerous near the top end of ingots than at any other part, and this seemed to demonstrate that, if the metal had remained fluid a little longer, these gases would have escaped. In practice, however, this was not the case, for metal that was not "dead melted" and contained these gases was always the longest in setting, and instead of going down in the mould as contraction took place, did the reverse, began to come up, and, unless force were applied, would come over the top of the mould. As it seemed to be the mechanically mixed gases that gave us the blowholes, these were the gases that makers and users of steel were interested in, and to overcome the effects of which many methods had been adopted. Having described several of the best known of these methods, Mr. Annable said his own experience told him that, if the steel were well saturated with any of the five following properties– carbon, manganese, silicon, phosphorus and sulphur—which were always associated with iron and steel more or less, the metal would sink down in the mould, and would be found quite free from blowholes. He believed that iron founders found the same thing, that common iron was easier to deal with in casting than the better brands were. But this metal, although free from "blows," would not be suitable for our everyday requirements, as we must have a definite composition for each and every purpose. Consequently the important question arose as to how they were to obtain these definite tempers without blowholes. What was needed was a simple, practical method whereby they could produce all classes of steel free from blowholes or cavities. He had tried several methods with success. One was dead melting, but this did not always give the desired results when dealing with large masses of metal, as they could not get rid of the metal before the temperature became reduced, and this gave rise to some changes. With small quantities, however, as pot metal, dead melting was all that was required; it answered admirably, and could be depended upon. One method he had tried, and which he would recommend, was to cast the ingots in closed top moulds. These were run from the bottom, and inside each mould a small quantity of combustible material was placed, which was fired by the incoming metal. This at once expanded the air in the mould and created a pressure, as it could not readily escape by the holes in the top of the mould, which were only two, each $\frac{3}{8}$-inch, whilst the head of metal they had in the runner counterbalanced the pressure, and the gases rushed out of the two small openings with a loud roar. It was not, however, pressure to which he wished to draw their attention, but to one fact, that the metal was being cast free from atmospheric air, to which Dr. Muller and others assigned the cause of blowholes. The metal was being cast in an atmosphere of carbonic acid, brought about by the combustion of the shavings which were placed in the moulds before casting began. Besides, they had all the ingot cast at one temperature, one density, and it would be found perfectly homogeneous, which could not be said of those cast from the top, whilst it would be also free from blowholes. In addition, the "pipe" in the top of the ingot would be found much reduced, according to the length of the runner, as the ingots received a supply of metal from it during the time they passed from the fluid to the solid state. Another method was to place the ladle on to the center runner, and then they excluded all air besides using the weight of metal in the ladle to force the metal home against the pressure of gas set up in the mould. The open-topped mould, so much used for Bessemer and Siemens ingots, was objectionable in more than one sense. When the metal fell from the ladle to the bottom of the mould, sometimes 8 or 9 feet, it not only took down with it a stream of air, but in falling on the bottom of the mould, the first twenty or thirty pounds of metal were splashed in all directions, and if they were to examine these splashes they would find them to be covered with a blue scale, which was oxide of iron. As they went on pouring the hot fluid metal, it re-melted the splashes, and quite a reaction took place in the steel at the lower end of the ingot; the oxide of iron gave up its oxygen, and attacked the carbon and manganese of the steel, just the same as when ore was thrown into the furnace, and they could at any time see the brownish red fumes leave an open-topped ingot mould when casting. These fumes, which were the result of the chemical action below, became less and less as the metal got nearer the top. He had stated that open-topped ingots were not homogeneous. To prove that this theory was correct, he had several ingots analyzed, and the result showed that at any rate carbon was eliminated by the reaction of which he had spoken, caused by the interference of the atmospheric air. Some might think he had exaggerated the effects of oxygen on molten or heated metals, but what he had said would be confirmed by the practical experience of any smith, as a small bar of iron or steel at a red-heat under the hammer would yield a thin film of scale at every blow. This scale was oxide of iron, and showed with what avidity oxygen attacked metals even at a red-heat. They might, therefore, assume that, with fluid metal broken up or disintegrated, as it was when falling some feet to the bottom of the mould, oxidization would take place at a greater ratio than when metal was only just red hot. He had seen ingots cast in the manner just described. When brought under the hammer, the bottom end had dropped off at the first blow, after which the remainder of the ingot had hammered all right and given good blooms and billets. This he attributed to the manner in which the ingot was cast. What they wanted was some simple method of treatment, which would give them all tempers of steel free from cavities. Pot metal, which gave them all kinds of tools, they could get free. Castings also might be made free by conforming to certain treatment and chemical mixture, by skill and care in preparing the moulds, and by judgment in allowing for fluid contraction by giving the metal plenty of head large enough in diameter. As the bulk contracted in cooling, it would get its feed or supply from the center of this head or column of metal put there for the purpose, and by mak-

ing provision in coils for that large amount of contraction in steel, this would take place without tearing the casting asunder. The ordinary Bessemer metal was very frothy and lively when cast below carbon 0.3, mild steel temper, and it was with difficulty that it was kept in the mould; but even should it be prevented coming over the top, it was not free from blowholes, which would be found to penetrate to half the depth of the ingot. Solid castings were being produced in steel, but the great difficulty arose where they had to make ordinary soft steel free from blowholes, and this was a difficulty which led to enormous waste in manufacturing operations. The subject was, however, receiving the fullest attention, and he had not the least doubt but that the difficulty would be shortly overcome, and that they would be enabled to produce ingots and castings that would be as sound and free from cavities as though they had been forged under the hammer.

A discussion followed the reading of the paper, and Mr. Annable, replying to a few questions, said a good reliable cylinder could not be made without putting a large head upon it; if it were made in any other way, it was sure to be a failure on account of the contraction which took place all the way down. In his opinion the steel made by compression was really no stronger than steel made in the ordinary way, provided, of course, that it received proper treatment afterwards. He believed the report with reference to the large output of steel in America was perfectly true, but, speaking for himself, he would only say that he had never gone in for quantity, but for quality. He did think they could get a reliable tool steel from the Bessemer process, because the proportion of manganese was too great; and they could not get the amount of carbon which was required. Tool steel must have its chemical elements mixed to a nicety, because if this were not secured, it would be unsuitable for the purpose. The chairman, in closing the discussion, observed that the difficulties connected with the manufacture of steel were being grappled with in a manner which gave them confidence in ultimate success. Good sound ingots and sound castings were the great desideratum in the use of steel, and he had no doubt the time would soon come when solid steel would be relied upon quite as much as in any other metal. The varieties of steel were so great that it was sometimes difficult to discover where steel began and where iron ended. One great mistake that was made was that the user of steel did not always specify the purpose for which the steel was required, and if engineers or other persons designing anything where steel was required would be frank and tell the maker what the steel was required to do when it was in position, they would frequently get more satisfactory results. With the tools of such immense power as were now being produced, many of the difficulties which had hitherto stood in the way of the manipulation of steel would disappear.—*Iron.*

RAILWAY NOTES.

The railways of South Australia bring in a total revenue of £469,000, yet the actual net receipts to be set against the interest on cost of construction is only £146,000. The *Colonies and India* says economical management might make these railways indeed a splendid property, equal in value, perhaps, to three-quarters of the national debt. But as things are at present it can only be said that they pay about half the interest on their cost of construction.

ORDNANCE AND NAVAL.

The many erroneous statements made in the daily and weekly papers with regard to the efficiency of primary batteries, induces me to send the following figures, relating to the quantity of fuel required to produce 1 horse-power per hour by means of steam, and by electricity. Perhaps these figures may be thought of sufficient interest for insertion in your next issue.

		Foot-pounds.
1 Heat-unit (Fahrenheit)........	=	772
Ditto (Centigrade)........	=	1,390
1 Horse-power per minute........	=	33,000
Ditto per hour.........	=	1,980,000

Ditto ditto in heat-units—

$$\frac{1{,}980{,}000}{1{,}390} = 1{,}425$$

Heat-units (Centigrade) contained in—

1 lb. of coal................	=	8,000
1 lb. of hydrocarbon (liquid)................	=	11,000 (about)
1 lb. of hydrogen...........	=	34,000

Assuming with Professor Adams (see opening address at the Society of Electricians), the greatest efficiency of a theoretically perfect heat-engine to be = 23 per cent. We have—

for coal........	23 per cent. of	8,000	= 1,840
for hydrocarbon	" "	11,000	= 2,530
for hydrogen....	" "	34,000	= 7,820

Hence, 1 horse-power requires for its development in such a theoretically perfect engine at least—

$$\frac{1{,}425}{1{,}840} = 0.774 \text{ lb. of carbon,}$$

$$\text{or } \frac{1{,}425}{2{,}530} = 0.563 \text{ lb. of hydrocarbon,}$$

$$\text{or } \frac{1{,}425}{7{,}820} = 0.182 \text{ lb. of hydrogen.}$$

The efficiency of a perfect voltaic battery and electric motor may, at the lowest estimate, be assumed as 90 per cent.

1 lb. of zinc = 1,300 J; 90 per cent. = 1,170. One horse-power developed in an electric motor by a current from a voltaic battery requires hence at least—

$$\frac{1{,}425}{1{,}170} = 1.133 \text{ lbs. of zinc.}$$

The above will elucidate the assertion about 1 horse-power being produced by 0.166 lbs. of hydrocarbon, against the lowest possible 0.563 lbs.: but it also shows Mr. Reckenzaun's modest estimate of 2 lbs. of zinc per horse-power per hour in its proper light.

I may here add that D'Arsonval (see *La Lumiere Electrique*, vol. v., p. 90) has obtained 368 kilogramme metres with 1 gramme of zinc; this corresponds to a consumption of 1.42 lbs. of zinc per horse-power per hour.

C. GODFREY GUMPEL.

—*In Journal of Society of Arts.*

BOOK NOTICES

THE AIR WE BREATHE, AND VENTILATION. By HENRY A. MOTT, JR., Ph. D., F.C.S. New York: John Wiley & Sons.

It requires but little technical knowledge to read this book understandingly. It is a small volume, but has a good deal of practical science condensed within its leaves.

In the portion relating to Ventilation, a large proportion of the space is devoted to illustrations of methods and apparatus employed in aiding or forcing ventilation.

NOTES ON ELECTRICITY AND MAGNETISM. By J. B. MURDOCK. New York: MACMILLAN & Co.

This is the book of an instructor who finds that students require additional aids to those furnished by such text books as Sylvanus Thompson's Elementary Lessons.

Direct reference is made to Prof. Thompson's book, and the subjects are treated in the same order.

It will doubtless prove a valuable supplement to the larger book.

ENERGY IN NATURE. By WM. LANT CARPENTER, B.A., B. Sc. London: Cassell & Company.

This is a popular treatise on Physics, and is especially designed by its style to interest the general reader.

The separate chapters treat successively of —Force and Energy, Heat, Combustion, Electricity and Chemical Action, Magnetism and Electricity, Energy in Organic Nature.

TABLES FOR CALCULATING THE CUBIC CONTENTS OF EXCAVATIONS AND EMBANKMENTS. By JOHN R. HUDSON, C.E. New York: John Wiley & Sons.

The author's method of calculation is new and improved. The tables are well printed, and constructed for road-bed-widths of 10, 14, 18, 24 and 28 feet, and for slopes of 1 to 1, 1 to $1\frac{1}{4}$ and 1 to $1\frac{1}{2}$.

The book is of more convenient size than most similar treatises.

THE WINE-PRESS AND THE CELLAR. By E. H. RIXFORD. New York: D. Van Nostrand.

This is designed as a practical guide to the vine culturist who designs also to make wine. Although the treatise is small, no necessary detail seems to be omitted.

The subjects treated in separate chapters are: Gathering Grapes, Must, Sugaring and Watering the Must, Stemming and Crushing, Fermentation, Red Wine, White Wine, Casks, Sulphuring and Aging, Cellars, Racking, Clarification, Sweet Wines, Defects and Diseases, Wine in Bottles, Cutting or Mixing Wines, Wine Lees and Piquette, the Composition of Wine, Miscellaneous.

The advice is adapted to American Viticulturists, especially those of California.

KNIGHT'S NEW AMERICAN MECHANICAL DICTIONARY. Section Four. Boston: Houghton, Mifflin & Co.

The present section includes the articles from Printing-Press to Zoogyroscope, and completes the edition. The five years that have elapsed since the former edition was completed have produced an abundance of material with which to enlarge the work. Indeed, the superabundance of material led to the adoption of an entirely new feature in the present work—that of references to technical journals which contained fuller accounts of late inventions.

Fifty-six full page illustrations and numberless smaller ones now illustrate the text.

It is an indispensable aid to the mechanical engineer.

A NEW SYSTEM OF LAYING OUT RAILWAY TURNOUTS. By JACOB M. CLARK. New York: D. Van Nostrand.

This useful little essay is accompanied by tables which will enable the practical engineer to locate a turnout instantly. The text gives the mathematical demonstrations upon which the tables are based. The author's method is intended, so far as it differs from others, to save time and labor.

The solutions heretofore published regard the turnout track as located on a curve which is tangent to a switched or deflected rail. It is generally more convenient to locate the turnout upon a curve which is tangent to the main track at a point not far from the heel of the switch. The head-block is then placed where the departure of the center lines from each other is equal to the necessary deflection or throw of the switch bar, which, in turnouts from straight tracks, should not be less than half, nor more than the entire distance from the head-block back to the tangent point.

In this way the exact solutions for all but very rare cases are reduced to three, each of which involves a simple case of plane trigonometry.

MISCELLANEOUS.

M. DUCHARTRE was led, by the influence which a light of very feeble intensity exercises upon heliotropic movements, to vary some experiments by using moonlight. He sowed seeds of plants which were very sensitive to light, such as *Lens esculenta*, *Ervum lens*, *Vicia sativa*. When the plants were a few centimetres in length he put them in a dark place, where he kept them until the night of the experiment. The stalks became slender, long and white; the leaves developed slightly with a light yellowish tinge. On three successive nights when the sky was exceptionally clear the plants were placed behind a large window with a southern exposure, so that they received the direct light of the moon from 9 p. m. to 3 a. m. According to the *Comptes Rendus*, from the very beginning of the exposure the stalks began to bend, so as constantly to present their concavity and the terminal leaf bud to the moon, following it in its course.

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STRAINS IN HIGH MASONRY DAMS, AND THE METHOD OF COMPUTING THEM.

By E. SHERMAN GOULD, C. E.

Contributed to Van Nostrand's Engineering Magazine.

The designing of a masonry dam of moderate dimensions—say up to 50 feet in height—involves no particular difficulty. Not to speak of that sheet-anchor of the constructing engineer, the study of existing works, the resisting and destructive forces are so easily determinable, that very simple calculations suffice to fix the proper, or at least the safe, proportions in any case.

As we pass to greater heights, however, the subject becomes a more difficult one. Not only do we find fewer precedents, but other considerations, beyond the mere balancing of the inertia of the structure against the exterior forces tending to overthrow it or thrust it forward, enter the problem as essential factors, and render its solution a matter demanding a deeper study.

Thus, as we pile up stonework to a height of 100, 150, 200 or more feet, we find ourselves menaced by a danger heretofore safely ignored, namely, the possible crushing of the lower courses of our structure under its own immense weight. Mere weight, as regards inertia only, is an element of safety; in high dams it may become an element of danger.

This possibility of crushing presents itself under a two-fold aspect. If we suppose the reservoir to be empty, then the pressure tending to produce crushing is the weight of the dam itself alone. If we suppose it full we combine with this the horizontal thrust of the water, modifying the pressure and changing the position of its point of application.

If, in order to diminish the pressure upon the foundations when the reservoir is empty, we reduce the weight of the superincumbent mass by rapidly diminishing the thickness as it rises above the foundation, we will thereby increase the pressure when the reservoir becomes filled; for the resultant of pressures will be then carried nearer to the outer toe of the dam, concentrating the strain upon a small resisting area.

If, on the other hand, we increase the mass of our structure with the view of crowding the resultant of pressures, when the reservoir is full, back from the outer toe, then, when the reservoir becomes emptied, and the sustaining thrust of the water withdrawn, the dam is allowed to settle back, as it were, upon its foundations, and we have a perhaps dangerous stress nearer the inner toe.

Again, it does not suffice to confine our calculations to the lower course, or base, of our structure. Our work must be not only safe but economical, and the necessity of economy increases with its size and importance.

These being the facts, it will be readi-

ly conceded that the problem of determining the section of equal resistance—or, in other words, the section of maximum economy—of such dams, combined with a proper but not extravagant factor of safety, is one calling for deep and intelligent study on the part of the engineer.

It was to the solution of this problem that the French engineers charged with the work addressed themselves when the construction of a masonry dam 164 feet high was projected for the purpose of controlling the waters of the river Furens, near St. Etienne.

Let us glance for a moment at the requirements and data of the problem.

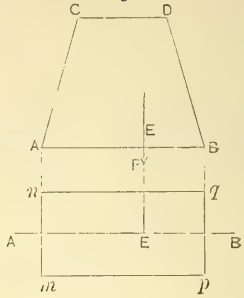
If we suppose a wall of homogeneous material and symmetrical transverse section—a "plumb" wall, for instance—we shall find the pressure upon the base, or any other horizontal section, to be uniformly distributed over the entire area of such section, the pressure per square inch being equal upon all parts of the same, because the vertical line drawn through its center of gravity cuts any and all of the horizontal sections which we may conceive to be passed through its mass in the middle.

If, however, from want of symmetry itself, or from being subjected to the action of exterior forces, or from any cause whatever, the line passing through the center of gravity of the mass situated above any given horizontal section, or the resultant of all pressures upon such mass if subjected to exterior forces, does not cut such horizontal section in the middle, the pressure per square inch is not uniformly distributed over the same, but reaches a maximum at the edge nearest to which the vertical line or resultant passes, and a minimum at that from which it is the most remote, the pressure varying according to some law which it is essential to determine.

This law is established by Monsieur Debauve in his *Traité des ponts en Maconnerie*, and the ensuing formulæ given, with some modifications in the 4th chapter of the 19th fascicule of his *Manuel del' Ingénieur des pont et Chaussées.*

I copy the following from a translation of the same made by me under instructions from Mr. Isaac Newton, Chief Engineer of the Croton Aqueduct, at the time when the projected extension of the New York City water supply was being studied in his office.

Fig. I



"Let us consider (Fig. 1) a body of masonry, a wall for example, of which the transverse section made by a vertical plane is ABCD, and of which the base is represented in plan by the rectangle *mn pq*. On the median line of this rectangle there acts a vertical pressure P, applied in E. This pressure is distributed unequally at different points of the base, and it is important to ascertain the law of its distribution.

"Let the length mn of the mass be equal to unity; let l equal the depth AB of the base, and u the distance BE of the point of application of the pressure P from the exterior edge B of the prism.

"The elementary pressure at the point p, that is to say, the pressure per square foot, is given by one or the other of the two following formulæ:

$$(1)\ p = 2\left(2 - \frac{3u}{l}\right)\frac{P}{l}$$

$$(2)\ p = \tfrac{2}{3}\frac{P}{u}$$

according as u is greater or less than $\frac{1}{3}\,l$.

"The Discussion of the preceding formulæ gives the following results:

"When the point E is in the middle of the base, the pressure is uniformly distributed, and the elementary pressure at the point B, as elsewhere, is equal to $\frac{P}{l}$.

"As E moves from the middle of the base and approaches B, the pressure upon that edge augments, while the pressure upon the edge A diminishes.

"When BE, or u, is equal to $\frac{1}{3}\,l$, the pressure on B is equal to $\frac{2P}{l}$, that is to

say, to the double of what it was under an uniform distribution. The elementary pressure diminishes proportionally to the distances from B to A. It is nil at A.

"When BE becomes less than $\frac{1}{3}l$, the formula (2) is to be used. The pressure on the edge B augments continually, as the point E of application approaches this edge. The opposite edge A sustains negative pressures, that is, tension. But the mass is supposed to simply rest upon its base, BA, and no account is to be taken of cohesion. There can, therefore, be no tension at A, and consequently there is, starting from A, a certain zone sustaining no pressure, a zone of which the extent increases in proportion as E approaches B.

"The pressure, P, concentrates itself, therefore, more and more upon the edge B. It is distributed over a zone of which the extent is constantly diminishing, and when the point of application actually coincides with the edge B, the elementary pressure upon the same becomes infinite, as is shown by formula (2).

"If the point of application, measuring from the middle of the base, should progressively approach the inside edge, A, instead of the exterior edge, B, contrary results would occur. The elementary pressure would increase up to infinity at A, and decrease down to zero at B, over a zone increasing in extent from the edge B.

"The important point is to know the maximum pressure in each case, and for this the formulæ (1) and (2) suffice."

In conclusion, Monsieur Debauve remarks that: "It always remains true that, in adding to or taking from the mass, we affect the distribution of pressure, and may, therefore, by these means, approach as nearly as we please to a uniform distribution."

This law and its inferences, as given above upon the authority of Monsieur Debauve, form the key to the entire problem.

The calculations for the Furens dam were made by Monsieur Delocre, and are fully set forth by him in the *Annales des ponts et chaussees*, 1866. These calculations are somewhat intricate, and the matter may be better understood from the simplified processes—combined calculation and graphics—of Monsieur Debauve, as given by him in the volume mentioned above. It is not neccessary, however, except as a matter of very great scientific interest, to follow the steps by which the proper section for such dams was arrived at. When the investigation was commenced, it was impossible to say precisely what form of cross-section would be reached. The problem was; from among all conceivable forms, to detect that one which fulfilled the desired conditions. Mathematical processes, employed with an intelligence and skill far beyond any praise which I am adequate to bestow, lead up to the desired result as unerringly as if the question had been one of simple maxima and minima. The *general form* having been thus established, the problem is solved once and forever, and the only task left for succeeding designers is the safe, though humble, one of imitation.

I will now rapidly trace the successive stages of the investigation.

If we had only the weight of the dam itself to consider, that is, if our reservoir was to be always empty, it is shown that the proper form of cross-section would be a double, symmetrical logarithmic curve (Fig. 2); for this form secures a

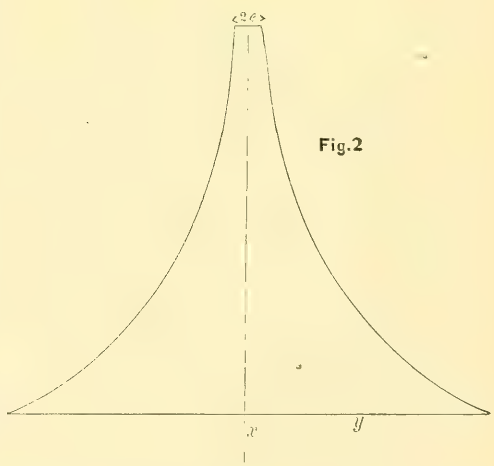


Fig.2

section of equal resistance; the increment of surface increasing in the same ratio as the increment of pressure, and the weight would be uniformly distributed over any given horizontal projection.

The formula for this curve is as follows:

$$x = \lambda \,.\, L.\frac{y}{e}$$

in which

x=any vertical height corresponding to a given half width y.

λ=limiting height of a vertical wall sustaining only its own weight. This limiting height is determined by fixing upon a limiting pressure per square foot=R and dividing it by the density per cubic foot of the masonry $=\delta$, thus,

$$\lambda = \frac{R}{\delta}$$

L=hyperbolic logarithm of y

e=any given half-width of top of wall. If unity be assumed, the above formula becomes

$$x = \lambda \,.\, L\,y.$$

If the reservoir were to be always full, then calculation shows that its proper form of cross-section would present a vertical face on the side next the water, and on the lower side a curve determined by the formula,

$$l = h\sqrt{\frac{\pi h}{R}}$$

in which

l=length of any horizontal section.

h=height corresponding to same.

π=weight of cubic foot of water= 62.5 lbs.

R=limiting pressure per square foot.

This would give a cross-section such as is roughly shown in Fig. 3, where the top width has the theoretical dimension of zero.

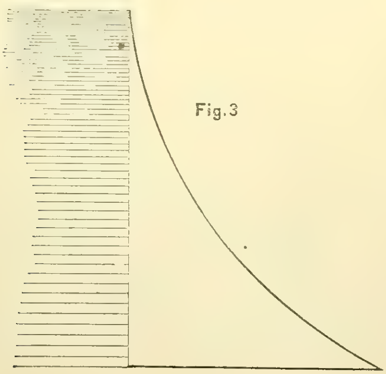


Fig.3

But the reservoir is subject to be alternately full and empty, and moreover must have, in practice, some actual top width instead of the theoretical width = zero, shown in Fig. 3. We should have, therefore, to adopt a compromise section, similar to that shown in Fig. 4.

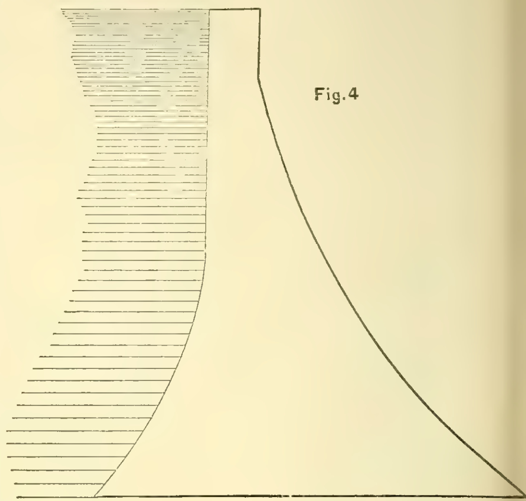


Fig.4

These forms are general. The precise form decided upon for the Furens dam is shown in Fig. 5, and that of the Ban, built soon after that of the Furens, by the same engineers, is shown in Fig. 6.

As a basis of practical calculation, it is first necessary to fix upon the limiting pressure per square foot or square inch to which it is proposed to subject the masonry. In the designing of the Furens dam considerable caution was

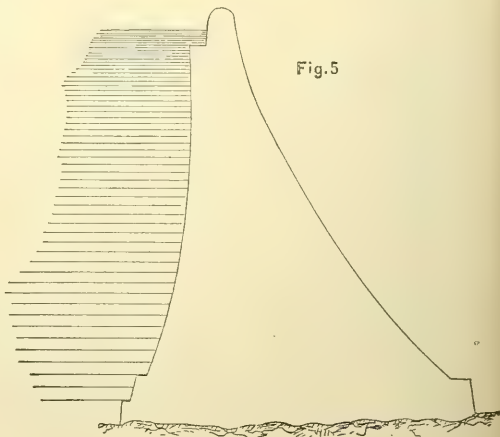


Fig.5

exhibited in this respect, and the limit was fixed at 92.5 lbs. per square inch. In the case of the Ban dam, this limit was advanced to 114 lbs. per square inch. Observation shows that masses of masonry sustain much greater pressures without injury. On the other hand, instances are not wanting of crushing taking place in the lower courses of high buildings; and besides, the saving of masonry consequent upon adopting a high limit does not increase as rapidly as original process of calculation in order to determine at once the proper form of cross-section, since we know that it must finally closely resemble that of the Furens or the Ban, it will be best to sketch a similar section, conforming to the desired conditions of height and top-width, and test it by the formulae (1) and (2), having previously determined upon the limiting pressure per square inch. If not satisfactory, a few trials will readily decide the necessary modifications.

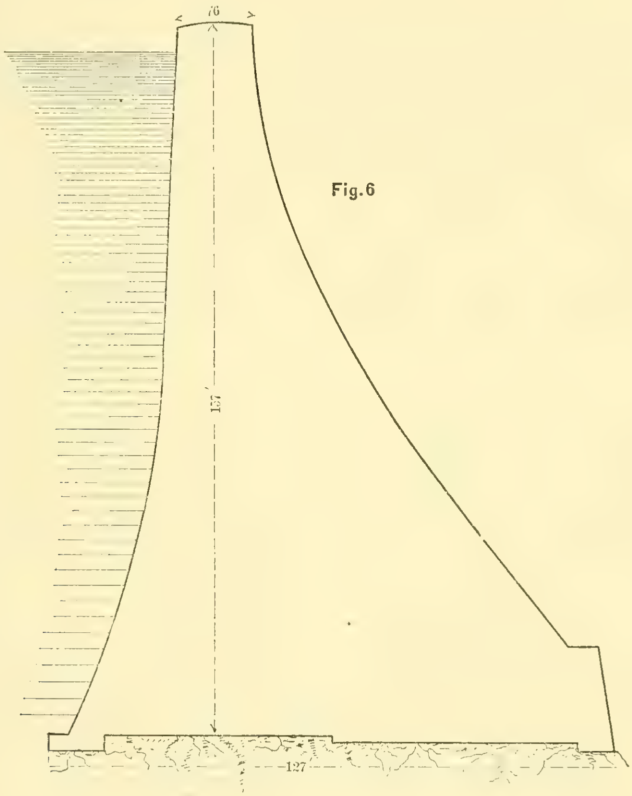


Fig. 6

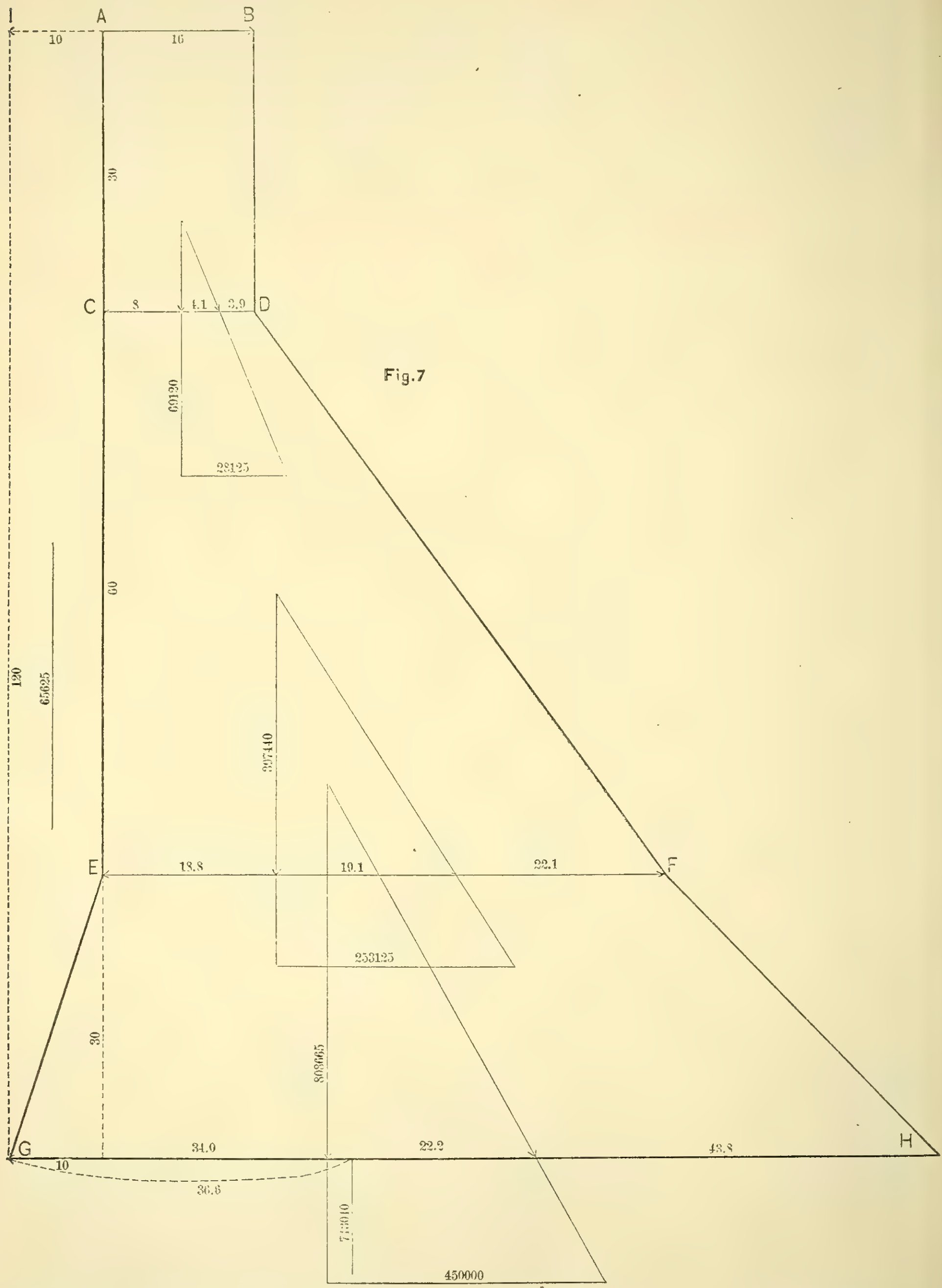
the pressure, in the case of dams. Thus, Monsieur Debauve shows that the saving of masonry between the limits of 86 and 200 lbs. per square inch per running foot of a dam 164 feet high, is only about one-quarter, while the pressure is considerably more than doubled. In other words, in this particular case, the saving is to the increased pressure about as 1 is to 9.

Sufficient has been now said to show how such dams may be designed. Instead of going through, in each case, with the

As some engineers may not be familiar with the processes to be employed in testing the cross-section, I will give an example.

I will premise that for the purpose of calculation, it is convenient to transform the section from one bounded by curve lines, to one composed of rectangles and trapezoids. These can be made more or less numerous according as we wish to approach more or less closely to a curvilinear figure, or to test a greater or less number of sections.

Let Figure 7 represent the cross-section of a dam 120 ft. high, divided into three masses. Let AB and CD = 16 ft; = 10 ft. We will suppose the density of the masonry = 144 lbs. per cubic foot.



EF = 60 ft., GH = 100 ft., AC = 30 ft., CE = 60 ft., and EG in vertical projection = 30 ft., and in horizontal projection

The object of our investigation is to ascertain at what points the resultants of pressures cut the lines CD, EF and GH,

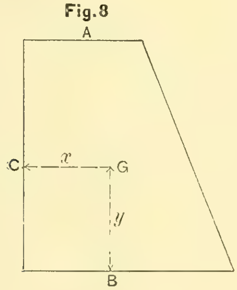
both when the reservoir is full and when it is empty, and their different intensities in order to apply the formulae (1) and (2.)

With the given data, the weights of the masses ABCD, CDEF and EFGH are respectively 69,120 lbs.; 328,320 lbs.; and 345,600 lbs., while the weight of the prism of water AEGI, at 62.5 lbs. the cubic foot, resting on the sloping face EG, is 65,625 lbs.

We will first deal with an empty reservoir, and proceed to fix the position of the vertical lines passing through the different centres of gravity. There are several ways of determining the centre of gravity of any given figure; one is to operate mechanically by suspending a piece of card board or other suitable material, cut into the form of the figure. But I think calculation, when it can be easily done, or otherwise one of the well-known graphic processes, is much preferable.

Thus, let Fig. 8 be a trapezoid having the two horizontal sides A and B, and

Fig. 8



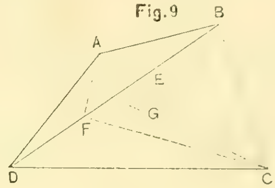
the vertical side C. Then the co-ordinates x and y of the centre of gravity G, are given by the relations—

(3) $$x=\tfrac{1}{3}\left(A+B-\frac{AB}{A+B}\right)$$

(4) $$y=\frac{C}{3}\left(\frac{2A+B}{A+B}\right)$$

A convenient graphic process for finding the centre of gravity of any quadritateral figure (Fig. 9) is given by Molesworth, from whose pocket-book the preceding formulae were also taken. Thus, "In any four-sided figure ABCD draw the diagonals, intersecting at E. Lay off DF = BE, and join FA, FC; then the centre of gravity of the triangle FAC is also the centre of gravity of the Figure ABCD," (marked G in the figure).

Fig. 9



In combining the successive masses, we will take their moments from the line IG passing through the inside toe of the dam. I give below the entire work. A little careful attention to what follows will render any lengthy explanation of the successive steps unnecessary.

The centre of gravity of ABCD of course, is in the centre of the figure. Hence, it is 8 feet from AC, and 18 from IG. The centre of gravity of CDEF is found by (3) to be 21.12 from CE, and therefore 31.12 from IG. The distances of the centre of gravity of AEFDB from IG and AE are therefore obtained thus:

$$\begin{array}{rcl} 18\times 69120 & = & 1244160 \\ 31.12\times 328320 & = & 10217318.4 \\ \hline 397440 & & 11461478.4 \end{array}$$

$$\frac{11461478.4}{397440} = 28.84$$

Distance from IG = 28.8 ft.
" " AE = 18.8 ft.

Again, the distance of centre of gravity of EFHE is found graphically to be 45.5 ft. from IG. The distance of centre of gravity of the entire section AEGHFD-B from IG, is therefore:

$$\begin{array}{rcl} 397440 & \ldots\ldots\ldots & 11461478.4 \\ 45.5\times 345600 & = & 15724800.0 \\ \hline 743040 & & 27186278.4 \end{array}$$

$$\frac{27186278.4}{743040} = 36.59$$

Distance from IG = 36.6 ft.

As we shall presently want the distance of the centre of gravity of the entire mass combined with the prism of water AIGE from IG, we will obtain it at once, thus: the distance of centre of gravity of AIGE from IG being obtained from (4) and found to be 4.76 ft.:

743040	....	27186278.4
4.76×65625	=	312375
808665		27498653.4

$$\frac{27498653.4}{808665} = 34.00$$

Distance from IG = 34.0 ft., all of which is shown on the figure.

We have thus the distances of the lines of vertical pressure from the inside face of the dam, when the reservoir is empty. We now must find the distances, from the outside face of the resultants of these pressures, when combined with the horizontal thrust of the water, the reservoir being full.

The thrust of the water, being at right angles to the line of action of the weights, cannot add to nor diminish the intensity of these last, but only throws their point of action further away from the water side of the dam.

The surface of the water being supposed to be level with the top of the reservoir, its thrust upon that portion of the dam situated above the line CD is given by substituting the height AC in the general formula

$$\frac{h^2\pi}{2} = \frac{(30)^2\, 62.5}{2} = 28125 \text{ lbs.}$$

This thrust is applied at one-third the height AC = 10 ft., and is combined with the weight 69,120 lbs. of the mass ABCD, at the vertical line passing through its centre of gravity. By construction, we obtain the triangle of forces shown in the figure, and by scaling the same get the distance = 4 ft. from the point D at which the resultant cuts the line CD.

We may also obtain the distance very rapidly, and of course more accurately, by calculation. Thus, since the thrust of the water is applied 10 ft. above the line CD, the distance between the resultant and the vertical component at the line CD is given by the proportion:

$$\frac{x}{28125} = \frac{10}{69120};\ x = 4.1$$

The distance from D can then be obtained by addition and subtraction; thus:

$$16 - (8 + 4.1) = 3.9 \text{ ft.}$$

The distance of the resultant from F s found in a similar manner. Weight of mass AEFDB = 397440 lbs. Thrust of water 253125 lbs. Height of point of application of thrust of water above EF = 30 ft. Then

$$\frac{x}{253125} = \frac{30}{397440};\ x = 19.10$$

and

$$60 - (18.8 + 19.1) = 22.1 \text{ ft.}$$

For the distance from H, we have; weight of entire mass and prism of water, 808665 lbs.; thrust of water 450000 lbs. Height of thrust above GH = 40 ft. Then

$$\frac{x}{450000} = \frac{40}{808665};\ x = 22.2 \text{ ft.}$$

And,

$$100 - (34.0 + 22.2) = 43.8 \text{ ft.}$$

All these results are shown in Fig. 7, which is the reproduction of a tracing taken from the drawing upon which all the previous work was laid down. The cross-section is to a scale of 200 ft. to the inch; the triangles of forces are laid down on varying scales, decreasing as the forces augment. It is recommended to calculate all the necessary elements, and then check by construction.

We are now prepared to apply formulæ (1) and (2), and obtain the maxima pressures per square inch. Having calculated the distances so carefully, we see that it will be permissible to force the figures to even feet, except in one case.

We will first investigate the strains on the inside face, the reservoir being empty, taking the maximum pressures upon C, E and G successively.

The upper mass ABCD, being symmetrical, exercises an uniformly distributed pressure over CD, all parts of which sustain therefore a weight of 4,320 lbs. per square foot, or 30 lbs. per square inch.

In the mass AEFDB, the center of gravity is distant 19ft. from E. As this distance is less than $\frac{60}{3}$, we use formula (2), putting $u = 19$ and $P = 397440$; whence we obtain the maximum pressure upon E = 13945.26 lbs. per square foot, or 96.84 lbs. per square inch.

In the whole mass the line drawn through the center of gravity cuts GH at a point distant 36.6 ft. from G. As this distance is greater than $\frac{100}{3}$, we use for-

mula (1), putting $u=36.6$, $l=100$, and $P=743040$; whence we obtain the maximum pressure upon G=13404.44 lbs. per square foot, or 93.10 lbs. per square inch.

We will now investigate the maximum pressures upon D, F and H, the reservoir being full. As we have seen, the horizontal thrust of the water merely throws the weight further along towards the outside face of the dam, without increasing it.

The first distance being 4 ft., which is less than $\frac{16}{3}$, we use formula (2), putting $u=4$ and $P=69120$, whence we obtain the maximum pressure upon D=11520 lbs. per square foot, or 80 lbs. per square inch.

The second distance being 22, which is greater than $\frac{60}{3}$, we use (1), putting $u=22$, $l=60$, and $P=397440$, whence we obtain the maximum pressure upon F=11923.2 lbs. per square foot, or 82.8 lbs. per square inch.

The third distance being 44, which is greater than $\frac{100}{3}$, we use (1), putting $u=44$, $l=100$, and $P=808665$, as we must now employ the weight of masonry augmented by the vertical component of the prism of water resting upon the oblique face EG. We thus obtain the maximum pressure upon H=10997.84 lbs. per square foot, or 76.37 lbs. per square inch.

In examining these results it will be seen that while the pressure is nowhere excessive, it is greatest at E and F. If it were thought best to bring the section into one of a more uniform resistance, a slight modification, when it is so nearly correct, will suffice. Even merely joining the tangents by curves would produce a quite perceptible effect.

It will be perceived that the ordinary calculation of the overturning and static moments is passed by in this investigation. It is implicitly included in the process used, and need not be considered separately. It is well, however, to see that the frictional resistance of 75 per cent. of the weight be sufficient to neutralize the tendency of the horizontal thrust of the water to provoke sliding.

In the preceding calculations the actual weights have been used. It simplifies the work very much, however, to consider only the areas, assuming the weight of a cubic foot of masonry to be equal to unity. The area of water must then be affected by a coefficient; in our example the proper coefficient would be $\frac{62.5}{144}=0.434$. Our assumed density of masonry being the same number of pounds per cubic foot as there are square inches in a square foot, still further simplifies the calculations, as we may use the areas instead of weights throughout, getting our answers directly in lbs. pressure per sq. inch.

Thus, to calculate the maximum pressure upon H: The total area of cross section =5160 square feet. Area AIGE of water, reduced to equivalent area of masonry= $1050\times0.434=455.7$, making a total area =5615.7. Using this as the value of P in formula (1), we obtain at once $p=76.4$ lbs. per square inch, as before.

Other simplifications, or convenient modifications, may suggest themselves in practice. Thus, it will be found, I think, more convenient to write formula (1) in this form:

$$(5) \qquad p=2(2l-3u)\frac{P}{l^2}.$$

In using the design of the Furens dam as a precedent for similar works, the exceptionally favorable site upon which it was built must always be kept in view. The "Gouffre d'Enfer," across which it is thrown, is a narrow gorge, showing exposed rock, or rock under a shallow cover, at the bottom and on both sides. It is so narrow and steep that the dam, seen in elevation, shows almost the form of a triangle with its vertex downward. Its height is 50 meters, and its length on the top 100 meters—only twice as long as it is high. In plan it shows the arc of a circle with its convexity turned up stream. It thus forms an arch, solidly abutted against the rocky flanks of the ravine, its short length enabling the principle of the arch to assert itself with effect.

The "Ban" dam is built on somewhat bolder lines, and under somewhat less favorable circumstances. Its total height is 47.80 meters, and its length, over all, about 160 meters. The ratio of height to length is therefore as 1 to $3\frac{1}{3}$. The masonry of which it is built seems to be of somewhat inferior quality. It is circular in plan, like the Furens.

GAS ILLUMINATION AND ITS ECONOMY.

By JAMES CHEESMAN.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

NEARLY ten years have passed since the electric arc light appeared on the market as a probable competitor with gas. Manipulators of stock, and the varying conditions of the public faith in the new illuminant have left gas stock in pretty much the same position as it was when the markets were first disturbed. The public have seen many and various installations of electric light plants for private and public purposes. The new light has made its way in the face of great obstacles, it has scored many successes, has enabled a few to make fortunes, has shown its shortcomings and its susceptibility of improvement; and it has also indicated very distinctly what its future is to be and how it may be related to gas as a co-worker, friend and competitor. Gas managers have ceased to tremble, the public mind has regained its balance, and the two systems of lighting have settled down to quiet and sober work. Each has its own appointed sphere, each its peculiar merits, and if one is capable of developing greater efficiency as an illuminant, so also is the other; if gas can boast of economy, it must not be supposed that its rival has attained finality in that respect either. Physics and chemistry have not yet altered their last words on these subjects, and enterprise and industry still pursue their undisturbed course, working on gas with a faith and hope undaunted by the rivalry of its competitor. Electricians were not afraid to enter an untried field, and are not, therefore, likely to abandon the contest in face of the threatened economies. While this is so, it is also true that as each system of lighting pursues its way, they must in the nature of things continue to advance at very different rates of increase. It is not on the score of cheapness only that gas will advance in public favor, but because it can be made purer, be more easily distributed, and burnt in a manner to yield steady and incandescent light.

The improvements in the manufacture of coal gas have economy in their favor, nearly all the impurities met with in commercial gas being such as have a market value. Seldom a month passes without some record of advance being made in the gas works of European countries. In the United States, gas has always been high in price, owing partly to the higher cost of the raw materials used in the construction of the works, the higher rates of interest paid on capital, partly because legislation is less stringent in the public interest, and partly also to the fact that there is so much to divert enterprise and capital in the South and West. It is in England that the greatest economies have been wrought in the processes of manufacture. The present condition of the gas manufacture of Great Britain represents a degree of completeness unattained in few industries and surpassed by none. Whatever savings have been realized have always been the fruit of applied science and that in a prominent degree. In the best managed works, chemical analyses are constantly made of the raw materials, and the products of distillation. The purifying mediums are also subjected to close scrutiny, so that nothing is lost sight of which may help to improve the processes of manufacture. In a very large number of cases works have adopted the method of retort heating by gaseous fuel. Experience has shown the great economy of a uniform and controlled heat during the working of a charge. When the best possible form of retort is in use there is always a great strain from unequal and insufficient or excessive heating. The best remedy yet found for this defect is the gas furnace, which enables the engineer to work out a charge in about one-half the time it takes when raw coke is burnt. Under this system of heating, not only can a large charge be worked, but it can be done in less than half the time, the retorts have a much greater durability, and the yield of gas shows a larger percentage. Heating by gaseous fuel made from the coke has another advantage, which effects a saving of from 40 to 60 per cent. in the consumption of coke.

Instead of heating but one bench of retorts, as is done with the coke ovens, two may be worked from the single gas furnace. As an instance of what may be done by economical methods the City of Carlisle (Eng-) affords a first-rate illustration. The population is about 35,000 inhabitants: At a recent meeting of the gas committee of the Municipal Council, the chairman stated that in 1868 they obtained but 7,715 feet of gas from the ton (2,240 lbs.) of coal, while now more than 10,000 feet is the quantity, or 34 per cent. increase. In 1868, 2,251 tons of coke were made, against 4,703 tons for the year ending 1882, or double the quantity of coke for an increase of only one-third more coal. Formerly they had but ten gallons of tar, now they get thirteen, the ammonia liquor was 12 or 13 gallons per ton, now it is from 26 to 27. While these improvements have been effected, coal had advanced in value 20 per cent., and wages have also increased. In 1868 the price of gas in this place was 75 cents per thousand feet, now it is 50 cents, with a prospect of further reduction as soon as the additional improvements have been carried out for utilizing those by products now allowed to run to waste. Many such cases might be cited to show what can be done by improved processes of manufacture. Recent examinations of gas in large cities have revealed the fact that some of the impurities and diluents have been removed. A great point to be kept in view is the prevention as far as possible of those deleterious compounds, by guarding against leakages in the retorts, improper heating, and in efficient modes of purification. Much attention is being given to the separation of the carbon anhydride, sulphuretted hydrogen, carbon disulphide and sulphuretted hydrocarbons. The percentages of these substances are to some extent controlled by employing moderate temperatures for heating the retorts, but this does not get rid of the evil entirely, nor does the various schemes of after treatment which have lately been tried dispose of the difficulty. Just now the distinguished gas examiners of the English metropolis are establishing additional test stations with a view of more effectively checking the evil of impurities. Never was applied science more earnestly practised than now, and at no period of our history has sanitary requirements made stronger demands on the resources of manufacturers to reduce to the lowest minimum the impurities of their wares. The threatened competition of the electric light will be found to bear its quota of fruit in obliging gas engineers to get rid of the noxious matters associated with gas. It may be confidently anticipated that ere long the only impurities which the use of gas will be attended with will be those of carbonic anhyride and water—the products of combustion substances which may be ejected from our apartments by suitable appliances if we choose to go to the necessary expenses.

Another important consideration is the distribution of the coal gas from the works to the consumers. At present nearly every city gas works suffers from an excessive leakage, due mainly to imperfect joints in the mains. Wherever there is waste there is a cause of increased cost of the gas. In some places this is simply enormous, the loss by leakage reaching eight millions of feet in a single year, and this in a city of less than 150,000 people. At another place the reduction of the leakage was from 39 to 13 millions, a large waste for a city of ninety thousand people only. In Carlisle it was formerly 15 per cent., now it is 5 only. In Bradford (Eng.) it was high, now it is but 4 per cent. When all the items of waste in a gas works are considered it is not difficult to understand how it is rates are so high.

The combustion of gas for illuminating effects is a matter which the consumer must debate thoroughly for himself. Whether the article be good or bad, high or low in price, it will devolve on him to see that he burns it properly and under the best conditions for the emission of light. The ordinary gas burner is a poor tool at best, which neither governs consumption nor effects steady combustion. The result is very much what we see in the ordinary open fire or a n vice for burning crude coal with a supply of cold air. When pressures are unequal combustion varies, giving a great diversity of illuminating effects. Governed at the meter the waste under the most favorable circumstances is seldom less than 20 per cent., so that some form of efficient governor at the tip is the most

certain of complete working effect. A recent test was made with an ordinary Lara tip, 3 feet burner, and decided some very interesting results:

Common Open Burner.			G. Check Burner.				
Pressure in Supply Pipe.	Pressure Under Tip.	Consumed per Hour.	Pressure in Service Pipe.	Pressure Under Tip.	Consumed per hour	Illuminating Power.	Decrease of Candle Power.
4–10	3–10	3 feet.	10–10	4–10	3 feet.	8 c. p.	.016 p. ct.
7–10	5–10	4 "	15–10	8–10	4.7	11.21	.18 %
9–10	7–10	5 "	20–10	12–10	6	13.06	.32 "
11–10	9–10	6 "	25–10	17–10	7	11.2	.50 "
14–10	11–10	7 "	30–10	21–10	8	10.24	60 "
17–10	14–10	8 "	35–10	26–10	9	10.08	.65 "
20–10	16–10	9 "	40–10	32–10	10.5	10.05	.70 "

Efficient Governor Under Tip.

Pressure on Service Pipe.	Pressure Under Tip.	Consumed per Hour.	Illuminating Power.
10–10	3–10	3	9.6 c.
15–10	3–10	3	9.6
20–10	3–10	3	9.6
25–10	3–10	3	9.6
30–10	3–10	3	9.6
35–10	3–10	3	9.6
40–10	3–10	3	9.6

From the above tests it appears that ordinary burners are most wasteful contrivances for burning gas, and that in proportion to the loss by excessive consumption the illuminating power is decreased. Thus with a check burner, and an average pressure on the service pipe of 25-10, there was exerted under the tip an average of 17.14-10, and which consumed an average of 6.88 feet per hour, giving a light equal to only 10.54 candle power. The efficient governor under the tip would give for the same consumption a light of fully 25.5 candles. The waste in this case is clearly 60 per cent. It is easy to see that whatever improvements which may come to us with reductions of prices, unless our present modes of consuming gas be abandoned we shall not only pay much more than we ought but lose a tremendous amount of the illuminating power of the article. Again, at $2.50 per thousand a consumption of four feet per hour is exactly one cent, and equal to 13.5 candle power, burned under fair conditions. Ten such burners in operation for five hours per day means fifty cents per day or $45.50 cents per quarter. For families of small means this is a serious matter, and for the well-to-do and wealthy it is no less so either. For those who must spend $100 per month on gas bills it is above all important that they should avoid waste and get the greatest possible illuminating results from their consumption.

At a recent exhibition a well-known system of regenerative burner was displayed for the purpose of illuminating large spaces with steady and pure light. It was the invention of the late Dr. Siemens, the celebrated inventor of the regenerative gas furnace for metallurgical work. In this burner the gas is made to pass downwards, and is heated before combustion takes place. The air needed for combustion is also heated to a very high temperature before it comes into contact with the gas, and when combustion takes place an intensely white if not

incandescent flame is the result. An incandescent light is the great object to be aimed at, and is the point at which all gas engineers must aim if they hope to perfect gas illumination. Hitherto this burner has been suitable only for outdoor use, and has found no sphere in domestic service. If the principle can work so well for large open spaces there is little reason to doubt that it may work equally well for domestic requirements, when a burner of smaller capacity has been made. It ought to be quite possible to obtain from a burner of twenty-five feet per hour capacity an illuminating power of at least 100 candles. Experiment in better modes of combustion for light with ordinary gas may give fifty candle power from a burner consuming only ten feet per hour. What has been attained in heating results with the gas furnace may doubtless be repeated in illumination by the adoption of a well-devised system of regeneration of the gas and the air required for its combustion.

In the International Gas Exhibition, shortly to be held in Holland, we may look for some such results. The saving on consumption from the use of such a burner would be enormous, and would far outweigh any reduction in price if the present modes of consumption were maintained. Consider for a moment the comparative value of probable reductions of price and the saving from more economical consumptions. There is no reason why gas should be more than $1.00 per thousand in cities where the coal is laid down at $5.00 per ton and the consumption is 2,000,000 feet daily. In the large towns and cities of England the cost of the raw materials are not nearly so much less than they are here, as to justify the high prices of American gas. The following table will show how they are served as to quantities and prices :

City.	Population.	Consumption Daily.	Cost of Coal.	Price of Gas 1,000 feet.	Consumption per Capita.
Leeds...........	350,000	14,000,000 ft.	$2.50	44 c.	40 feet.
Bradford........	200,000	6,750,000	2.50	60	33.75
Reading	42,000	215,786	4.00	78	5.13
Nottingham.....	180,000	3,158,000	2.75	60	17.5
Carlisle....... ..	35,000	406,300	2.75	50	11.6

Four of these five cities own their own works and run them to pay interest on the capital at four per cent., and to make a small profit which is used for improvements besides free lighting of the public streets. Reading works are owned by a company which is paid to do the street lighting, and makes profit enough to pay a dividend of eight per cent. only. It will be seen that coal costs $4.00 per ton, the daily consumption is less than a quarter million feet, and the average quantity used five feet only, while the price is seventy-eight cents the thousand feet. Suppose this little city be taken to represent the cost of coal to gas companies in American cities of more than one hundred thousand, and we take the per capita consumption at ten feet, then, with one million feet daily consumption any company might supply gas at $1.25, and pay 10 per cent. dividend. Now, if the reduction in the price is just fifty per cent. all round, how much better off will the average consumer be if we continue the present wasteful methods of gas consumption? Very little indeed. By the adoption of some efficient governor a saving of from 25 to 50 per cent. may be realized, and if some inventors will give us a regenerative burner for domestic use we may have an incandescent gas light of high power, which may enable us to enjoy an illuminating power of fifty candles with a consumption of ten feet per hour. The most efficient burner we now have hardly exceeds 18 candle power for five foot capacity, or barely 40 candle power for a ten foot burner. If we can regenerate the gas and air required for its combustion, it is a very moderate expectation to hope for an increase of only twenty per cent. in the lighting power of the illuminant. A chandelier fitted with three five foot burners of eighteen candle power would

consume 15 feet per hour and give the yellow light we now get of 54 candles. A ten foot regenerative burner of fifty candle power would give us a light nearly incandescent. If such can be realized, and there is little doubt that it can, our gas bills may be reduced to the extent of 75 per cent. on what we now pay. The cost of gas for light must soon undergo reduction, but so must the quantity consumed be cut down. Reform must commence first at the burner's tip, combustion must be more complete, and given a gas of fair quantity, the nearer we come to it the better and cheaper will be our light.

WHAT IS FRICTION?

From "The Engineer."

It is now just a century since Coulomb first investigated the laws of friction, and half a century since Morin made at Paris the series of experiments which has rendered his name immortal; and yet it would hardly be too much to say that it is only at the present moment that we are beginning to arrive at a clear conception of what we mean by so familiar a term. In saying this we by no means wish to insinuate the slightest disparagement of the illustrious physicists we have named. The fault lies not with them but with us. They had no desire—in the case of Gen. Morin, at least, we have his own authority for saying so—to impose their investigations on mankind as the last word of science, as absolutely and everywhere true, beyond as well within the limits within which they were tried. They claimed to have laid the foundations and to have laid them aright, but they looked for other workmen to come forward and complete the edifice. Until very recently, however, such workmen have been less than few, their contributions more than scanty. To the past generation of engineers, immersed in the practical details of construction, and in the thousand-and-one cases of commercial manufacture it was much easier to take Morin's results as they stood, and work by them, than to investigate the question any further for themselves. The same spirit of indifference has crept into our text-books; which quote Morin's results—with or without the courtesy of mentioning his name—as if they were no less rigidly true and general than the theory of gravitation itself. Yet it required the labors of a whole generation of astronomers to place Newton's theory beyond the reach of cavil; while the question of its possible limitation remains in dispute to the present day. In the sharpest contrast to this keen activity on the part of the votaries of science, the question of friction, whose practical importance it is scarcely possible to overrate, has been allowed to sink back, after the light flashed on it by the experiments we have referred to, into a hazy twilight, from which it is only just beginning to emerge.

To illustrate the present state of the case, let us begin with the treatment of friction, as it will be found in any standard book on "Applied Mechanics." First, we shall probably find a distinction drawn between statical friction, where the two surfaces are initially at rest, and dynamical friction, where they are already in motion. There we shall find a statement of what are called the "Laws of Friction," in something like the following terms:

(1) Friction, whether statical or dynamical, varies directly as the force which presses the two surfaces together.

(2) This force remaining the same, it is independent of the area in contact.

(3) Under the same conditions the value of dynamical friction is much less than that of statical friction, but it is constant at all velocities. To the statement of these laws may be added, in more elaborate and theoretical treatises—such as Moseley's "Engineering and Architecture"—a few words as to the limiting cases in which the laws cease to be exact, as, for instance, where the pressure approaches that of abrasion; and also of the state of things which prevails when the surfaces are fully lubricated with oil or grease, in which case

Morin concludes that the friction, whatever the nature of the surfaces, approaches to a constant value at between 7 and 8 per cent. of the pressure. Then will follow tables, taken almost exclusively from Morin's results:

(*a*) For plane surfaces at rest, sometimes dry, sometimes wet, sometimes lubricated.

(*b*) For plane surfaces in motion, under similarly varied conditions.

(*c*) For gudgeons or axles revolving upon their bearings, and more or less lubricated with ingredients of various descriptions.

In collections of formulae and rules, such as those of Molesworth and Rankine, these tables in an abridged form will be found to be the whole that is offered upon the subject. So deeply rooted is this "orthodox" doctrine, that we are acquainted with but one work on mechanics in which it is even hinted that the third law, as to dynamical friction, is by no means universally true; or that the friction of dry and lubricated surfaces are not phenomena of the same character. Yet scepticism on these points has long existed, but it is only within the last few years that it has broken out into open rebellion. We are now able to assert positively two facts of which the compilers of our text-books have not had the slightest glimmering. The first is that what is called friction in the case of dry surfaces, and what is called friction in the case of fully lubricated surfaces, are not analogous phenomena, but totally different in every respect, observing different and even contrary laws, and having nothing whatever, but an unfortunately chosen name, to bind them together. The second is that dynamical friction is constant for similar surfaces only within comparatively narrow limits of velocity; and that beyond those limits it either increases or diminishes, as the speed varies, in a very unmistakeable manner. It is evident that these two facts completely overthrow the sweet simplicity of the laws and tables of friction as they appear in our existing manuals.

It is worth while to dwell for a moment on the steps by which this change in our view of the question has been brought about. As long ago as 1852 the experiments made by Poirée and Bochet on shoe brakes and on the wheels of railway vehicles sliding on rails showed that the co-efficient of friction diminished very much as the velocity increased. Between the limits of 900 ft. and 3600 ft. per minute the coefficient of friction in the case of wheels sliding on rails diminished from .2 to .13. It is obvious that this is altogether contrary to the so-called law of dynamical friction, but it does not seem to have really awakened the sense of engineers to the question. There is nothing further chronicled until 1877, when Professor Kimball presented to the Royal Society a paper on the relations between friction and velocity. At ordinary speeds he found that the friction between pieces of pine wood diminished rapidly as the speed increased. Again, with a wrought iron shaft 1 inch diameter, running in a cast iron bearing and well oiled, an increase of velocity from 6 ft. to 100 ft. per minute caused the co-efficient of friction to fall as low as three-tenths of its first value. The same result was found with lower pressures, the pressure having in the first case been 77 lb. per square inch. About the same time Professor R. H. Thurston was carrying out in America a number of experiments intended to test, under varying conditions of speed, temperature, pressure, &c., the friction of well lubricated journals. These were subsequently published in his well-known book, "Friction and Lubrication." As to velocity, his conclusion was that the co-efficient of friction at first *decreased* with increase of velocity, but after a certain point *increased*, and that the point of change is different at different pressures and temperatures. On the whole he considers that, with well lubricated bearings, the friction increases with the velocity at all speeds exceeding 100 ft. per minute, and that the rate of increase is approximately as the fifth root of the speed. Almost contemporaneously with these researches of Prof. Thurston, another American, Mr. George Westinghouse, was carrying out, in conjunction with Captain Douglas Galton, the magnificent series of experiments on the brake question which have since become classical under the name of the "Galton-Westinghouse Experiments." These threw much light upon the question of friction as between metals—generally cast iron and steel—which were rubbing over each other without lubrication, and at

very high speeds. In every case they showed a remarkable diminution of friction as the speed increased. This result held throughout the whole range of the experiments, in which the speed varied from 400ft. to 5300ft. per minute. It should be observed, however, that, owing to the nature of the instruments used, the observations only lasted half a minute, and it was found that during that time the coefficient of friction continued to diminish. The ultimate values assumed by it under different circumstances cannot, therefore, be exactly known; but from the appearance of the curves, obtained by plotting the results, it is clear that the values for high speeds would still be much smaller than for low speeds. Professor Kennedy has deduced from the experiments the result that the coefficient of friction was sensibly less at high than at low pressures, and that between the wheels and the rails—where the pressure was, no doubt, far greater than that on the brake blocks—the friction was not more than one-third of the amount found for the latter. This experiment is in accordance with Professor Thurston's results as to pressures, with ordinary velocities and loads; but the latter found that after a certain point a change took place, and further increase of pressure occasioned an increase in the friction. These results varied greatly under various circumstances, and they applied to lubricated journals, which, as we have seen, are really in altogether different circumstances from those of dry friction, as illustrated by the behavior of brake blocks.

Such was the state of the case when the Institution of Mechanical Engineers took up the question. Their progress in determining it has certainly been of the slowest; but they have lately issued a report which consolidates and advances our knowledge of the question in a remarkable degree. The experiments, which were conducted with great care by Mr. Beauchamp Tower, were first directed to ascertain the friction of journals under the best possible circumstances of lubrication; in other words, with a journal running in what may be described as an oil bath. By this it is not meant that the journal was absolutely buried in oil, but simply that its lower surface was always in contact with fresh oil, the upper surface being that on which the pressure rested. The results of these first experiments were very remarkable. In the first place, it was found that the absolute friction, that is the actual tangential force per square inch of bearing required to resist the tendency of the brass to go round with the journal, was much smaller than had ever been suggested before, falling in many cases as low as $\frac{1}{1000}$ of the pressure existing on the same area; secondly, it was found that this friction was nearly constant under all loads within ordinary working limits, and certainly it did not increase in direct proportion to the load, as writers on friction have always assumed. It only began to vary considerably when the pressure became excessive, and then the friction rose very rapidly and the bearing heated and seized. From this result it was naturally deduced that the friction of bearings in such circumstances is rather liquid than solid friction. The theory of liquid friction is that it is independent of the pressure per unit of surface; is directly a dependent upon the extent of surface, and increases as the square of the velocity. In the case of these oil-bath experiments the friction, as we have seen, is nearly independent of the pressure, and it was also found to increase with the velocity, at least with speeds beyond 150ft. per second. The question of its variation according to the surface in contact was not gone into. As regards other results, it appears that an increase in temperature caused a very marked diminution in the friction. For instance, with lard oil, the coefficient with the temperature at 120 deg. Fah. was only one-third of what it was at 60 deg. Fah. This is in accordance with previous results, but shows remarkably the advantage derived from keeping bearings warm. Again, it was discovered, though by accident, that the pressure existing in the film of oil at the top of the bearing, where the external pressure was highest, was very large; indeed, so great as to force the oil up through a small hole against a pressure of at least 200 lb. per square inch, this pressure being more than double the mean load on the horizontal sections of the journal.

Subsequent experiments with ordinary methods of lubrication were by no means so satisfactory. The methods for introducing the lubricant, which are found to answer in the case of railway vehicles,

were found to fail altogether with this experimental journal. The cause is attributed, and no doubt rightly, to the absence of any shock or vibration in this case, such as goes on continually with a railway vehicle in motion. Fair results were, however, obtained by using an oily pad, pressed lightly against the under-surface of the journal. Although the supply of oil was so small that the journal scarcely felt greasy, yet the bearing carried about 500 lb. per square inch; but in this case the results approximated much more closely to the laws of solid friction. The coefficient was approximately constant at about 1 per cent. of the load, and no very definite variations of friction with the speed could be observed. The lubricating of the journal by means of side grooves fed from a siphon lubricator was also successful, and gave somewhat the same results, as far as the constancy of the moment of friction is concerned, with those obtained by the oil bath; but the absolute amount of the friction was about four times as great. Now it will be observed that these results are practically coincident with those of Professor Thurston, and may be taken to establish the first of the two facts with which we started, viz., that the friction of thoroughly lubricated journals is a totally different phenomenon from what is commonly known as friction between dry surfaces. A complete reformation in the treatment of the subject by text-books, and in the tables supplied therein, becomes an imperative necessity.

It will be observed that none of the investigators we have mentioned commit themselves to any theory as to the real nature of friction, whether solid or liquid; they are content to record facts, and leave others to frame hypotheses from them. It is, however, a well-known rule in the history of science, that the most fruitful progress in any department is made under the influence of some definite hypotheses, which it is the object of experimenters to confirm or disprove. Any physicists, therefore, who would put forward a good working hypothesis on the question of friction, or rather on the two questions of solid and liquid friction, would probably deserve well of the engineering profession and the world at large. There are plenty of problems, besides those we have indicated, which such a theory should embrace. For instance, we know of a case some years ago where steel tubes were manufactured by pulling an annular ingot of steel, in the cold state, through an opening in a plate, much after the fashion of wire-drawing on a large scale. In this process the finished tubes as they came out were generally perfectly cool, a result which probably few would have expected; on the other hand, one would occasionally appear which was sensibly warm, if not hot. But the cause of this was well known by the workmen engaged in the manufacture; it could always be traced to the presence of a minute piece of grit or other substance which had got into the hole, and had drawn a fine scratch upon the surface of the steel as it passed through. This is surely a remarkable fact. We do not at all say that it is impossible or even difficult of explanation, but we may at least commend it to the attention of our readers; and it would not be hard to mention many others of the same kind. To attempt an answer to these problems would lead us far beyond our present limits, but taken in conjunction with the experiments here described, they may at least justify us in putting forward the question placed at the head of this article, viz., What is friction?

M. Bourdalou, having stated in 1864, in his work, "Nivellement Géneral de la France," that the average level of the Mediterranean is by 0.72 metre lower than that of the Atlantic, this result was received with some distrust by geodesists. General Tillo points out now, in the last issue of the Russian *Izvestia*, that this conclusion is fully supported by the results of the most accurate levelings made in Germany, Austria, Switzerland, and Spain, which have been published this year. It appears from a careful comparison of the mareographs at Santander and Alicante by General Ibanez, that the difference of levels at these two places reaches 0.66 metre, and the differences of level at Marseilles and Amsterdam appear to be 0.80 metre when compared through Alsace and Switzerland; the Comptes Rendus de la Commission Permanente de l'Association Geodesique Internationale arrive at 0.757 metre from the comparison with the Prussian levelings, whilst the fifth volume of the "Nivellements der Trigonometrischen Abtheilung der Landesaufnahme" gives 0.809 *viâ* Alsace, and 0.832 *viâ* Switzerland. The difference of levels at Trieste and Amsterdam, measured *viâ* Silesia and Bavaria appears to be 0.59 metre. Each of these four results—0.72, 0.66, 0.80, and 0.59—having a probable error of 0.1 metre, their accordance is quite satisfactory, and we may, *Nature* says, admit thus that the average level of the Mediterranean is in fact lower by 0.7 metre than that of the Atlantic.

THE LONG-COLUMN FORMULA—A REVIEW OF THE CRITICISM OF PROF. BURR.

By S. W. ROBINSON, C. E., Prof. Mech. Eng., Ohio State University, Columbus, Ohio.

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In this Magazine for November, 1883, Professor William H. Burr directed attention to a column formula first published in June 1882, and in terms not the most complimentary. Pressure of duties and engagements has prevented me from earlier pointing out the existence of overdrawn statements in the criticism, statements, indeed, which can easily be shown to be more applicable to the criticism itself than to the criticised.

Relative to applying the theory of flexure to columns, it will probably be admitted by most thinkers, that when the ultimate resistance of a column is less than that of a short block of the same section and material, it is because the column gets out of line under load. In support of this, it will be conceded that if the column has side stays so numerous as to entirely prevent the deviation of the column from a straight line, the supporting power of the piece will be that of a "block." It is equally certain that the stiffer the piece is, that is, the higher the co-efficient of elasticity, the more will it stand before the deviation from line occurs. Again, when a column fails, under load, it appears to be certain that the greater the ultimate compressive resistance of the material the greater will be the load-sustaining power of the column. These considerations warrant the conclusion that a rational formula for columns, applicable within certain limits of length over some function of diameter, should contain both the coefficient of elasticity, and the modulus of crushing; except for such material as cast iron where, beyond a certain length, the modulus of tenacity displaces the modulus of crushing with some modification of form of expression. Again, beyond a certain length for a given diameter and section, deflection will become so great before any crushing occurs, that the failure, by deflection alone, of the column is wholly independent of the modulus of crushing, for which the formula will contain only the coefficient of elasticity to account for properties of the material, as shown by Euler's "the oldest column formula." These notions are sanctioned by eminent European authority, as well as formulas in accordance therewith containing the coefficient of elasticity and modulus of rupture, and reduced to practical form for cast-iron, wrought-iron, and wood. (See Sankey's Ritter, p 343). These formulas, as well as my own, are assailed to equal extent by the remark, "it is not possible to produce a formula," "based upon the common theory of flexure" that will give "results agreeing closely with actual tests" "unless the columns are excessively long." (Euler's formula being in mind in the last remark.) It is proposed to show, further on, by computed results compared with experimental ones, that it is, instead of "is not" "possible to thus produce a formula which shall represent with tolerably close approximation the ultimate compressive resistance" of bridge columns.

Professor Burr states that the common theory of flexure becomes only approximate for pieces under combined compression and flexure, at a limit, which according to experiment, is not below a length of two hundred radii of gyration of section. If he means by this that that theory is only approximate for beams in general, except within the elastic limit, he may be indorsed. But if he means that beyond this, the theory is not applicable to columns as well as to beams, there must be dissent, at least to such extent as flexure precedes failure, because the stresses are the same in kind in the column as in the beam, accompanied with a neutral axis, and terminate in the same way when carried to excess, viz.: by crushing or tearing of some portion of the cross-section of the piece. Hence the Professor should revise his statement, and make it read, "within 200 or more radii of gyration of section, the

common theory of flexure serves for columns as for beams carried to rupture, while beyond the 200 radii it applies as to beams within the elastic limit."

That the limit is not less than 200 radii, is probably true, my criterion formula (106) being that to fix its value. This for ordinary channel columns with flat ends, gives the length at about 250 radii of gyration. At this limit my formula gives the same results as Euler's. The latter formula, for failure of columns by springing out of line, is therefore to be used for computing columns of greater length, and the former for failure by combined springing and crushing for shorter columns, down to a length in the neighborhood of 20 to 30 radii of gyration of section. Thus, instead of "proving altogether too much," the new formula proves about right; it being that upon which the criterion formula depends. Experimental tests will be cited in support of the same point further on.

Another objection to the new formula is in the form of a doubt of its agreement with experiment through a wide range of length over radius of gyration, the range which I have previously cited being regarded as too limited. Though this range cited reaches about to 150, and includes nearly if not all bridge members, yet I shall, further on, show that it will work up to twice this value; indeed up to where the criterion calls for Euler's formula, from which limit the latter formula will be compared in continuation of length over radius of gyration to over 600.

One of the strongest points which Professor Burr doubtless flatters himself for having made, is to the effect that "as a matter of fact he takes the compressive resistance of a long column as given by the oldest column formula" (viz. Euler's), "and introduces it into his own equations," and hence if the resistance of the column is thus made to depend on "Euler's formula, what is needed if his eqs. (97), (98), (99), and (100)?" But, unfortunately for this flattery, Euler's formula need not be mentioned or thought of in producing my formula. Though it was used for convenience sake, yet the object was not to inoculate my formula with Euler's and thus impart virtue to the former. The object was to obtain an expression for the radius of curvature of the axis of the column at the point of greatest deflection.

To produce my formula without the slightest reference to Euler's, take eq. (93) as written by aid of Fig. 21, viz:

$$ad=\frac{t}{\varepsilon}=\frac{y_1+d_1}{\rho}, \qquad . \quad . \quad . \quad (93)$$

to start with. Next, knowing that the axis of the column, when deflected under load, is a sinusoid, its equation may be written in the general form

$$y=a \sin v.$$

Now for Fig. 18, of the original article, regarding l as the length AB of the column with rounded ends, and taking the origin of co-ordinates at A, we have $y=o$ for $v=o$; also $y=o$ for $v=l$. To meet the last condition put

$$v=\pi\frac{x}{l}$$

giving

$$y=\text{a} \sin\pi\frac{x}{l} \qquad (a)$$

In this, $y=o$, for $x=o$ or for $x=l$, as required for this particular curve. The deflection at the middle of the column being greatest call it y_1 and for this x should $=\frac{l}{2}$. Introducing these for y and x, and

$$y_1=a.$$

Putting this value of a, into equation (a), we obtain for the equation of the curve of the axis of the particular column of Fig. 18,

$$y=y_1 \sin\pi\frac{x}{l}, \qquad . \quad . \quad . \quad (b)$$

Now, to find the radius of curvature for the middle point of the column to introduce in (93), differentiate (b) twice, and introduce the differential co-efficients into the differential equation for the radius of curvature, viz.:

$$\rho=\pm\frac{\left(1+\frac{dy^2}{dx^2}\right)^{\frac{3}{2}}}{\frac{d^2y}{dx^2}} \qquad (c)$$

Hence from (b) we obtain

$$\frac{dy}{dx}=y_1\frac{\pi}{l}\cos\pi\frac{x}{l}$$

and

$$\frac{d^2y}{dx^2} = -y_1\frac{\pi^2}{l^2}\sin\pi\frac{x}{l}$$

whence

$$\rho = \frac{\left(1+y_1^2\frac{\pi^2}{l^2}\cos^2\pi\frac{x}{l}\right)^{\frac{3}{2}}}{y_1\frac{\pi^2}{l^2}\sin\pi\frac{x}{l}}$$

for the radius at any point. At the middle of the column $x=\frac{l}{2}$, and ρ becomes

$$\rho_1 = \frac{1}{y_1\frac{\pi^2}{l^2}} = \frac{l^2}{\pi^2 y_1}, \quad . \quad . \quad (d)$$

If the curve of the axis of the column be regarded as a circle instead of a sinusoid, with the origin of co-ordinates at the middle of the column and on the axis, we find the radius of this circle to be the radius sought. The equation of this circle is

$$y\,(2\rho-y)=x^2$$

or dropping y as small compared with 2ρ and, observing that $y=y_1$ for $x=\frac{l}{2}$, we obtain

$$\rho_1 = \frac{l^2}{8y^1} \text{ approx.,} \quad . \quad . \quad (e)$$

which is the same as eq. (d), except 8 takes the place of π^2; a difference due to the difference of the curves, and one which would not very much affect the results given by the final equation for columns.

If the curve of the column's axis be assumed to be a parabola with vertex at $\frac{1}{2}l$, we obtain

$$\rho = \frac{l^2}{8y_1} \text{ exact,} \quad . \quad . \quad (f)$$

Combining (d) with (93), we obtain an expression for y_1 which is free from ρ, and the same as (96) of the original article. That value of y_1 combined with (96a), viz:

$$t = \mathrm{T}y_1\frac{d_1}{\mathrm{I}_1} + \frac{\mathrm{T}}{\mathrm{K}}, \quad . \quad . \quad (96\,a)$$

results in an equation, which solved for T, is found to be my column formula (97) for round ends.

Had we combined (f), instead of (d) with (93), the resulting formula (97) would have had 8 in place of π^2, the latter being preferable from the fact that the elastic curve of the column is known to be a sinusoid instead of a parabola or circle. Euler's column formula is not required for showing this fact.

By comparing Fig. 19 with Fig. 18, the equation of the axis curve can be stated for flat end columns. Working out the radius of curvature similarly as above, and applying it in (93), the column formula for flat ends results. Likewise for flat and round ends.

Hence, Professor Burr's own expression becomes applicable here to himself, viz., "nothing could be more erroneous" than to state that my formula depends upon the ancient formula of Euler's, or that I take the compressive resistance as given by it and introduced it into my own equation. Also, the argument drops equally flat as found in the flimsy statement that "with a little consideration the reason is obvious" why my formula does not vary more from actual test results, viz: because it is "such a melange of Euler's formula, and of that for blocks as not to give results at random, but which are far enough from expressions of a true law." Professor Burr should try to avoid drawing statements with such haste, as to leave them with no foundations in fact, and take a second look before declaring my column formula simply Euler's, doctored with such a heavy dose of errors as not to give results at random.

Another example of this sort of argument is found in the statement that "a Phœnix column is equally liable to fail in any direction," ends flat, "while d_1 has different values in different directions." Testimony opposed to this statement is found in several good published authorities, among them the recent work by Prof. W. H. Burr on *The Elasticity and Resistance of the Materials of Engineering*, an authority which my opponent will doubtless accept as sound. On page 448, the formulas of C. Shaler Smith, C. E., are quoted, three of which are for Phœnix columns. These formulas, in conformity with the original Gordon's, contain d, the least diameter as is supposed, though no statement is found in the book to indicate what diameter. But as d is the only quantity in these formulas referring to the cross section of the col-

umn, it is certain that different result for strength is obtained when d is taken the least instead of another diameter. But as the formulas are given without adverse comment, the supposition is that they are acceptable to the author of the work, or that, so far, the column is most likely to deflect in the direction of its least diameter when failure occurs, this being the supposition respecting d in framing formulas containing the diameter. Other authorities than Gordon, Smith, and Burr indorse the same formulas, and also formulas differing in form of expression, as for instance Prof. Wm. Cain, C. E., in an article in this Magazine, Vol. XVII., p. 459.

But, without borrowing authority, if it be granted that the incipient flexure is as likely in one direction as another, as seems conclusive as far as considerations of the moment of inertia of the cross-section of the Phœnix column go, this one fact asserts positively the conclusion that the column will, as a general rule at least, commence fracturing on a remote edge of a rib in Phœnix columns instead of on the body of the tube between ribs; that is, where the radius is greatest instead of least or indifferent; because, with equal flexural tendencies, as for fixed or pointed ends, the column itself remaining fixed with respect to rotation, the slightly curved axis of the loaded column will admit of free rotation about the primitive unflexed axis, the latter line becoming an axis of a conoid of revolution described by the bent axis. Such rotary movement would find no resistance as far as the moment of inertia of the Phœnix section can affect it. But in this rotary movement of the flexed axis, those elements of the column which are most distant from the axis suffer the greatest strain due to the flexure, and hence as the strain is increased under the above action we must look for initial rupture on the edge of a rib, viz., at the distance d_1.

This is true for all column sections for which the moment of inertia is constant for all axes, such as the square section, even hollow, if the section of the cavity is similar and symmetrical with respect to a pair of axes at right angles to each other (Burr, p. 412). Hence, for a square column, the failure will, as a rule, be in the direction of the diagonal of the section, and in Phœnix columns it will be in the direction of the flanges, &c. These conclusions are supported by experimental test—the largest proportion of the Phœnix column tests cited by my critic, deflected in the direction of the flanges as the columns failed under load. See Trans. Am. Soc. C. E., Vol. XI., June 1882.

A Phœnix column laid across supports and loaded for transverse strain as a beam will, according to classic engineering literature, offer a resistance

$$\frac{\mathrm{RI}}{d_1}=\frac{\mathrm{RA}k^2}{d_1}$$

where R=modulus of rupture, I=principal moment if inertia of the cross section k=its radius of gyration, d_1=the distance to the most remote fiber on the side of initial rupture, and A=the section.

According to this the Phœnix column (beam) will be weakest when the flanges are in the plane of flexure, because I or k is constant for all positions of the cross sections, while d_1 varies with position of flanges, and is greatest when the flanges are in the plane of flexure as stated. When I or k is constant, and d_1 variable, the numerator of the above expression is constant, while the denominator is variable, making the expression least when d_1 is greatest. From this it is certain that if a Phœnix column (as such) is admitted to be in any measure subject to flexure under load, it must be more liable to fail by deflection in the direction of a flange than otherwise. It may be observed, further, that as a result of flexure, columns having the same constant I and variable d_1s, the weakest will be those having the greatest value of d_1. For a single column the same may be stated with reference to k and d_1, as indeed is stated above for Phœnix columns; and, in general, the strongest column with given weight and length is nearly that which has the greatest value of k over d_1. These truths are not recognized in any of the 38 formulas found in a recently published work, or any other formulas which have come to my notice except the one called in question by my critic; truths which are as essential to correct column formulas, as are the fundamental truths to any correct formulas. These statements relative to d_1 apply only to columns varying in length

from 20 to 30 radii of gyration up to about 200 or 300.

I might indeed add that there are cases where the column will even be theoretically strengthened by cutting off portions of these outstanding flanges. This is the case with Phœnix columns when computed by my formula, the entire removal of the flanges affecting the strength but slightly. This is analogous to the strengthening of triangular beams by removal of a large percentage of the sharp edge. (See *Trapezoidal Beam* in Professor Woods' "Resistance of Material," about p. 170). Columns with triangular sections are also likewise strengthened, for deflection in a particular direction.

The remark that I "hold that Euler's formula contemplates only pure bending, and not combined bending and compression," is an error, and due to a most palpable oversight; that is, if the term bending is intended to signify deflection within the elastic limit, or an elastic spring of the column under load from its original line. This is plain from the fact that Euler's formula, (85) &c., in the original article, are deduced from equations worked out for simultaneous action of both compressive and transverse forces, from which the latter are finally dropped, and with the supposition that the former may deflect the column, causing flexure. That the column is supposed to fail by "springing out without breaking," does not debar the idea of compression. It is supposed that the column has failed when it has sprung badly out of line, even though no crushing has commenced and yet compression is experienced.

In comparing the formula with experiment, we are at liberty to use our judgment as to the proper value of the crushing resistance and coefficient of elasticity proper to the material in hand. The short blocks of Phœnix column section tested with the columns, indicate that a higher value of the resistance to crushing than 40,000 should be taken in computing for the 10 Phœnix columns cited. Using 42,000 for t, 31 million for E, and the distance to the edge of the flange for d_1, then by substitution into my formula, viz. (See *Science Series*, No. 60):

$$\frac{T}{K}=\frac{t}{1+\frac{d_1^2}{2k^2}\left(\sqrt{1+\frac{at\,l^2}{\pi^2\varepsilon d_1^2}}-1\right)} \qquad (1)$$

we obtain

$$\frac{T}{K}=\frac{42000}{1.82\sqrt{1+\frac{1}{27000}\frac{l^2}{k^2}}-.82} \qquad (2)$$

For both flanges cut off as suggested above, we obtain

$$\frac{T}{K}=\frac{42000}{\sqrt{1+\frac{1}{15000}\frac{l^2}{k^2}}} \qquad (3)$$

both of which give practically the same values for the Phœnix columns cited, viz.:

$l \div k$.	Experiment.	Above Formula.
112	34,650	30,900
100	35,150	32,510
88	35,000	34,140
76	36,130	35,730
64	36,580	37,280
52	37,000	38,710
40	36,440	39,940
28	40,700	40,970
16	50,400	41,650
27	57,200	42,000

Mean discrepancy, 3778.

These results compare better than those presented by Prof. Burr in the November Magazine, due mainly to the fact that the constant t was taken too small for material found in Phœnix columns, and to an unsuitable value of d_1.

To indicate how the above results compare with such as obtained from other formulas for the same Phœnix columns, apply the Rankine formula,

$$\frac{T}{K}=\frac{36000}{1+\frac{1}{36000}\frac{l^2}{k^2}}, \qquad (4) \text{ or } (114)$$

a general formula for wrought-iron columns. For this we obtain the

mean discrepancy=7645.

From a general formula much used by leading bridge companies, where the numerator of (4) is taken at 40,000.

mean discrepancy=4452.

And from a formula found in the *Elasticity and Resistance of the Materials of*

Engineering, page 442, especially for Phœnix columns, the

mean discrepancy $=3685$.

Of these, only the latter result is found superior to that of the new formula, and in this case less than three per cent. It may be remembered that this superior formula is empirical, deduced from Phœnix column experiments, and not fit for other columns, nor claimed to be. This point is mentioned because a general formula is desirable for use in general specifications, into which it is not convenient to introduce several dozen formulas for as many cross sections which might be proposed by bidders.

In applying the new formula above, the ends have been treated as though fixed, for two reasons: first, because the columns were especially prepared by double riveting of segment joints at ends; and, second, because of the indifference of the flanges upon the results as pointed out above.

Doubts for the new formula were expressed by my critic for its having compared with experiment only through a limited range. To compare through a wider range, experiments of Hodgkinson and others are cited. First, from *Elasticity and Resistance of the Materials of Engineering*, p. 473.

CYLINDRIC TUBES WITH FLAT ENDS, RANGING FROM 20 TO 120 INCHES IN LENGTH, AND $1\frac{1}{2}$ TO 4 INCHES IN DIAMETER.

$l \div k$.	Ultimate Resistance. Experiment.	By New Formula.	Rankine's, or Gordon's Formula.
240	14670	15430	13850
179	23206	19650	19000
160	21900	21370	21040
141	29800	23350	23190
120	27670	26490	26310
120	31180	25840	25710
90	29790	30000	29390
89	33300	30170	29510
87	26960	30440	29740
80	29330	31500	30570
80	30000	31500	30570
71	35100	32870	31580
67	26800	33470	32010
65	33330	33770	32220
60	34220	34510	32730
45	32200	36620	34080
45	36980	36620	34080
40	35660	37300	34470
40	36000	37300	34470
39	36910	37380	34500
39	39570	37380	34500
35	36490	37820	34800
28	37390	38610	35210
21	48200	39030	35570

In adapting the new formula to these tubes, we find for all very nearly

$$\frac{d_1^2}{2k^2}=1\tfrac{1}{8}$$

$$a \text{ in eq. (1)} = \frac{4}{\left(1+\dfrac{k}{d_1}\right)^2}=1.44, \quad . \quad (117)$$

see original article, or *Science Series*, No. 60, p. 130, and taking $t=40{,}000$, $E=30$ millions, we obtain from. (1) for the resistance per square inch,

$$\frac{T}{K}=\frac{40000}{1\frac{1}{8}\sqrt{1+.000084\dfrac{l^2}{k^2}}-\frac{1}{8}} \qquad (4)$$

from which the quantities in the third column of the above table were computed.

From the table we find the

	New Formula.	Rankine's Formula.
Mean discrepancy regardless of sign.....	2501.	2967.
Dropping last result in the table do.	2211.	2541.

By noticing the value of l over k in the above table, it is seen to cover the entire range, and even more called for by Prof. Burr at the outset of his criticism, within which the stress is one of combined bending and compression.

But in the following table we find a still greater range of l over k, one indeed which goes beyond the limit set by the criterion formula, viz:

$$\frac{l^2}{k^2}=\frac{4}{a}\left(\frac{d_1}{k}+1\right)\frac{\varepsilon\pi^2}{t}, \qquad (106) \text{ or } (116)$$

(a being the same as in eq. (117)), below which value of l over k the new formula is to be employed, and beyond which Euler's. See *Science Series* No. 60, p. 109.

Solid Rectangular Bars with Flat Ends, Ranging from 4 to 120 Inches in Length, and from 1″ Square to about 6×1 Inches in Section, Including 3×1,3×½ Inches, &c.

$l \div k$.	Experiment.	New Formula.	Rankine's, or Gordon's Formula.
643	2410	2550	2885
540	3380	3660	3956
414	4280	6220	6250
400	5630	6662	6604
311	9600	10680	9921
300	9750	11180	10290
272	10170	12400	11790
270	12970	12410	11900
207	18070	16040	16440
207	17700	16040	16440
206	16850	16110	16500
204	19990	16260	16620
200	17270	16560	17050
135	27770	23220	23900
104	29660	28000	27690
100	25330	28720	28180
50	34550	38790	33600
25	48680	43610	35390
13	50400	44500	35830

The experimental results in this table are found in the *Elasticity and Resistance of the Materials of Engineering*, p. 474.

The new formula takes the form

$$\frac{T}{K} = \frac{45000}{\frac{3}{2}\sqrt{1+.00009\frac{l^2}{k^2}}-\frac{1}{2}} \qquad (5)$$

and the criterion fixes the limit at about $l=300k$, beyond which Euler's formula was applied, viz:

$$\frac{T}{K} = 1066000000\frac{k^2}{l^2}, \qquad . \quad . \quad (6)$$

In (5), a has the value 1.6.

In (6) this is omitted for the reason that no crushing or cocking is supposed to take place at the end bearing until after the column has sprung out in failure.

From this table we find the

	New Formula.	Rankine's Formula.
Mean discrepancy regardless of sign......	2230.	2736.
Dropping last result in table, do.	2026.	2073.

The last result in this, and in the previous table, is proposed to be dropped, for the reason that Rankine's formula is considered by some as inapplicable below where $l \div k$=about 25 to 30.

The experimental results of the last table are those from which the constants in Rankine's (sometimes called Gordon's) formula, were in the main determined, giving the formula the specific form stated in (4).

It is therefore fair to this formula to compare its results with those by the new formula, as done in the 3d and 4th columns of the table.

We therefore find that the new formula, which was not made with a view to representing the experimental results of this table any more than all others, when placed in comparison with a formula which is made to do its very best by special calculation of its constants from these very results of test, does not appear at a very great disadvantage.

The high value of t=45,000 was taken, for the reason that the solid, flat, English bar iron experimented upon may be supposed to have a high crushing resistance.

Only one more comparison will now be made with experiment, and with other formulas. The results of experiment are quoted from the same work as previously, p. 476.

Angle Bar $3\times3\times\frac{5}{16}$ Inches with Flat Ends.

$l \div k$.	Experiment.	New Formula	Rankine's Formula	Special Formula
100	23600	25400	28180	23740
80	29480	29190	30850	26760
60	35380	33480	32730	30860
30	39400	37270	35120	43600
Mean of discrepancies regardless of sign		1530	3220	2895

The radii of gyration are taken for a diagonal axis, for the reason that it is less than that for an axis parallel to a leg of the angle which seems to be that given in the table quoted from. The last column is quoted from the same work, p. 477, computed by a special empirical formula probably by aid of radii given in the table.

In the new formula the compressive resistance is taken at t=42000. The angles being of English iron. Also e=25000000, a=1.8, k=.6, d_1=1.22, and

$$\frac{T}{K}=\frac{42000}{2.1\sqrt{1+\frac{1}{14000}\frac{l^2}{k^2}}-1.1,} \qquad (8)$$

These various comparisons will perhaps serve more satisfactorily than a theoretical discussion to show the capacity of the new formula, both as regards the range of l over k, and the form of cross section of column. In both respects they considerably extend the previous comparisons with over 30 full-sized columns given in *Science Series*, No. 60, and a good idea of how the formula meets this enlarged demand is at once obtained by reference to the magnitudes of the mean discrepancies.

INSTITUTION OF CIVIL ENGINEERS—ADDRESS OF THE PRESIDENT, SIR J. W. BAZALGETTE, C.B.

In addressing you as President of the Institution of Civil Engineers, I desire first to thank you for the honor you have conferred upon me by electing me to that position. For, although our Institution cannot boast of an ancient origin, its existence dating only from the year prior to the birth of our Most Gracious Majesty Queen Victoria, the presidential chair has nevertheless been filled by men whose names are engraven, not only in golden letters upon these walls, but will also be impressed upon the memory of future generations, as the pioneers of some of the greatest achievements which have been attained in practical science.

Forty-six years have elapsed since I was first elected a graduate of this Institution, and it has been my privilege, therefore, to have known it in its early days, and to have become personally acquainted with many of those honored men who have passed away, as well as with those who still remain as shining lights in our profession. But these recollections impress upon me the more deeply a sense of the responsibilities which attach to the position it has now become my duty to endeavor to uphold, and induce me to ask at your hands the same kind consideration and support throughout the coming session which you have always shown to my predecessors, and by which I have been placed in this chair.

Each annual report of the council forms a fit comment upon the rapid strides with which our institution has progressed. When I joined it in 1838 it was only twenty years old, and its members and associates did not include 351, including 33 graduates; whereas there are now 3,665 members and associates, and 778 students, making a total of 4,443, or between twelve and thirteen times the number registered forty-six years ago.

Whilst the profession of the engineer has been described by Tredgold in terse and emphatic prose, as "the art of directing the great sources of power in nature for the use and convenience of man," which has changed the aspect and state of affairs in the whole world, Pope has described its achievements in poetic language of great beauty in the well-known lines:

"Bid harbors open, public ways extend;
Bid temples, worthier the God, ascend;
Bid the broad arch the dangerous flood contain,
The mole projected break the roaring main;
Back to his bounds their subject sea command,
And roll obedient rivers through the land.
These honors peace to happy Britain brings;
These are imperial works, and worthy 'kings.'"

Had the poet lived until these days he would probably have added some lines on the influence of engineering on the health, comfort, and longevity of mankind. The rapid development of engineering science has, in fact, created the profession of the civil engineer.

In 1856 Robert Stephenson showed that in the United Kingdom alone there then existed 8,000 miles of railway, or a length equal to about the diameter of the globe, and the actual length of rail laid was sufficient to girdle the earth.

millions of pounds sterling, or one-third of the national debt, had then been expended upon these railways. But there are now over 18,000 miles of railway, nearly 10,000 miles of which are double, or more than double lines, having an authorized capital of over 800 millions, and upon which the gross annual receipts are 67 millions, and the annual working expenditure 35 millions. These railways carry 623 millions of passengers annually, besides half a million of season-ticket holders, and 246 millions of tons of minerals and general merchandise. Nor is this surprising seeing that railways and commerce stimulate and react upon each other. Thus, for instance, the output of coal alone in this country, the bulk of which is carried by rail, now exceeds 156 millions of tons per annum, whereas in 1855 it was only 64 millions.

Europe and America are the only continents which can be said at present to be moderately well furnished with railways, harbors, and other works which have now become necessaries of a civilized community. Yet out of the 1,334 millions of inhabitants of the globe, Europe contains but 328 millions, and America only 100 millions, making together barely one-third of the world's population; whilst Asia contains 700 millions, or more than double the population of Europe. Africa contains 200 millions, and China alone contains 280 millions, or nearly as much as all Europe, and nearly three times the population of America. What a vast field, then, still lies dormant to engineering enterprise, and who in the present day would venture to fix a limit within which the energies of the rising engineer shall be fettered?

Every passing year widens the pathway by opening up new discoveries and new subjects for employment. Those, however, who seek to follow the profession in these days without careful preparation, combining with sound practical experience, the mathematical and scientific principles which lie at the root of all engineering, and without a study of what has already been achieved by others, and of new discoveries as they are developed, would soon be left behind in the race in which they had embarked.

Although not specially designed for the better education of the younger members of the profession, the delivery of a series of lectures on the varied applications of electricity, at extra meetings held specially for the purpose during the past year, cannot have failed very materially to promote that object. It has also introduced into the history of the institution a new and marked feature. The announcement of the names of the eminent men, who had kindly consented to deliver those lectures, was a sufficient guarantee of their intrinsic value and interest. They were enthusiastically welcomed, and attended by overflowing numbers. The advantages to be derived from them, like their subject, are unlimited by time or space, and this institution is deeply indebted to their authors.

The success which has attended them has induced the council to inaugurate a second series of six lectures, during the present session, on the subject of "Heat in its Mechanical Appliances."

At the commencement of 1882 Sir William Armstrong drew attention to the manner in which the science of the engineer is now invoked for the purposes of war, the issue of which, he considered, would in future be chiefly dependent upon the superiority of the mechanical resources displayed.

The vast engineering and commercial enterprises referred to by Mr. Brunlees result from the more happy condition of peace, and it is proposed this evening to dwell more especially on those engineering works which promote the health and comfort of the inhabitants of large cities, and by which human life may be preserved and prolonged, and to furnish data from which some estimate may be formed of the sanitary condition and municipal management of some of the largest cities.

Civilization induces people to congregate in the neighborhood of cities and towns, for by daily intercourse, commerce, wealth, and intellectual development are encouraged. The geographical limits within which they can be brought into daily communication with each other have been enlarged by the introduction of railways, tramways, and other means of cheap and quick transit. But the rapid growth of cities constitutes the great difficulty of adequately providing for the requirements of the inhabitants.

Few, if any, cities, nor even docks,

railways, or other engineering works have originally been laid out with sufficient regard to the probable demands of the future. It is difficult to induce the present generation to expend its capital in providing for the requirements of after generations. The thoroughfares, sewers, and water supply, constructed for a city containing a few thousands of inhabitants, become totally inadequate to the wants of a population which has grown into millions; and the difficulties and costs of providing open spaces or widening thoroughfares in after years are very seriously increased. Thus, for instance, the first cost of forming a road which has subsequently become one of the main streets of a city was probably covered by the purchase of some vacant land and the structural works. But when in after years the road has become a crowded street, and costly buildings have been erected, and leases and trade interests of great value have grown up, the cost of their purchase becomes so great that not unfrequently the improvement is delayed, or its dimensions are cramped for want of funds.

London affords such an instance of rapid growth. It is now without a rival as regards its size and population, not only in the present, but as far as we know in the past history of the world. Our data as regards the past are very imperfect; but the population of Carthage, during the height of its prosperity, and shortly before its destruction, and the population of Rome about 300 years B.C., have each been estimated at about three quarters of a million, whilst that of Babylon was probably even less.

In defining its limits, London has been divided into what has been termed Outer London and London; Outer London, including several populous suburbs and the area over which the jurisdiction of the metropolitan police and some other metropolitan enactments extend, and London proper, which forms the subject of our more immediate consideration this evening, being limited by the municipal boundaries and under municipal government.

London, or the metropolis, as defined by the Metropolis Management Act of 1855, contains at present nearly 4,000,000 of people, covering an area of 117 square miles, upon which are built 500,000 houses, giving an average of eight persons to each house, and nearly seven houses and 53 persons to each acre. Its population is equal to that of the whole State of Holland, is greater than that of Scotland, and double that of Denmark, and if it continues to increase at the same rate until the end of the century, it will then equal that of Ireland, as indeed Outer London now does.

Its population has quadrupled since 1801, when it numbered only 959,000. From that date the increase has been at the rate of from 16 to 20 per cent. every ten years, estimated upon the population at the end of the previous decade.

It now increases at the rate of about 70,000 per annum, and when it is remembered that this is equivalent to the addition to London every year of the whole of the population of another city as large as Geneva or of Plymouth, one is inclined to ask in astonishment, Where will it end?

The value of property in London has increased even more rapidly than its population. In 1841 its rateable value was 6 millions sterling, in 1855 it was 10½ millions, and now it is 28 millions, having increased nearly five-fold in the last 43 years.

But the traffic through London has increased even more rapidly than either its population or rateable value. The arterial lines of thoroughfare, which were wide enough half a century ago, are now altogether insufficient. Thus, for instance, although the Strand and Cheapside have been relieved by the formation of a new route between Charing Cross and the Bank, along the Victoria Embankment and Queen Victoria Street, and Holborn has been relieved by a new route from Oxford Street to Shoreditch, and new and widened streets continued to be made throughout the city and other crowded localities, the old lines of thoroughfare still remain congested by the traffic. 384,000 pedestrians and 75,000 vehicles now pass over the metropolitan bridges daily; and the number of pedestrians increases at the rate of 4½ per cent. per annum, whilst the vehicles have increased at the rate of 13 per cent. The traffic on three metropolitan railways, viz., the Metropolitan, the Metropolitan District, and the North London, together increased between 1871 and 1881 from 79

millions to 136 millions per annum, or to 373,000 passengers per diem.

The government of the ancient City of London, by its Lord Mayor and Corporation, has up to the present time remained intact, with all its privileges and wealth; excepting that as part of the general municipality it sends three members to the Metropolitan Board of Works.

But the metropolis has from time to time been placed under the management of various local authorities. Prior to 1848 there were seven independent commissions of sewers. These were then consolidated into one commission. In 1855 the principle of local self-government was adopted, and 38 vestries, or district boards, subject to some general control of the Metropolitan Board of Works, were substituted, being representative bodies. The Metropolitan Board was also clothed with additional powers and duties, and these have in almost every subsequent year been enlarged, until it has now become the administrative authority for over one hundred Acts of Parliament affecting the metropolis. The present Government, however, have contemplated the creation of a new municipality, which shall include the city, and govern all the interests of the metropolis as a whole.

The demands for improvements of the metropolis, involving large expenditure, are still very pressing, for although the population in 1855 was more than two and a-half times greater than at the beginning of the century, little had been done up to that period in forming new, or widening existing thoroughfares, in sewerage, or other metropolitan improvements; and the onus has therefore been cast upon the present generation, not only of providing for the demands of their own rapid growth, but also for the growth of the previous half century; and, although 33 millions have been expended by the metropolitan authorities on such improvements since 1855, the arrears of the years of previous inaction are still far from having been wiped out, whilst the growth of each year brings with it fresh and irresistible demands for further expenditure. Hitherto the coal dues, which touch but slightly the pockets of the poor, and lighten the burden of municipal taxation to the extent of 2½d. in the pound, have stimulated the introduction of many of those improvements upon which we now look with satisfaction; and should those dues be allowed to expire, it will doubtless act as a damper upon the execution of works of urgent and growing necessity.

In 1878 there were 1,710 miles of streets, roads and courts, 1,338 miles of which were macadamized, or flint roads of an average width of 30 feet between the channeling, and upon which was laid in the course of the year 307,700 tons of macadam, flint or hoggin; 274 miles were granite-paved roads, and 61 miles were paved courts, 15 miles wood paving, and 22 miles asphalte; but since that period wood paving has been extensively substituted for some of the other kinds.

From the surface of those roads were removed in 1878, by scavenging, 616,500 cubic yards of scrapings, about 40 per cent. of which consisted of road detritus, 40 per cent. of water, and 20 per cent. of garbage from markets and refuse from the houses and streets. Upon the sides of the roads are fixed 67,500 catch-pits, from which also were removed 133,000 cubic yards, and about 25,600 cubic yards from the sewers.

Subways have been constructed under the Victoria Embankment, Queen Victoria Street, Garrick Street, Southwark Street, Commercial Street, and Northumberland Avenue, in which are laid gas and water mains and telegraph wires, in order to prevent the breaking up of the pavements of the streets.

There are now about 2,300 miles of underground covered sewers, more than half of which have been constructed during the last twenty-seven years. They vary in size from 9 inches to 12 feet 6 inches in diameter. All the houses are connected with them, and the house refuse and the most offensive decomposing matter is removed through them by the water supplied to the houses flowing through the sewers after use, in an unobtrusive, inoffensive, and economical manner, and without manipulation of any kind. The waste water thus becomes the motive power or carrier for conveying the refuse to covered reservoirs on the banks of the Thames 12 miles below London Bridge. These reservoirs are 16 acres in extent, and capable of containing 60 millions of gallons; and an aver-

age of about 150 millions of gallons of sewage and rainfall are discharged at the outfalls into the river daily on the ebb tides.

There are also forty-eight outlets at various points for the overflow of storm water into the river during heavy rains. Engines of 3,520 nominal HP. are employed at the pumping stations, which lift the sewage of the lower districts to heights varying from 18 to 36 feet.

The main intercepting scheme was practically completed and came into operation in 1870–71; and, considering that the mean annual death rate in London was, in the decade ending 1850, 24.8 per 1,000; in that ending 1860, 23.7; in that ending 1870, 24.4, and in that ending 1880 it had been reduced to 22.5, and for the year ending 1882 was only 21.4, it may not be unfair to claim for those works a considerable share in this decrease of the deaths. But a decrease from 24.4 to 21.4, or three persons per thousand, represents about twelve thousand lives saved every year in London, and a proportionate increased length of life to the living. The death rate for the past year has not yet been published, but it will probably be found to be about 1 per 1,000 lower than in 1882, so that the improvement since 1870 has been continuous, and the annual saving of life now, as compared with 1870, is about 16,000.

London is at present supplied with water by eight independent water companies, under the authorities and restraints of certain Acts of Parliament. They supply in the aggregate 140 millions of gallons daily, of which from 15 to 18 millions are consumed outside the metropolitan boundaries. The consumption within the metropolis is at the rate of about 31 gallons per head per diem. Of the total quantity 69 millions of gallons are obtained from the Thames, and 71 millions from the River Lee, the New River, and other sources. One hundred and forty-three pumping engines, of 16,490 nominal H.P., are employed, the head of pressure on the district supplied varying from 20 to 380 feet. They have ninety-three filter-beds, covering 95 acres; 54 subsiding reservoirs, covering 465 acres, and capable of containing 1,290 millions of gallons; also forty-nine service reservoirs, the capacity of which is 154½ millions of gallons, and 4,000 miles of mains laid, of which 3,000 are within the metropolis.

The charges of the water companies for the water are, with minor exceptions, based upon the rateable value of the houses supplied, and not according to the quantity of water consumed; that is to say, by their Acts of 1852, the Chelsea, the Grand Junction, the New River, and the West Middlesex Water Companies are authorized to charge 4 per cent. on houses rated under £200; and three per cent. on houses rated over £200; and the East London, and the Southwark and Vauxhall to charge 5 per cent. on the rateable value of all houses; whilst the Lambeth, by their Act of 1848, may charge water rates varying from 7½ per cent in small houses to 5½ per cent. on houses rated over £100; and the Kent Company, by their Act of 1864, may charge from 6 per cent. on small houses to 4 per cent. on houses rated at £90 and upwards; and all the companies are authorized to make additional charges, fixed according to rateable value, for baths and water-closets, and in some cases for high service. But inasmuch as the rateable value of the houses in London has risen since 1855 from £4 per head to £7 per head of the population, and the consumption of water per head has remained the same, the price of water, as based upon the rateable value, is now 75 per cent. dearer than it was in 1855; and there is no reason to doubt that so long as the price remains a fixed charge upon the rateable value of the houses, the cost of each gallon of water consumed, and the value of the property of the water companies, will continue to increase in every future year in the like ratio.

The total capital employed by the water companies is about £13,200,000, or at the rate of 61.7d. per 1,000 gallons of water supplied, and the net income for water amounts to 7.3d. per 1,000 gallons; whilst the annual cost of pumping and maintenance of works is 1.1d., which, added to the cost of engineering and management, makes the outlay 2.7d. per 1,000 gallons, showing a net profit of 4.6d. per 1,000 gallons.

In 1880 it was proposed to purchase the interest and property of the water companies, and place the water supply under the municipal authority, as it is at

Glasgow, Manchester, Liverpool, and in most foreign cities; and an arbitrator between the Government and the water companies valued their interests at that time at £33,000,000.

The mode of charging upon the rateable value of the houses, instead of by meter, moreover, makes the payment for the quantity consumed fall very unequally upon the consumers. If charged by meter a very effective check would be put upon the serious waste which now takes place.

Five per cent. upon the present rateable value of £28,000,000 would produce £1,400,000 a year, which is about the income of the water companies for water supplied within the metropolis, and which is equivalent to 7.37d. per 1,000 gallons of water supplied, or at the rate of 7s. per head per annum, on the present average rate of consumption.

Four per cent. upon a house rated at £400 a year would make the water rate £16 per annum; whereas, if there were twelve inmates to such a house, charged according to the average consumption of water at 7s. per head, the water rate on that house would be only 4 guineas per annum.

But a house rated at £30 per annum, and charged 5 per cent.. thereon for water, would pay, according to rateable value, £1 10s. per annum; and if it contained 6 inmates, and were charged according to quantity consumed, it would amount to £2 2s. per annum, supposing the quantity consumed were equal amongst all classes; but inasmuch as the poor do not consume so much water as the rich, these figures are only illustrative and approximately correct.

A purer and more copious supply of water, on constant supply and at high pressure, is demanded, and whether this is to be attained by purchase, or by some regulation of the present water companies' powers, it is obvious that each year's delay will only increase the cost and the difficulties involved.

The lighting of the metropolis is effected mainly by three gas companies, at a cost varying from 2s. 10d. to 3s. 2d. per thousand cubic feet. Over 20,000 millions of cubic feet of gas per annum are manufactured out of 2 millions of tons of coal, and it is distributed through pipes, the total length of which is about 2,500 miles; and they vary from 3 inches to 4 feet in diameter. The cost of lighting London by gas is therefore about £3,000,000 per annum, or more than double the cost of its water supply. The quality of the gas is tested by gas examiners at thirteen different stations, and is required to have an illuminating power of 16 candles when consumed at the rate of 5 cubic feet per hour, to be entirely free from sulphuretted hydrogen, with a maximum of 4 grains of ammonia, and from 17 to 22 grains of sulphur in 100 cubic feet of gas. In 1880 85,300 meters were tested.

But electric lighting is rapidly advancing; and, in confirmation of Dr. Hopkinson's opinion, that it may be looked upon as the lighting of the future, it may be mentioned that when, in 1878, the Jablochkoff Company commenced lighting a portion of the Victoria Embankment, the charge for each lamp was 5d. per hour. At the end of three months the price was reduced to 3d. Six months later it was reduced to 2½d., and, since June, 1881, forty lights on the Embankment and ten on Waterloo Bridge have continued to be lighted at the rate of 1½d. per light per hour.

Each of the electric lamps on the Embankment gives an illuminating power of 265 candles, so that, at the charge of 1½d. per hour per lamp, the cost per thousand candle power is 5.66d. per hour; whilst gas, at 3s. per 1,000 cubic feet, and consuming 5 cubic feet per hour for every 16 candles, costs per 1,000 candle power 11.25d. per hour. In other words, twice the illuminating power is at present obtained on the Embankment by electric lighting for the same money, if expended on gas. But it has been stated that the Jablochkoff Company are losing money on this contract. Incandescent lighting though much more costly in production, is more economical in the regulation and distribution of the light.

Eleven of the bridges over the Thames, on which tolls were levied, have been purchased and made free of toll within the last five years. At Hammersmith, Putney, and Deptford Creek new bridges are in process of construction. A new bridge at Battersea will shortly be built, and others have been strengthened with new chains or deeper foundations. But, considering that over 1½ millions of

people live east of London Bridge, that is to say, a greater population than exists in any city in the world, except London and Paris, it cannot be doubted that there is still a great need of improved communication across the river below London Bridge.

The Victoria, the Albert, and the Chelsea Embankments of the Thames are a total length of about 3 miles, and by them $52\frac{1}{2}$ acres of mud foreshore have been reclaimed from the river and converted into thoroughfares and ornamental gardens.

Since 1865 the extinction of fires and the saving of life and property from fire has been a duty cast upon the municipality, and a brigade is maintained, under the direction of Captain Shaw, containing 54 land-engine stations, 4 floating engines, 124 fire-escape stations, 576 firemen, 41 steam and 115 manual fire engines, besides tugs and other appliances. There were, in 1882, 1,926 fires, of which only 164 resulted in serious damage, and only 36 persons lost their lives, owing to the able management and gallant conduct of that small staff. The consumption of water in extinguishing fires is about 17 millions of gallons, and the cost of the brigade is rather over £100,000 a-year.

The Metropolitan Police outside the city are under the supervision of commissioners appointed by the Government; but the corporation of the city appoints its own police. They number in all 13,000; the area protected by them extends over Outer London, and covers 700 square miles, and the proportion of police to the population is rather less than in Paris, which is 1 in 373. But the area over which the Metropolitan Police are scattered is 23 times larger than in Paris, and it is satisfactory to know that, in spite of the amount of crime which escapes detection and punishment, London is the safest capital for life and property in the world. There are in London about 10,000 cabs, and 2,000 omnibuses or stage coaches under the management of the Metropolitan Police.

The parks, commons, and open spaces which are available for public recreation within the metropolis, and which form the lungs of London, are forty-two in number, and contain about 4,490 acres, exclusive of the squares, or about 6 per cent. of the whole area of London; whilst just outside the boundary are Epping Forest, Richmond Park, and Wimbledon Common, together containing 9,000 acres more.

There are fourteen markets of various kinds. The most important of these are: Farrington dead meat and poultry market, and Deptford Foreign Cattle Market; Islington Cattle Markets, 15 acres in extent; Billingsgate Fish Market and Covent Garden Vegetable Market; and into these markets are imported annually for consumption in London, about 800,000 head of cattle, 4,000,000 of sheep, calves and pigs, also 9,000,000 of fowls, game and rabbits, and over 100,000,000 of eggs, and a like number of oranges and lemons. About 320,000,000 of quartern loaves are consumed in London annually.

Billingsgate Fish Market covers only half an acre, and being insufficient for the supply of fish to the whole of London, a new market is contemplated. 35,000 vessels and 100,000 fishermen are employed in catching fish upon the coasts of the United Kingdom, and, besides the fish exported, 400,000 tons of fish are consumed in this country, of which 130,000 tons are sent to London, two-thirds by rail and one third by water. The average wholesale price of fish sold in Billingsgate is $1\frac{1}{2}$d per lb., but the consumer does not get the benefit of this low rate, and to enable the poorer classes to enjoy the food which so abundantly surrounds our shores, a market is needed which shall be accessible to all railways, having the means of storing fish in dry air at a temperature of 34°, and accompanied by the means of rapid distribution to all parts of the metropolis.

Paris contains a population of 2,240,000, occupying 77,000 houses, and covering an area of 30 square miles. This gives an average of 29 persons per house, and four houses, and 116 persons per acre. Paris, therefore, is more than twice as densely peopled as London, and each house in Paris contains nearly four times as many inmates as the London houses. Its rateable value is twenty-four millions sterling, being not quite one-seventh less than that of London.

It has 582 miles of streets, upon which are laid $73\frac{1}{2}$ miles of tramways, and the total length of its sewers is 440 miles.

Great care and labor are expended in cleansing and watering the streets and ornamental spaces in Paris. From a surface of over thirteen millions of square yards of street are removed annually 1,193,550 cubic yards of deposit, equal to a depth of 3½ inches of mud, dust and garbage spread over the whole surface. The cost of street cleansing in 1881 was £245,000, with a further expenditure of £82,000 for removing snow. The annual rainfall is 22 inches.

The water-supply amounts to eighty-two millions of gallons per diem, being at the rate of thirty-six gallons per head of the population. Two-thirds of this are obtained from the rivers Seine, Marne and Ourcq, and one-third from distant springs and artesian wells in Paris. Its sewers vary in size from 6 ft. 6 in. × 3ft. to 18 ft. 5 in. × 14 ft. 5 in., but the larger ones are in fact subways, containing galleries with a channel in the center, and waterpipes overhead. The construction of these has cost over four millions, and their cleansing and maintenance about £50,000 a year. A small portion of the sewage is disposed of by the irrigation of garden ground in the neighborhood of Paris, but by far the greater portion is still removed out of the city in cans by carts. In 1869 600,000 cubic yards were thus removed at an annual cost of about 2s. 3½d. per head of the population. There are four abattoirs under the administration of the municipality. Paris is lighted by gas lamps equivalent to 44,000 lamps of one burner each, which consume 770 millions of cubic feet of gas, at a cost for gas of £130,000, or about 3s. 4d. per 1,000 cubic feet, and a total cost of £190,000 per annum, including lighting and maintenance. This latter sum includes also the electric lighting of the Avenue de l'Opera, the Cours de Louvre and the Carousel; but this does not include the cost of lighting the private houses.

There are in Paris 600 omnibuses, 520 tramcars, 500 steamboats on the Seine, over 8,000 cabs, and the city is protected by a force of 6,000 men acting as police.

The city of New York contains a population of 1,350,000, occupying 100,000 houses, and averages 13.5 persons per house; and the population of Brooklyn is 585,229. New York has 350 miles of paved streets, through which are laid 200 miles of tramways, and 391 miles of sewers.

Its water-supply is derived mainly from the Croton River, and the supply amounts to 60½ gallons per head. Its annual rainfall is 46.68 inches; over a million of loads of refuse are removed from its streets per annum at a cost of £206,000.

Through the kindness of the British Consuls and other persons in authority I have been enabled to obtain, in a more or less complete form, some information with reference to 75 foreign cities. I do not propose to weary you this evening with all the data thus collected; but I have epitomized and tabulated it in a convenient form for reference, and it enables such a comparison to be drawn between some of the conditions existing in London and other large cities, as will justify the assertion that London is without a rival as regards health, extent and population. Next in importance to it is Paris, although it contains little more than half the population, and covers little more than one-fourth the area of London. The population of New York and Brooklyn together is nearly two millions, being rather less than that of Paris. Berlin, Philadelphia, St. Petersburg and Tokio (Japan), each contain about one million of people, and are equal to the joint population of Liverpool and Birmingham. Bombay, Vienna and Constantinople each contain about three quarters of a million: whilst Chicago, Pekin, Canton, Naples, Hamburg and Cairo, each contain about half a million, and are about the size of Glasgow. Calcutta, Baltimore, Boston, Buda Pesth, Madrid, Barcelona, Marseilles and Amsterdam, are about equal to Birmingham, each containing about 400,000; whilst the population of 40 of the remaining foreign cities varies from about 300,000 to 100,000, and they compare respectively with either Leeds, Hull or Brighton.

The rapidity with which the population of most of these cities has increased within the last forty years has been much greater than the rate of increase of the population of the globe. Thus, for instance, whilst the population of London, Paris, St. Petersburg and Vienna, has increased about 200 per cent., and that of Constantinople, Naples, Madrid, Rome and Amsterdam about 100 per cent., the population of the globe has increased

only 40 per cent., the greatest increase having been in America, and the least in Asia.

The rapid growth of cities is doubtless due to the development of civilization and of engineering science, which have stimulated manufactures and trade, and have turned those who were formerly agriculturalists into artisans, obtaining more lucrative employment in large cities. But these altered conditions demand commensurate civic improvements.

The ultimate object of all sanitary science is the comfort and convenience of the living, and the reduction of the death-rate; and although this will necessarily vary according to the climate and other local surroundings, in spite of all that human skill can accomplish, the death-rate may be largely reduced by the application of our improved knowledge of medical and sanitary science; and a reduced death-rate is therefore an indication of such advance. Thus, for instance, the death-rate of London has already been shown to have been gradually reduced from 24.4 per 1,000 in the decade ending 1870, to 21.4 per 1,000 at the end of 1882.

In Baltimore the death-rate is 21.9 per 1,000; in Boston, 22; Philadelphia, 22.3; Chicago, 23.6; Amsterdam, 24.3; Hamburg, 24.8; Rome, 26.1; Paris, 26.3; Berlin and Bombay, 26.4; Vienna, 29.2; Calcutta, 30.1; New York 30.6; Buda Pesth, 34.2; St Petersburg, 35.2; Cairo, 37; and in Pekin, though no accurate account has been kept, it is believed to be 50. These figures represent a saving of life in London which is worthy of special consideration; for whilst we indulge in a grumble at the atmosphere we breathe, at the water we drink, and the sanitary arrangements by which we are surrounded, London is, in fact, the most healthy city of magnitude in the world. If its death-rate were raised to that of Paris or New York, then from 20,000 to 36,000 persons who now live in London would die every year, or from 55 to 100 persons every day; and if the London death-rate were raised to that of St. Petersburg, then 55,000 more deaths would occur each year, or an average of 151 persons every day, in excess of the present deaths. Now it should be borne in mind that whilst the difference in the death-rate between London and Paris at the end of 1882 was 4.9 per 1,000, the average death-rate in England and Wales for the nineteen years between 1861 and 1880 was 21.9 per 1,000; and in France during the same period it was 23.7, being only 1.8 per 1,000 in excess of the death-rate in England and Wales, so that the improvement of the health of London cannot be attributed to its natural surroundings.

The comparative density of the population, and the number of persons residing in each house, vary very largely in different cities. In Chicago the average number of persons residing in each house is 4, in Baltimore and Naples 4½, in Philadelphia 6; in London, Boston and Cairo, 8; in Marseilles 9, in Pekin 10, in Calcutta and Amsterdam 11, in New York 13½, in Hamburg 17.07, in Rome and Munich 27, in Paris 29, in Buda Pesth 34.2, and Madrid 40; in St. Petersburgh 43.9; in Vienna 60½, and in Berlin 63.

The rateable value of the cities per inhabitant affords some indication of the cost, and therefore of the extent of accommodation of the houses, as compared with the number of their occupants; although the difference in the mode of rating, and in the cost of building, vary so greatly in different cities that this test cannot be altogether relied on.

In Pekin the rateable value of the city is £2 8s. per inhabitant; in St. Petersburg £1, Amsterdam £3 5s., and Calcutta it is £2 16s.; in Naples £3 12s., in Buda Pesth £4 4s., in Marseilles £5 2s., in Vienna £6 2s., in London £7, in Hamburg £7 9s., in Berlin £7 4s., Paris £10 14s., Brussels £11 8s.

The following tables give a comparative view of the quantities of water and gas supplied to the inhabitants of some of the most important cities in the world, the cost of providing the supply, the cost at which it is sold, and the margin of profit.

It remains to be determined whether a separate house for each family, however humble, or larger houses divided into separate flats for the accommodation of several families, is most conducive to health and comfort.

With larger houses divided into flats, it is practicable to give more air-space to each individual, to have wider streets, and at the same time to house a larger number of persons upon a smaller area.

Name of City.	Population.	Daily Water Supply.		Cost of Prouction per 1,000 Gallons.	Charge per 1,000 Gallons.	Profit per 1,000 Gallons.
		Total Gallons.	Gallons per Head.			
				d.	*d.*	*d.*
London...........	4,000,000	125,000,000	31.0	3.2	7.3	4.1
Paris.............	2,240,000	82,000,000	36.0	—	—	—
New York........	1,350,000	79,152,389	60.5	—	—	—
Berlin............	1,190,659	12,812,800	13.3	7.5	10.0	2½
Philadelphia......	1,000,000	54,156,898	54.15	—	—	—
St. Petersburg.....	929,090	19,807,700	21.3	1.17	3.44	2.27

Name of City.	Population.	Annual Consumption of Gas.		Cost of Production per 1,000 Cubic Feet.	Charge per 1,000 Cubic Feet.		Profit per 1,000 Cubic Feet.
		Total Cubic Feet.	Cubic Feet per Head.		Public.	Private.	
London....	4,000,000	20,000,000,000	5,000	Net. 1/9½	3/1		1/3½
Paris......	2,240,000	9,726,709,281	4,342	3/8 Deducting value of residual products, and including dues of all kinds paid to the municipality.	3/6	7/- Mean 6/	Mean 2/4
Berlin......	1,190,659	3,416,397,640	2,869	1/4 Including general expenses, but excluding rents, sinking fund, &c. 2/4 Including the latter items.	3/9	4/6* 4/3†	

* Supplied by Municipal Gas Works. † Supplied by Imperial Continental Gas Association.

When, therefore, it is stated that Paris, with more than half the population, covers only rather over a fourth of the area of London, and gives an average of 116 persons per acre, as against 53 persons per acre in London, it does not follow that Paris is therefore necessarily overcrowded. These conditions may exist with either superior or inferior house and street accommodation and sanitary appliances, and the expression "over-crowding" should have reference rather to an insufficient air-space per individual in the dwellings and the thoroughfares, than to the density of population per acre covered.

The condition of the few who can afford to live in well-built mansions, each as his separate castle, leaves but little to be desired; but for the masses of the population, who occupy smaller establishments, and more especially for the poorer classes, larger houses, laid out in separate tenements, appear to offer many advantages. The streets of a city in which such houses prevail will be shorter, and the cost of maintenance and cleansing therefore less, than when the population is spread in smaller dwellings over a greater surface. The working classes are, moreover, thus brought into closer contact with their employers than when

driven into poorer neighborhoods in the outskirts of a city, and at a distance from those business localities which afford them employment. Ground rents are reduced, the houses are more likely to be well built and to have their sanitary arrangements attended to, to have thicker walls and less exterior surface, as compared with the interior space, and therefore to be warmer and dryer. There has been of late years, and still is, a growing tendency in this direction in London, by the erection of such buildings as the Queen Anne's and Westminster Mansions, and many others at the West End for the rich, and the Peabody and other Artisans' dwellings for the poor.

In contemplating any comprehensive improvement or extension of large cities, the following are some of the questions which present themselves for consideration :

What should be the widths of the streets?

To what height should the houses be restricted?

What should be the minimum air-space allotted to each individual in the houses?

What proportion of the area of a city should be set apart for its lungs and recreation grounds?

What public buildings and markets, and what water-supply, sewerage, and means of lighting should be provided?

What should be the regulations to be enforced in order to secure the effectual combustion of fuel, and to prevent the contamination of the atmosphere by smoke?

The widths of the streets of a city should have reference, not only to the amount of traffic which may be expected to pass through them, but also to the heights of the houses surrounding them. When the houses are large and the population dense, the traffic in the streets will be greater, and, in order to secure the free admission of light and air, no street should be of less width than 40 feet, and not less than two-thirds of the height of the houses surrounding it.

The limit of the height to which houses may be carried with advantage is becoming extended by the more general use of hydraulic lifts, but it must have reference also, not only to the height at which the upper stories can be supplied with water, but also to the height to which a useful jet of water can be thrown in cases of fire, as well as to more general considerations of economy in construction and convenience.

The sizes and positions of the public buildings, markets, and open spaces for air and recreation, and the works necessary for water-supply and sewerage all require adaptation to the varying local conditions, and each will form the subject of special study for the engineer.

In London 5,800,000 tons of coal are consumed per annum, being at the rate of nearly $1\frac{1}{2}$ ton per head of population, in addition to the 2,000,000 tons used in the manufacture of gas ; and bearing in mind that each ton of coal consumed generates 56,000 cubic feet of carbonic acid gas, and that in a pure atmosphere there are not more than $3\frac{1}{2}$ parts of carbonic acid to 10,000 parts of air, the mode of dealing with this product becomes a subject of grave importance. It is, however, the imperfect combustion of coal which causes the more apparent annoyance of smoke, soot and fog, and the appliances and regulations necessary to secure the effectual combustion of fuel, so as to prevent its waste and unnecessary contamination of the atmosphere, may be carried out in new cities without difficulty, and at no great cost. But there must always be great objection to the introduction into an old city of any improvement, however simple, which renders necessary some structural alteration in every house. Our past-president, Sir John Hawkshaw, recognizing this difficulty, has offered some valuable suggestions on the subject, advocating the more general use of semi-anthracite coal in the ordinary fire-grates in London.

Prior to the introduction of the Artisans' and Laborers' Dwellings Act of 1875, much had been done by private efforts to improve the dwellings of the poorer classes. No less than twenty-eight associations had been formed with this object, and had provided improved homes for thirty-two thousand four hundred and thirty-five persons, at a cost of about £1,200,000, and at an average rental per week of from 2s. to 2s. 9d. for one room, 3s. to 3s. 6d. for two rooms, and 4s. 6d. to 6s. 6d. for three rooms. The return realized upon the outlay varied from $2\frac{5}{8}$ to $6\frac{1}{4}$ per cent. The average cost of eight blocks of build-

ings erected by the Metropolitan Association, including the purchase of land, or where leased, the ground rents, capitalized was at the average rate of £41 per inhabitant, varying from £29 to £81.

But these associations had the advantage of selecting such vacant sites as they could obtain on the most favorable terms, whilst under the operation of the Artisans' Dwellings Act the houses on any unhealthy district for which the new buildings are substituted must first be purchased compulsorily, as well as the public-houses and shops which are frequently mixed up with them, with all their trade interests, at a very heavy cost, and then cleared and new thoroughfares and sewers constructed.

Twelve areas, situate in different parts of London, embracing an aggregate area of 40 acres, in which the houses were overcrowded and declared to be unfit for human habitation, have been already dealt with by the Metropolitan Board of Works, at a cost of £1,500,000, and some further areas by the Corporation. In several areas the houses have been pulled down, new streets formed, and new buildings for the working classes erected on the sites, and in others the works are in various stages of progress. The cost of the new buildings has varied from 6d. to 8d. per cubic foot of the building, and the sites which have been cleared for their erection have been sold at prices ranging from 2s. to 5s. per foot super.

Inasmuch as the cost of a building depends mainly upon its size, it becomes necessary to consider what is the minimum air-space which can, with due regard to health, be allotted to each inmate. In the dormitories of poor houses and prisons a breathing space of from 450 to 500 cubic feet, with proper ventilation, has been deemed requisite for a healthy man; and two children have been estimated as equal to one adult.

The police requirements for the common lodging-houses are 240 cubic feet per head, and 450 cubic feet are allowed to each policeman lodged at a station. The Poor-law Board allowed 500 cubic feet per head in sick wards, and 300 feet for every healthy person in dormitories. Now 300 cubic feet per head means a room for two people, 8 feet high and $8\frac{3}{4}$ feet square, or for four people a room 8 feet high and $12\frac{1}{4}$ feet square; whilst 500 cubic feet per head means a room 8 feet high and $11\frac{1}{4}$ feet square for two people, and a room 8 feet high and $15\frac{3}{4}$ feet square for four adults. 500 cubic feet per inhabitant have been generally allowed in carrying out the provisions of the Artisans' Dwellings Act; and as the doors and windows of the new buildings are larger, and the surrounding streets and open spaces much wider than previously, the ventilation of the new buildings is superior to that of the old ones, and the condition of the atmosphere is thus rendered purer, although this air-space is less than could be desired.

Nevertheless, in comparing the death-rate, which has been below the average of that of the metropolis, in the new buildings, and the death-rate of the unhealthy localities for which they were substituted, it must not be forgotten that the comparisons refer to a totally different class of persons; all the habits of the persons displaced being eminently conducive to the shortening of life.

A practical difficulty is, morever, involved in every attempt to provide suitable houses compulsorily for the poorer classes, namely, that they object to be placed under any supervision or restraint, and cannot afford to pay the rents necessary to defray the cost at which the improved accommodation can be so provided, and even where low rents have offered it has been found that the new dwellings became inhabited by a better class than those who have been displaced from the unhealthy localities, and the occupants of these are driven into other poor neighborhoods which are thus again rendered overcrowded and unhealthy.

This, in fact, constitutes a great social dilemma, for whilst on the one hand the importance of suitable and healthy dwellings for the poor will be readily admitted, it may fairly be questioned whether it is just to throw the increased charge upon the rates, so that the man who is able only by industry and self-denial to pay his own rent, should be taxed for the rent of his less industrious and more self-indulgent neighbor; and why, he may fairly ask, if he is to pay a portion of his neighbor's house rent, should not the cost of his neighbor's food and clothing also be defrayed out of the public funds?

In order to be brought under the operation of the Artisans' Dwellings Act it be-

comes the interest of the landlord of the dwellings of the poor to allow them to fall into a condition which renders them unfit for human habitation, so that they may be purchased compulsorily, and the higher the rents the larger will be the amount of compensation he will obtain. It ought, on the contrary, to be made a disadvantage and loss to him to have his property declared unhealthy.

This property is frequently sublet to middlemen who collect such rents as produce a very high rate of interest on its value. What is needed is a more strict supervision by one competent authority, having no local interest, so that all places may be judged by a uniform standard. The provisions of the Common Lodging Houses Act, 1851, or of the Public Health Act of 1866, and the Artisans' Dwellings Acts of 1868, 1879 and 1882, might be modified, and the authority armed with more summary powers to oblige the landlords to repair and maintain their houses in a habitable and cleanly condition, and to prevent the demoralizing influence of overcrowding; and in cases of default, after due notice, such houses should be pulled down in the same manner in which "dangerous structures" are now dealt with under the Building Act of 1855. If the sites so cleared were sold for the erection of new dwellings for the poor, and money advanced for their erection at a low rate of interest, coupled with restrictions as to the class of building, and limiting the number of occupants, and the rents to be charged, the existing dwellings of the poor would be maintained in a proper manner, or new ones would be substituted at low rents.

The new dwellings would, nevertheless, become occupied by the laboring classes in receipt of regular wages, to whom, undoubtedly, a preference would be given; and the helpless and the depraved, who now seek shelter in overcrowded slums, would eventually be driven into the workhouses or common lodging-houses.

Longevity and premature decay are doubtless influenced by the food and general habits of the people, and by temperature and other local atmospheric conditions, although all these may be largely modified and brought under control by attention to sanitary laws and appliances. Artificial atmospheres are in fact created in large cities according to the character of the buildings, the air-space allotted in them to each inmate, and the mode of ventilation and warming, as well as by the width of the streets, the sewerage, and other sanitary arrangements. Moreover, the hereditary constitutions of the citizens become in after generations affected by the condition of the cities in which they and their forefathers have lived.

The facts and figures before us point to many of the causes for so great a variation in the death-rate as has been shown to exist in different cities. A high death-rate will, in most cases, be found to be the companion of defective house accommodation, ventilation, water-supply, sewerage, or scavenging. Thus, for instance, St. Petersburg, with a population of nearly a million, and the high death-rate of 35.2 per 1,000, is without sewerage, and its water-supply is taken from the River Neva, more or less contaminated by percolation from the subsoil. Cairo, with a death-rate of 37 per 1,000, is supplied with water from the Nile, having no sewers, and the sewage filtering through the subsoil into the Nile above the water intake. Vienna, with a death-rate of 29.2 per 1,000, has an average of sixty people in each house, or twice as many as in Paris, whilst the rateable value of the houses in Vienna is only one-sixth more than of those in Paris. Pekin, with a death-rate of 50 per 1,000, is without proper sewerage, water-supply, street-cleansing, or other proper sanitary arrangements.

The subject thus briefly touched upon largely affects the life and well-being of mankind, and is sufficiently interesting to invite closer and more exhaustive investigation under every variety of circumstances. For those members of the institution who have made sanitary engineering the subject of their special study, a wide field is open which will well repay its cultivation. The data which I have collected would form a starting-point for any member who will undertake the preparation of a paper for the institution. The Institution of Civil Engineers is the acknowledged representative head of our profession in all parts of the world. To be admitted to full membership has always been the just ambition of the ris-

ing engineer, because it is a real guarantee of professional status and qualification. It has, moreover, proved itself invaluable as the channel of communicating professional information, and of bringing its members into contact with each other, advancing their common interests, and encouraging good feeling.

It is the duty, and has always been the desire of its members, to embrace every opportunity to promote the objects of the institution. In doing this, the reputation of not a few has been first established through the ability displayed in the papers read, and in the discussions upon them at these meetings, all of which are widely circulated in the printed minutes. Allow me, in conclusion, to express an earnest hope that the meetings of the present session may be as productive of, at least, as valuable papers, and as good attendances and discussions as have characterized those of previous years.

THE TEST AND CHOICE OF LUBRICANTS BASED UPON A MECHANICAL METHOD OF TESTING.

By R. JAHNS.

From "Organ für die Fortschritte des Eisenbahnwesens," Abstracts of the Institution of Civil Engineers.

In testing a lubricant the two following conditions should be fulfilled:—

(1.) That the particular lubricant possesses those qualities necessary to reduce the friction, between two metal surfaces, to a sufficient degree for the purpose required.

(2.) That the particular lubricant has the power to resist, for a sufficient period of time, the destructive action of the work which it has to perform in its application.

Under the term "work" which the lubricant has to perform in the last condition, is understood the overcoming of the resistance of cohesion in the layer of lubricant between the two metal surfaces. This work not only produces heat, but also changes the chemical affinity of the constituents of the lubricant, and destroys and produces changes in their physical properties. The lubricant between the two metal surfaces is subject to no other action than this, and it must therefore appear advisable to base the test of the powers of resistance or endurance upon the examination of this action. Mechanical means must be preferred to the chemical tests generally employed, because the results of the former discover the effect of the action of all that takes place in the lubricant, which could not be perceived or followed by the chemical method alone. The author remarks that lubricants are still, in very many cases, solely judged and compared by their behavior in practice, and that this method only gives results, after a period of weeks or months, which are not even then reliable, as a regular and complete inspection cannot continually be made of the subjects of the test, whilst other factors, such as heat, cold, and dust, come into play. In the mechanical method these factors disappear, and the results of the test can be rapidly arrived at. For this purpose the test should deal with a minimum quantity of the lubricant, and the testing machine be constructed with this object in view.

To discover whether the first condition be fulfilled, the lubricant to be tested is applied to the outer surface of a hollow spindle and to the inner surface of a steel bearing, whose cross-section being a semicircle of a greater radius than that of the spindle immediately below it, the contact between the two metal surfaces is reduced to a minimum. Round the bearing, and attached to it, is hung a metal stirrup.

On the spindle being rotated, the perpendicular, passing through the centre of gravity of the stirrup and that of the cross-section of the spindle, is inclined at an angle from its original direction when at rest. This angle is directly proportional to the intensity of friction between the spindle and bearing with lubricant between them, and can be measured by a

scale on the lower portion of the stirrup. In testing to see how far the lubricant satisfies the second condition, that is, what amount of heat it develops in a given time, a testing-machine of the following construction is used.

To produce a satisfactory test, the heat generated must be solely derived from the lubricant. To this end a spindle, whose central portion is parabolic, is employed; on the top of this the segment of a bearing is accurately fitted, and held down by a weighted lever attached to the mountings of the machine. The spindle is supported below by two mounted rollers with flanges to their ends. A certain quantity of the lubricant to be tested is now poured upon the parabolic portion of the spindle, over which it is equally distributed by a strip of leather between the rollers and the spindle being moved to and fro by an oblique ridge on one of the rollers.

The spindle, which is hollow and closed at one end by a thin corrugated steel plate hermetically sealed, is now filled with sulphuric ether fumes, whose average expansion, within the limits of temperature occurring here, is about ten times that of the atmosphere.

On the spindle being caused to revolve by means of a belt passing round its centre, the heat given off by the lubricant is imparted through the metal of the spindle to the ether fumes within; these expanding, exert a pressure on the yielding corrugated plate, and set in motion the pricker, to which it is attached by an arm. Below this pricker is an endless sheet of paper stretched round two revolving cylinders, whose axes are parallel to that of the spindle from which motion is transmitted to them by a screw and wheels. A certain number of revolutions of the spindle is thus represented by a corresponding length of paper; the proportion in this case being chosen so that two hundred and fifty revolutions of the spindle per minute move the paper 3.75 millimetres (0.15 inches).

To avoid any hindrance to the progressive motion of the pricker mechanism is employed, by means of which the paper, at the end of every fifteenth second, presses against the pricker, releasing it again immediately.

The curve thus traced, after at first rapidly rising, soon becomes a straight line parallel to the direction in which the paper moves; after a time, if the lubricant be not renewed, its power will be exhausted, the temperature of the spindle increased, and the curve consequently again rise.

The abscissa of the curve represents the number of revolutions of the spindle, whilst the change of condition of the lubricant is represented by the ordinates; the product of the two, that is, the area between the curve and its abscissa representing the destructive action of the work in the lubricant.

Thus the power of endurance of a lubricant is inversely proportional to the area enclosed by the curve and its abscissa after any given period of time.

The author then goes on to express the relative value of two lubricants in the contents of two parallelopipeds of equal base, whose volumes are respectively equal to the areas enclosed by the curve into the sines of the respective angles representing the values of the lubricants in the first case. It is recommended that tests be made of good lubricants, such as olive and rape-seed oils, and these used as standards with which to compare all other tests.

At a recent meeting of the Berlin Physical Society Prof. Neesen gave a short account of the contrivance by which in his lectures he measured the mutual attraction of two magnets by means of scales. In conclusion, he reported experiments instituted by him with a view to determining the influence of magnetization on electrical conducting power. In these experiments he had made use of a magnetic substance of high specific resistance, a solution of chloride of iron. Two equal tubes were filled with the same solution, and inserted as the two branches of Wheatstone bridge into the circuit of a galvanic battery; the two other branches being so arranged that the galvanometer stood at zero. The electrodes in the two tubes consisted of iron plates, and were exactly alike. The tubes, that is, the fluid conductors, had in the diffent experiments different shapes and different diameters. The contents of the one tube were then magnetized either by a magnetizing spiral or by a powerful electro-magnet, and the galvanometer was observed during this process of magnetization. The result of the experiments was in every case a negative one. Very slight deflexions were indeed observed in the galvanometer needle in the case of the experiments with the magnetizing spiral, but these proceeded from the slight heating of the fluid, an effect which, notwithstanding the solution of chloride of iron was surrounded by a casing circulating water, had not been wholly avoided.

THEATRE VENTILATION.*

By Mr. JOHN P. SEDDON, F.R.I.B.A.

From "The Building News."

In his introductory remarks, he urged that every cowl disfiguring our chimneys was an unnecessary evil, as a smoky room was caused generally by want of a proper supply of fresh air to the fire, or else by some wrong construction of the fireplace or flues. Great as was the evidence of neglect of ventilation in our houses, the condition of our theatres was even worse. Dr. Angus Smith, in his work on "Air and Rain," gave a tabular statement of analyses of air from various sources, and showed that some samples from the dress circles of theatres were even more foul and prejudicial to health than others collected from within street sewers. Many people, though enjoying dramatic representations, seldom went to a theatre, because, although not particularly de licte or fastidious, they invariably suffered afterwards from the effects of the bad and heated atmosphere in them, or from colds caught in consequence of the efforts made to escape from it during the intervals between the acts. Theatre audiences had to imbibe such a noxious compound of the products of gas burners and human lungs as often converted the comedy they went to see into veritable tragedy. There is (continued the lecturer) no great practical difficulty in providing our theatres with fresh, and extracting foul, air, except what arises from attempting half-measures only; and it has been in consequence of such half-measures that the failures made hitherto have occurred. What is wanted is simply a plentiful supply of fresh air forced into every part of the building of a theatre, and not into the auditorium only, together with the extraction of the foul air from the several parts where it collects. This, then, is the problem—one of paramount importance—the solution of which appears to be still waited for alike in private and public buildings. I propose now to consider it specially in its relation to theatres, than which there is probably no description of building more capable of being efficiently warmed and ventilated; while yet there is certainly none other that is so generally devoid of all rational provision for health and hygienic comfort. It is obviously opposed to the interests of the manager of a theatre to have his house ill ventilated, for the very fact of its being so is a most powerful, perhaps the most powerful, reason that so large a number of the population avoid theatres altogether. This deterrent is infinitely more potent and active than any conscientious scruples respecting the morality of the stage, and it alienates a much larger proportion of the public. Religious disapproval does indeed keep away some persons from the theatre, but their number is as nothing to that of those who absent themselves solely and purely on account of the intolerable stuffiness of the atmosphere therein, and the almost inevitable headache that follows an evening spent in the majority of theatres. On the contrary, a well-ventilated theatre means, *cæteris paribus*, a full house for most nights of the week. I have been told that a theatre at Manchester was some years ago notorious for the extremely defective state of the atmosphere within it for want of proper ventilation, and that it was, in consequence very generally avoided — one lessee after another failing. But at last a manager, more enterprising and more enlightened than his predecessors, hit the right nail upon the head. He not only took the theatre, but he thoroughly ventilated it, and in a few years he retired with a handsome fortune. To the managers of theatres, and especially those of the lyric stage, good ventilation is a matter of special importance, because it immensely improves the acoustic qualities of buildings. That annoying echo which muddles up the notes in tantalizing confusion is instantly removed as soon as a good system of ventilation is applied to a building. Then it is seriously to be remembered that few buildings in the present

* A lecture delivered at the Parkes Museum of Hygiene.

day are wholly disconnected from the street sewers, as their drain-pipes, in spite of traps, do form a means of connection therewith. Now, theatres being the most highly heated buildings of any, act the most powerfully as pumps to draw up the gases and vapors underground into their interior, with what results it is easier to imagine than describe. This is another cogent reason why greater care should be given to the ventilation of theatres than even to buildings generally. Now, if it be, as I have stated, not so extremely difficult to accomplish so desirable a purpose, why have almost all the efforts in that direction been otherwise than successful? The answer to this question I believe to be: Because those efforts have been generally confined merely to the extraction of the vitiated air by means of mechanical or automatic appliances, and that in scarcely a single instance has there been in addition any intelligent effort to introduce by the same means a sufficient amount of fresh air, constantly flowing in, in order to take the place of the vitiated air that has been sought to be extracted, and at such a regulated temperature as to be inoffensive, by reason of the absence of cold draughts impinging upon persons sitting near the inlets. In consequence of this oversight the utmost that they were able to effect, therefore, was to draw in cold fresh air from the various accidental or temporary opening chinks around doors and windows, down chimneys, &c., so creating draughts; or to drain towards the auditorium the still fouler and more vitiated air from other wholly unventilated parts of the building. The above remarks apply likewise to sunburners, and to the great gas chandeliers so much affected in the old theatres. These only succeed in heating and expanding the air, and expelling, *pro tanto*, the amount so expanded. Mechanical extractors, such as fans, cylinders, pumps, &c., do indeed extract a considerable quantity of air from the auditoriums to which they are applied, and anemometers in consequence register thousands of cubic feet of air removed per minute, to the satisfaction of the patentees of such appliances. But, however satisfactory these results may prove to those gentlemen, they are not equally so to the unfortunate occupants of dress circles, as the semi-exhausted condition of the atmosphere so produced there is more intolerable than a surfeit of even vitiated air would be; while to the stallholders, blasts of the comparatively cold, but far more highly vitiated air, from the parts of the house behind the stage front, for which no attempt at ventilation is ever made, sweeping over the footlights and carrying the gaseous products with them, are even more offensive. Some persons imagine that because theatres are lofty buildings that, therefore, they must be airy as well as spacious; but no mistake can be more fatal for they only form vaster receptacles of foul air and are far less easily emptied of the same than buildings of more moderate height would be, unless a proper system of ventilation is applied to them. Even when inlets to admit fresh air are provided for auditoriums of theatres, they are ordinarily upon an altogether insufficient scale, and being situated, as, indeed, they should be in ordinary buildings, but not in a theatre, at a level above the heads of the audience, they fail, in consequence, of producing the expected effect, because there are no exhausting outlets at a lower level to induce a circulation of the fresh air. In the rooms of other buildings the fireplace is the principal outlet for vitiated air, and at a low level. It, therefore drains off the heavy carbonic acid which falls as it cools. But in a theatre there is generally no extracting outlet fixed at so low a level, and, therefore, the occupants of the pit and stalls are placed, as it were, in a bath of carbonic acid, which rises, as it accumulates until they are forced to breathe it, to their discomfort and deadly peril. The foregoing remarks apply principally to the auditoriums of theatres; but the importance of extending a proper system of ventilation to the entire structure must not be overlooked for an instant. The stage, the greenroom, the dressing rooms, the refreshment rooms, the property rooms, and all the other parts of a theatre in which the numerous employés are engaged, need, though they seldom receive, equal consideration in this respect. These subordinate parts in almost every theatre, as in Covent Garden, for instance, are in a far worse condition than those portions that are devoted to the public. The condition of the

dressing rooms, and passages and corridors leading to them, are almost invariably exceptionally frightful, for they are numerously occupied with quantities of gaslights continually burning in them; and there are seldom any or but few outlets provided for the vitiated air generated in them. and they have no inlets for the supply of fresh air. Under such circumstances how can actors perform their parts with alacrity? Languor, the precursor of disease, must sap their energy. Yet again, the lobbies, the halls, and corridors should be most plentifully supplied with fresh air regulated in temperature, whereas, as a rule, these parts of a theatre are absolutely neglected in this respect. Were these filled with an ample supply of warmed or cooled fresh air, they would become feeders for the remainder of the building, and the cold draughts now complained of in even the best ventilated theatres, which sweep into the auditorium as soon as any of its doors are opened, would be entirely obviated. But, it may be asked, how can the whole of the multifarious parts of such a building as that of a theatre be sufficiently ventilated and warmed, or cooled, as occasion may require? My reply is, as before: By avoiding the half-measures only, which alone hitherto seem to have been attempted in this country. The thorough principle I am laying down is but the same that is universally, and on the whole successfully, adopted in America. It is only in points of detail that it would seem that the methods adopted by our trans-Atlantic cousins need some modification, such, for instance, as taking care that the warmed air is not burnt, and by increasing the freshness and volume of the warm-air supply needed in winter, or of the cooled air desirable in summer. The great essential for theatre ventilation is, that the whole structure, from basement to roof, should be completely filled throughout, by mechanical means, with pure air, regulated in temperature as required. At the same time, although it is the inlets that primarily demand attention, mechanical means should also be used secondarily for the constant drawing off of vitiated air from the several places where it is apt to collect, such as the floors of the pit and stalls, under the galleries, upper and dress circles, over the footlights, &c. The opposite plan to the above—that, namely, of relying mainly upon the exhausting appliances, and leaving the fresh air to enter as it can, to supply the place of the air that has been extracted—is fraught with evil from its liability to produce draughts from the various points whence the semi-vacuum created can succeed in obtaining a supply, such, for instance, as temporarily open doors, and even from the chimneys, down which the smoke is actually drawn by this abnormal demand for air. Indeed this is one of the most fruitful causes of the smoky chimneys, which cowls are supposed to remedy. The system I have thus recommended, if fully carried out, will get over the difficulty of having to decide whether the inlets should be vertical or oblique—a question that could only arise when they are small and few, and when the air they admit is unregulated in temperature; or whether the breezes needed should be obtained by utilizing the mouths of all the cupids, angels, and gods and goddesses that a fertile invention of the decorator of a theatre may introduce; or whether the cornucopiæ and such-like gauds should be made hollow or not. Given power enough, which is easily obtainable, to flood the whole building with air tempered as required, and it will matter less as to the precise distribution of the inlets in the auditorium itself. There is one thing that will doubtless tend to render this problem of the ventilation of theatres, which has been found so difficult, easier to accomplish—I mean the introduction of electric lighting instead of by gas. This has already been done by the enterprising manager of the Savoy Theatre—I doubt not to his own material advantage, as well as to the comfort of his ever-crowded audiences. Freedom from draughts has not, however, yet been attained in the auditorium of this theatre. The refreshment rooms, however, though situate some twelve feet below the level of the street adjoining, are admirably ventilated, much in the way I have proposed, by the Æolus Waterspray and General Ventilating Company's appliances, and it would only seem to be necessary to adopt the same system generally throughout the building to insure a complete success. We cannot afford, however, to let this present generation pass away stifled and incommoded, while

electricians are perfecting their methods of lighting. Nor is it every theatre that has a spare acre or two, in the middle of London, alongside of it, for the accommodation of a puffing steam-engine to create the light, nor an untenanted embankment as a convenient neighborhood to put up with the noise of one. In the meantime the gaslights in theatres should, as far as possible, have their products of combustion conveyed outside the building without being allowed to mingle with and contaminate the atmosphere of the interior. Yet, notwithstanding this serious drawback of the ordinary gaslighting to contend with, we have scientific resources at command sufficient to enable us to overcome all the difficulties that at present retard the proper development of this essential of civilization—proper and adequate ventilation. Another advantage of this system I am advocating that depends upon an ample supply of air forced in, rather than upon means of extracting the vitiated air, is that all those ugly cowls, which you must have noticed are my special *betes noirs*, become needless. A tax ought to be put upon every such excrescence in proportion to its size and unsightliness. This tax should be quite prohibitive of all such monstrosities as those that disfigure the roof of St. James' Hall, and make London a laughingstock to foreigners, who pay a little more attention to the beauty and sky line of their streets than we do of ours. We should soon then see the last of those metal pipes which obtrude from our roofs, looking as if they had sore throats wrapped round with comfortless comforters, and which in pretty little models seem sometimes to extract air with marvelous ease, but which somehow always fail to do so when most needed, as, for instance, when there is no wind to blow, or when it may happen to blow a little too hard. The system is applicable at moderate cost to old theatres as well as to new ones; and, of course, it is our oldest theatres that stand most in need of being ventilated, since most of them are but death traps to all concerned with them—audience and actors, and supernumeraries. It would, perhaps, be invidious in me, who, as an architect, cannot have any interest whatever in any of the numerous patent and other inventions for ventilating appliances which are offered to the public, to specify which of them I should particularly recommend. Assuredly, the proper course for any one desiring to build a new theatre, or to improve an old one, is to consult some professional man whose business it is to consider scientifically all the conditions of the special case with which he has to deal. Managers of theatres are evidently learning wisely to take this course; and without doubt most of the recently-built theatres are vast imimprovements upon those that preceded them. This is certainly due to the great skill and care bestowed upon them by their respective architects. If no theatre is yet absolutely perfect in this matter of ventilation (and I do not know one that is) I shall be glad if any hints or suggestions of mine, with regard to the general principles that should be followed, be of use or assistance to any of my professional brethren in their future works; and my object in bringing forward this matter will have been attained. I am not able to speak much from personal experience as to the condition of theatres on the Continent in regard to their ventilation. But I think it may be assumed that the general experience there is much on a par with that here. In the *Builder* for Sept. 22nd, 1883, the following statement appeared: "As for the matter of ventilation, that also requires, in the interest of the public, most earnest consideration. There at least we have but little to learn from our Continental neighbors, who suffer in their theatres quite as much as we do in ours. The day that the last grand chandelier shall have disappeared from our theatres will be a memorable one in the history of the stage. Then, at length, a visit to the theatre will cease to be what it is, at present, too often proves—a singularly uncomfortable mode of enjoying one of the most instructive of all pleasurable relaxations." There is, however, in this museum an example of French ingenuity devoted to the endeavor to remedy the state of things I have been describing. This is an elaborate design setting forth a scheme for a system of ventilation applicable to a theatre. It undoubtedly shows a considerable amount of cleverness and thought, and the disposition of the extracting flues, applied to the level of the floor of the pit and stalls, is admirable. But the author has certainly not to my mind grasped the

general principles that I have been endeavoring to prove to be the right ones. This may be seen from the very title that has been adopted—namely: "Ventilation des theatres: Injection, centrale, gratuité, Evacuation peripheric." That is to say, the admission of the fresh air is to be concentrated in the auditorium and is to be attained by what is called natural (as opposed to mechanical) means, to supply the place of the vitiated air to be extracted by flues placed round that part of the theatre, no other than which has ever been considered. The author, a Parisian architect, commences by saying that "no place is more easy to ventilate than a theatre"; which renders me curious to know if he has ever reduced his theories to practice, because I should anticipate that he would simply have succeeded in making the building a veritable "temple of the winds." The reason, however, that he gives for considering that this class of structure is easy to ventilate is, "that the people in it occupy fixed places in the circumference, leaving the whole central part empty." From which remark it is obvious that he ignores the remainder of the building with all its exceedingly complicated arrangements, and the numerous parts where foul air can and does collect, and thence invade the auditorium. "The injection of the fresh air," he continues, "is easily obtained gratuitously." That is, it is suffered to come in at its own good will and pleasure, if it will do so, to replace what is drawn off by the exhausting appliances. I have, however, already explained what the result of such an experiment would be. "This injection of fresh air," he says, "being made far enough from the spectators, so that none of them are inconvenienced by it; it is also made under such conditions as to make the transmission of the sound-waves from the scene to the public." Now, the position he has chosen for this concentrated inlet of air is at the back of the stage, facing the auditorium, whence it would rush, charged with all the dust raised by the motion of the actors on the stage and by the shiftings of the scenes, over the footlights right into the faces of the stallholders. We may, I think, turn with greater advantage to consider the attempts that have been made in America, where necessarily, from its climatic extremes, the subject of ventilation has been much considered. The following particulars I have just met with (after the foregoing remarks had been written) in a paper, by Mr. Arthur J. Gale, upon "American Architecture," published in the *Transactions* of the Royal Institute of British Architects for 1882–84. These seem to show that ventilation upon (as far as I can judge) the identical principles I have laid down, has been successfully carried out in the Madison Square Theatre in New York. Mr. Gale says that this is not a very large but a very successful house. He then describes it as follows: "The fresh air inlet is by a descending flue, 6ft. square, lined with wood, in which is a conical cloth bag 40 ft. deep, to filter the air, which afterwards passes over ice in summer, four tons being used each night—two tons before and two tons after the air passes the fan at the bottom of the inlet shaft. This fan forces the air into a brick duct, from which sheet-iron pipes lead it into four brick casings surrounding steam radiators that supply the required heat in winter. This fresh air, so regulated in temperature, is conveyed by 4in. tin circular pipes to the four sections of 90 seats each in the auditorium. Other special ducts supply additional cooled air, when needed, to other parts of the building. All the gaslights are incased in glass, and have ventilating shafts. By these and other exhaust shafts the vitiated air is drawn off from various parts of the building by means of another fan in the roof. The footlights are ventilated by the same means. Tests have proved the ease with which the system can be worked and the excellent results which have been attained by it. The temperature on one occasion at 9.30 P. M. was outside 85 degrees Fahrenheit in the delivery, just beyond the ice 70 degrees, and at the main outlet 86 degrees. I really think now that, having cited the last two examples, the one showing, as I think, how not to ventilate a theatre, and the other being a record as to the manner in which one has been successfully treated, I need say no more in support of the general principles that I have advocated. As to the special appliances which are required to carry them out in practice, they must necessarily vary according to circumstances. I shall conclude, therefore, with the recommendation to managers to give the

system proposed a fair trial, and not to be content with half-measures, and, as a parting piece of advice to be cautious not to allow themselves to be led, or misled, as in all probability they would be, by the specious advertisements of patentees of particular appliances, the value of which (if any) depends entirely upon the manner in which they are employed. In all cases they would be wiser to place this class of work in the hands of a professional architect in whom they have confidence, and whose reputation would depend upon the success of the result as a whole, and who, of course, has no interest whatever in anything that would form part of the details of the general arrangement. Since the foregoing paper was written, my attention has been called to a description of the ventilation and warming of the Metropolitan Opera House of New York, given in the *Sanitary Engineer* paper of that city, for December 20th of last year. The system adopted seems to be exactly what I have recommended, and to have been designed and carried out by the architect, Mr. J. Cleveland Cady, and the ventilating engineer, Mr. Fredric Tudor. The principle involved is stated to be that of "plenum ventilation," the object being to have an excess of air entering the building to that which is leaving it by the regularly provided foul-air outlets, the result of which is to have a pressure within the building slightly in excess of that of the air without the walls, so as to insure an outward current through crevices of doors and windows, or accidental openings. To accomplish this in a practical manner a blowing-engine is used, and the supply of air almost unlimited. The air is drawn in at a height of 75ft. from the ground, down a shaft to what is called a settling chamber, 48ft. by 20ft., by 10ft. high, whence it is drawn through heating coils by a fan to a main air-duct. The air is admitted to a space under the auditorium floor, through numerous openings, and thence through the risers of the steps on which the drains are set. The boxes are supplied from the main air-duct by flues in the walls leading to spaces between their floors and ceilings. By this means every stationary chair in the house has air admitted to it. Outlets for foul air are provided where the several gas-lights afford extracting power. There is a principal controlling valve to the ventilation in the center of the dome-shaped ceiling of the auditorium. By the adjustment of this the pressure within the house is regulated, and the condition of plenum maintained under varying conditions of the speed of the fan. The stage has separate ventilating and warming appliances, by means of which a difference of pressure is kept between the house and the stage when the curtain is down, enough to belly the latter slightly towards the stage; the rising of the curtain then allows air to pass from the house to the stage ventilators. I am told that the Criterion Theatre is constructed below the level of the street, and consequently a most difficult place to ventilate, and when first built almost insupportable from foul heated air; it was made pure and sweet, and the temperature kept down by a ventilator of the "Norton" system, which consists of an extracting appliance of great power without, however, provision for fresh air to replace that extracted, other than what might find its own way downward from the street. Unfortunately this ventilator has been removed to gain space in some recent alterations. In the Prince's Theatre, recently opened, and built by Mr. Phipps, as at the Savoy, the basement, the most difficult portion of a building to ventilate, has been most efficiently ventilated and warmed by the "Æolus Waterspray Company." The success attending this system thus partially employed renders it a matter of regret that it was not applied to the whole structure in the thorough manner I have advocated. Had it been so, I think we should have had as memorable an example of improvement to record as that at New York, which I have been describing. I think, at any rate, it is evident that this system affords ample power to completely warm and ventilate any building, and no doubt there are others that can accomplish the same end. It remains for architects, or rather their employers, to make proper use of the means now placed at their command. The comfort and health of the actors and *employes* at theatres would be very greatly enhanced if, in their dressing rooms, which as a rule are wholly neglected and in a parlous condition, the simple precaution were taken which should be adopted in the rooms of all buildings—

that, namely of providing a supply of fresh air to every fireplace, and extracting the foul air by ventilators into the smoke flues. There are numerous excellent appliances for this purpose, such as Boyd's, Shillito's, and other ventilating grates, now so constantly and properly employed in school buildings. In fact, every grate, stove, or other heating apparatus ought to be compelled to be made a place for the admission of fresh air, because it is easily tempered and admitted there without causing draughts. I consider that some public authority ought to condemn as uninhabitable every room to which fresh air is not thus supplied, and I am sure that by such a simple regulation the annual death-rate of the community would be at once greatly diminished. Smoky chimneys would become almost things of the past, and cowl manufacturers extinguished. If a chimney smokes, the last place that needs to be looked to is the top of the chimney; the first is the fireplace and its surroundings. A chimney smokes. Why? The room has no supply of air, or the fireplace has been badly constructed, leaving a reservoir of stagnant cold air above it; or the flue is too large, which is generally the case; or too straight, with no provision of expanded space for gusts of down draughts to expend themselves in; or lastly the top may be without any protection against wind or special currents of air. But whatever the cause, sufferers from smoky chimneys can, as a rule, conceive of no remedy but a cowl, whereas that is the last that should be tried. Take out and reset the grate, or replace it by one to which air can be freely admitted, or contract the opening above the fireplace, or make an expansion in the flue for down draughts to expend themselves in, or put a slightly architectural finish to the top of the chimney, to shield it from the wind which injuriously affects it. But do not put up an ugly twisted twirling corkscrew excrescence to distract your neighbors as well as yourself, to ruin the skyline of your buildings, and proclaim to all the world your misery and ignorance, as well as that of your architect and builder. There are in this museum and elsewhere many useful appliances for the construction of all the several parts of a building used in the consumption and extraction of smoke; but it seems to me evident from their comparative scarcity in houses generally, that the public does not realize their utility—I might even say their necessity. I think this arises from their being presented to their notice in what I may call a desultory and isolated manner. I have therefore arranged with a firm to show the proper construction of fireplaces with their flues and chimney tops as they ought to be built, at the forthcoming Health Exhibition, in a complete system. By this means the public, I think, will be able to see the nature of the general problem which has to be grasped, and then can judge which of the appliances exhibited in that exhibition are the most suitable for each special purpose. Otherwise they are likely to become bewildered among the multitude of articles presented to their notice, most of which, though good in themselves, are liable to fail unless combined and applied with intelligence.

MAINTENANCE AND ROLLING OF MACADAMISED ROADS.

By A. DEBAUVE.

nslated from "Annales des Ponts et Chaussées," for Abstracts of the Institution of Civil Engineers.

A DETAILED comparison is drawn between the relative advantages and disadvantages of the maintenance of roads in the country by patching and by rolled layers; and also between rolling with horses and by steam. The advantages of the system of repair by patching are, great simplicity, and a regular profile of road; the disadvantages are, loss of material, and injury to the traffic. The system of layers appears more costly in the first instance than patching; but the expenses of cleaning and sweeping, the crushing and shorter duration of the metalling, and the inconvenience and the harm done to the traffic in the latter case, more than balances this in the case of roads of any importance. The advantage

of layers over patching is greater in proportion to the cost of the road metal; and the only serious drawback to the system is that, towards the close of the life of a layer, the road, having become hollow in the centre, is liable to be rapidly deteriorated in a very rainy season.

After giving some information on the best thicknesses for layers of metalling, and the points to be attended to for securing a good road, the author proceeds to the consideration of horse and steam rollers. As the resistance to traction is much greater over a new layer at the commencement of the rolling than towards the end, whilst the number of horses harnessed to a roller remains the same, it is advantageous to be able to double the weight of a roller. The Moquet roller realizes this condition, having a weight of from about $3\frac{1}{2}$ to $4\frac{3}{5}$ tons when empty, which can be increased to from 7 to $7\frac{1}{2}$ tons by filling two large, deep cases with gravel. The balance is secured by placing one case in front, and one behind the roller; and the lowness of the centre of gravity gives great stability. It is important that the roller should turn easily, to avoid having to unharness the horses; and the system in which the roller is formed, of four similar rings side by side, accomplishes this. An index recording the number of revolutions, with which the steam rollers are provided, would be a very useful adjunct to horse rollers. The heaviest rollers are the best for rolling new layers of metalling; but the weight is practically limited to what six or seven horses can draw, the largest number which is readily procurable, and which can be easily managed; and about 7 to 8 tons is the heaviest roller they can draw properly. The cost of traction with horses on main roads, with gradients not exceeding 1 in 33, allowing a turn for every 220 yards, is about 3*s.* 6*d.* per mile, and about $13\frac{3}{5}$ miles are traversed in a day of ten hours.

Four steam-rollers were purchased, within the last three years, for forming and repairing several main roads in the department of Oise, as it was doubtful whether sufficient horses could be procured for rolling in the ordinary way. Each roller cost £480, and weighed from 10 tons to $11\frac{1}{4}$ tons, according to its load of water and coal. The minimum pressure exerted by the steam roller per foot of width is $1\frac{1}{2}$ ton, which is the same as that of the half-loaded horse roller, whose smaller width compensates for its lighter weight. Owing to the greater width of the steam roller, a day's journey of $8\frac{2}{3}$ miles with it is equivalent to the $13\frac{3}{5}$ miles of the horse roller, and this distance is easily accomplished by the steam roller, so that its effective work is at least equal to that of a horse roller with six or seven horses, on roads with moderate gradients. In the exceptional case of gradients from 1 in 33 to 1 in 20, the steam roller is much more advantageous. The cost of traction with the minimum of six horses, is 38*s.* 4*d.* per day; whilst with the steam roller it is as follows:

Driver	8.0
Coal, 3.2 cwt. at 32*s.* per ton	5.12
Oil, waste, wood, &c.	2.08
Allowance for cleaning and stoppages	2.0
Total per day	17.2

which is less than half the cost of traction with horses. The actual cost of traction with a steam roller, in the author's district, per cubic yard of material used for repair, during 1880–82, amounts to $8\frac{1}{2}$*d.*; whereas the mean cost in another district, with horse rollers, was 15*d.*, making a saving with steam rolling of $6\frac{1}{2}$*d.* per cubic yard, or a total saving of £366 in two years. On this estimate, the cost of the steam roller would be saved in three years; and as, in the special work accomplished by the steam rollers, the cost with horse rollers would have really reached $25\frac{1}{2}$*d.*, instead of 15*d.*, the steam rollers have already effected a saving exceeding their cost. The possible objections to the use of steam rollers are shown to be immaterial; and the conclusion arrived at is, that steam rolling marks an important progress in the maintenance of ordinary roads.

M. Chevreul has probably enjoyed a longer working period than any other great scientific worker. About a month ago he communicated a paper to the Academie des Sciences, at the close of which he remarked: "The observation is not a new one to me; I had the honor to mention it here at the meeting on the 10th of May, 1812."

ON MORE CONVENIENT EQUIVALENTS FOR CONVERTING BRITISH INTO METRICAL MEASURES THAN THOSE HITHERTO IN USE.

By G. JOHNSTONE STONEY, D.Sc., F.R.S.

From "Nature."

Capt. Clarke's determination of the length of the British yard in metrical measure, made at Southampton in 1866 for the Ordnance Survey (see *Philosophical Transactions* for 1867), differs by a small amount from that which had been previously made by Capt. Kater, and it is noteworthy that the small difference between these excessively careful determinations is greater than the difference between Capt. Clarke's determination and the very simple equivalent,

The yard=914.4 millimeters;

so that the outstanding error which will be incurred if this very convenient number is adopted is of an amount which is inappreciable in ordinary good scientific work. It is less than the expansion produced in iron standards of length by one degree of temperature. Again, the pound avoirdupois differs, according to Prof. Miller's determination (which is the most elaborate we possess), from the simple equivalent,

The pound=453.6 grammes,

by only one-quarter of a grain avoirdupois in a kilogramme. This is about 1/70 of the correction which would have to be made in weighing water in order to reduce its apparent weight to its weight in vacuo, and is of small account even in carefully conducted scientific work. The value of the gallon, which follows from Capt. Clarke's determination of the meter, is 1.000027 times that adopted in Dowling's Metrical Tables, and differs from the simple equivalent,

The gallon=4544 cubic centimeters,

by an amount which is less than a cubic centimeter in ten liters, an error which is inappreciable; measures of capacity not admitting of being compared so closely as weights and measures of length. Hence we may take as our fundamental units—

The yard=914.4 millimeters,

with an error of less than a fifth-metret* in the meter, on the authority of Capt. Clarke;

The pound=453.6 grammes,

with an error of one-quarter of a grain avoirdupois in a kilogramme, on the authority of Prof. Miller;

The gallon=4544 cubic centimeters,

with an error of less than one cubic centimeter in ten liters, on the authority of the best previous determinations corrected by Capt. Clarke. It is a truly remarkable circumstance that the first of these numbers happens to be divisible by 3^2 and 2^3, the second by 2^3 and 7, and the third by 2^6. Divisors more convenient could hardly have been chosen for dealing with the disorderly way in which British measures are subdivided. They furnish the following tables, which may be safely recommended:

Table I.—Measures of Length.

The yard	= 914.4	millimeters.
The foot	= 304.8	"
The inch	= 25.4	"

Table II.—Weights.

The pound	= 453.6	grammes.
The half-pound	= 226.8	"
The quarter pound	= 113.4	"
The ounce	= 28.35	"
The grain	= .0648	"

[This last gives the gramme=15.43210 grains, a number which it is singularly easy to recollect.]

Table III.—Measures of Capacity.

The gallon	=4544	cubic centimeters.
The quart	=1136	"
The pint	= 568	"
The half-pint	= 284	"
The noggin	= 142	"
The fluid ounce	= 28.4	"

* By metrets are to be understood decimal subdivisions of the meter. The fifth-metret is the fifth of these, or the hundred-thousandth of a meter. It is about the diameter of one of the red disks in human blood.

If any person using these tables wishes to carry refinement farther, he may do so by subtracting one in every hundred thousand after using Table I, by subtracting one in sixty thousand after using Table II, and by subtracting one in ten thousand after using Table III. These corrections will carry accuracy to the limit of Prof. Miller's and Capt. Clarke's determinations.

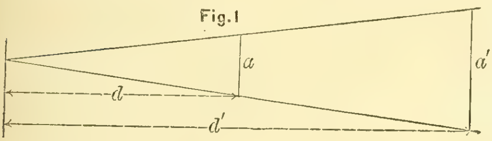
THE THEORY OF STADIA MEASUREMENTS ACCOMPANIED BY TABLES OF HORIZONTAL DISTANCES AND DIFFERENCES OF LEVEL FOR THE REDUCTION OF STADIA FIELD OBSERVATIONS.

By ARTHUR WINSLOW, Assistant Geologist, Second Geological Survey of Pennsylvania.

Contributed to Van Nostrand's Engineering Magazine.

The fundamental principle upon which stadia measurements are based, is the geometrical one that the lengths of parallel lines subtending an angle are proportional to their distances from its apex. Thus if, in Fig. 1, a represents the

Fig. 1



length of a line subtending an angle at a distance d from its apex, and a' the length of line, parallel to and twice the length of a, subtending the same angle at a distance d' from its apex, then will d' equal $2d$.

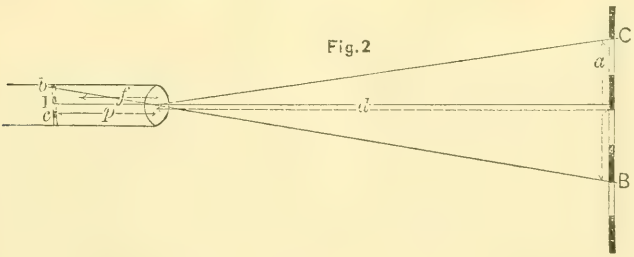
This is, in a general way, the underlying principle of stadia work; the nature of the instruments used, however, introduces several modifications, and these will be best understood by a consideration of the conditions under which such measurements are generally made.

In the telescopes of most instruments fitted for stadia work, there are placed either two horizontal wires (usually adjustable) or a glass with two etched horizontal lines at the position of the cross wires, and equidistant from the center wire.

A self-reading stadia rod is further provided, graduated according to the units of measurement used.

In a horizontal sight with such a telescope and rod, the stadia wires seem to be projected upon the rod and to intercept a distance which in Fig. 2 is represented by a.

Fig. 2



In point of fact there is formed, at the position of the stadia wires, a small conjugate image of the rod which the wires intersect at points b and c, which are respectively the foci of the points B and C on the rod. If, for simplicity's sake, the object glass be considered a simple bi-convex lens, then, by a principle of optics, the rays from any point of an object converge to a focus at such a position that a straight line, called a secondary axis, connecting the point with its image, passes through the *center* of the lens. This point of intersection of the secondary axes

is called the optical center. Hence, it follows that lines such as c C and b B, in Fig. 2, drawn from the stadia wires through the centre of the object glass will intersect the rod at points corresponding to those which the wires cut on the *image* of the rod. From this follows the proportion:

$$\frac{d}{p}=\frac{a}{\mathrm{I}}\therefore d=\frac{p}{\mathrm{I}}a, \quad . \quad . \quad . \quad (1)$$

Where:

d=the distance of the rod from the center of the objective;

p=the distance of the stadia wires from the center of the objective;

a=the distance intercepted on the rod by the stadia wires;

I= the distance of the stadia wires apart.

If p remained the same for all lengths of sight, then $\frac{p}{I}$ could be made a desirable constant and d would be directly proportional to a. Unfortunately, however, for the simplicity of such measurements, p (the focal length) varies with the length of the sight, increasing as the distance diminishes and *vice versa.* Thus the proportionality between d and a is variable.

The object, then, is to determine exactly what function a is of d and to express the relation in some convenient formula.

The general formula for bi-convex lenses is:

$$\frac{1}{p}+\frac{1}{p'}=\frac{1}{f}. \quad . \quad . \quad . \quad (2)$$

f is the *principal* focal length of the lens, and p and p' are the focal distances of image and object, and are *approximately* the same as p and d, respectively, in equation (1):

$$\text{therefore, } \frac{1}{p}+\frac{1}{d}=\frac{1}{f}, \text{ approximately.}$$

$$\text{and } \frac{d}{p}=\frac{d}{f}-1$$

$$\text{From (1), } \frac{d}{p}=\frac{a}{\mathrm{I}}$$

$$\therefore \frac{a}{\mathrm{I}}=\frac{d}{f}-1$$

$$\text{whence } d=\frac{f}{\mathrm{I}}a+f \quad . \quad . \quad . \quad (3)$$

In this formula, it will be noticed that, as f and I remain constant for sights of all lengths, the factor by which a is to be multiplied is a constant, and that d is thus equal to a constant times the length of a, plus f. This formula would seem, then, to express the relation desired, and it is generally considered as the fundamental one for stadia measurements. As above stated, however, the equation

$$\frac{1}{p}+\frac{1}{d}=\frac{1}{f}$$

is only *approximately* true and the conjunction of this formula with (1) being, therefore, not rigidly admissible, equation (3) does not express the exact relation.* The equation expressing the true relation, however, though differing from (3) in value, agrees with it in form and also in that the expression corresponding to $\frac{f}{\mathrm{I}}$ is a constant and that the amount to be added remains, practically, f. The constant corresponding to $\frac{f}{\mathrm{I}}$ may be called k† and thus the distance of the rod from the objective of the telescope is seen to be equal to a constant times the reading on the rod, plus the principal focal length of the objective. To obtain the exact distance to the *center* of the instrument, it is further necessary to add the distance of the objective from that centre, to f; which sum may be called c. The final expression for the distance, with a horizontal sight, is then

$$d=ka+c \quad . \quad . \quad (4)$$

The necessity of adding c is somewhat of an incumbrance. In the stadia work of the United States Government surveys an approximate method is adopted in which the total distance is read directly from the rod. For this method the rod is arbitrarily graduated, so that, at the distance of an average sight, the same number of units of the graduation are intercepted between the stadia wires on the rod, as units of length are con-

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* This is demonstrated on page 318.

† k is dependent upon I and can, therefore, be made a convenient value in any instrument fitted with adjustable stadia wires. It is generally made equal to 100, so that a reading on the rod of 1 corresponds to a distance of 100+f.

tained in the distance. For any other distance, however, this proportionality does not remain the same; for, according to the preceding demonstration, the reading on the rod is proportional to its distance, not from the center of the instrument, but from a point at a distance "c" in front of that center; so that, when the rod is moved from the position where the reading expresses the *exact* distance to a point, say half that distance from the instrument center, the reading expresses a distance *less* than half; and, at a point double that distance from the instrument center, the distance expressed by the reading is *more* than twice the distance. The error for all distances less than the average being minus, and for greater distances plus. The method is, however, a close approximation, and excellent results are obtained by its use.

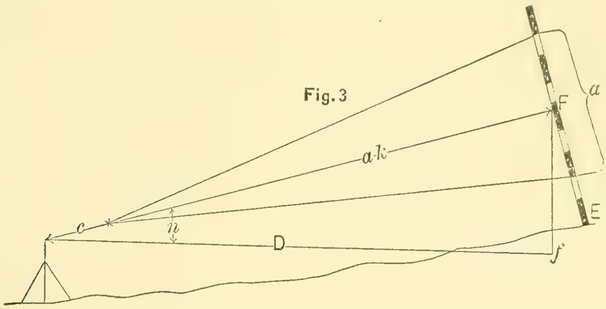


Fig. 3

Another method of getting rid of the necessity of adding the constant was devised by *Mr. Porro*, a Piedmontese, who constructed an instrument in which there was such a combination of lenses in the objective, that the readings on the rod, for all lengths of sight, were exactly proportional to the distances.* The instrument was, however, bulky and difficult to construct, and never came into extensive use.

For stadia measurements with inclined sights there are two modes of procedure. One, is to hold the rod at right angles to the line of sight; the other, to hold it vertical. With the first method it will be seen by reference to Fig. 3, that the distance read is not to the foot of the rod E, but to a point, f, vertically under the point, F, cut by the center wire. A correction has, therefore, to be made for this. An objection to this method is the difficulty of holding the rod at the same time in a vertical plane and inclined at a definite angle. Further, as the rod changes its inclination with each new position of the transit, the vertical angles of back and foresight are not measured from the same point.

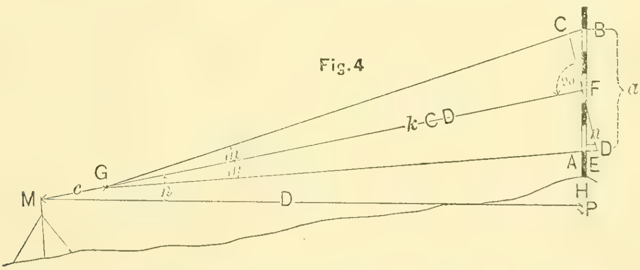


Fig. 4

The method usually adopted is the second, where the rod is always held *vertical*. Here, owing to the oblique view of the rod, it is evident that the space intercepted by the wires on the rod varies, not only with the distance, but also with the angle of inclination of the sight. Hence, in order to obtain the true distance from station to station, and also its vertical and horizontal components, a correction must be made for this oblique view of the rod. In Fig. 4,

* A notice of this instrument will be found in an article by Mr. Benjamin Smith Lymon, entitled "Telescopic Measurements in Surveying," in *Jour. Franklin Inst.*, May and June, 1868, and a fuller description is contained in *Annales des Mines*, Vol. XVI, fourth series.

$AB=a=$ the reading on the rod;

$MF=d=$ the inclined distance $=c+GF$ $=c+kCD$,

$MP=D=$ the horizontal distance $=d \cos n$,

$FP=Q=$ the vertical distance $=D \tan n$

$n=$ the vertical angle,

$AGB=2m$.

It is first required to express d in terms of a, n and m.

From the proportionality existing between the sides of a triangle and the sides of the opposite angles,

$$\frac{AF}{GF}=\frac{\sin m}{\sin\ [90^\circ+(n-m)]}$$

$$\text{or, } AF=GF \sin m \frac{1}{\cos\ (n-m)};$$

$$\text{and} \frac{BF}{GF}=\frac{\sin m}{\sin\ [90^\circ-(n+m)]}$$

$$\text{or, } BF=GF \sin m \frac{1}{(\cos n+m)};$$

$$\therefore AF+BF=GF \sin m\left(\frac{1}{\cos\ n-m)}+\frac{1}{(\cos\ (n+m))}\right)$$

$$AF+BF=a, \text{ and } GF=\frac{CD}{2}\frac{1}{\tan m}=\frac{CD \cos m}{2 \sin m}$$

By substituting and reducing to a common denominator,

$$a=\frac{CD}{2}\ \frac{\text{os}m\ [\cos\ (n+m)+\cos\ (n-m)]}{\cos\ (n+m) \cos\ (n-m)}$$

Reducing this according to trigonometrical formulæ,

$$CD=a\frac{\cos^2 n \cos^2 m-\sin^2 n \sin^2 \text{m}}{\cos n \cos^2 m}$$

as $d=MF=c+k.CD$,

$$\therefore\ d=c+k\,a\frac{\cos^2 n \cos^2 m-\sin^2 n \sin^2 m}{\cos n \cos^2 m}$$

The horizontal distance, $D=d \cos$ n.

$\therefore D=c \cos \text{n}+ka \cos^2 n-k\,a \sin^2 n \tan^2 m.$

"The third member of this equation may safely be neglected, as it is very small even for long distances and large angles of elevation (for 1500′, $n=45^\circ$ and $k=100$, it is but 0.07′.) Therefore, the final formula for distances, with a stadia kept vertical, and with wires equidistant from the center wire, is the following:"

$$D=c \cos n+a\,k \cos^2 n \quad . \quad . \ (5)$$

The vertical distance Q, is easily obtained from the relation: $Q=D \tan n$.

$$\therefore Q=c \sin n+a\,k \cos n \sin n$$

$$\text{or } Q=c \sin n+a\,k\frac{\sin 2n}{2} \quad . \quad (6)^*$$

With the aid of formulæ (5) and (6) the horizontal and vertical distances can be immediately calculated when the reading from a *vertical* rod, and the angle of elevation of any sight are given; and it is from these formulæ that I have calculated my stadia reduction tables. The values of $ak \cos^2 n$ and $ak\frac{\sin 2n}{2}$ were separately calculated for each two minutes up to 30 degrees of elevation; but, as the value of $c \sin n$ and $c \cos n$ have quite an inappreciable variation for 1 degree, it was thought sufficient to determine these values only for each degree. As c varies with different instruments these last two expressions were calculated for three different values of c, thus furnishing a ratio from which values of $c \sin n$ and $c \cos n$ can be easily determined for an instrument having any constant (c).

Similar tables have been computed by *J. A. Ockerson* and *Jared Teeple*, of the United States Lake Survey. Their use is, however, limited, from the fact that the meter is the unit of horizontal measurement while the elevations are in feet. The bulk of the tables furnish differences of level for stadia readings up to 400 meters, but only up to 10° of elevation. Supplementary tables give the elevations up to 30° for a distance of one meter. For obtaining horizontal distances reference has to be made to *another* table, which is somewhat an objectionable feature, and a multiplication *and* a subtraction has to be made in order to obtain the result. Last, but not least, these tables are, apparently, only accurate when used with an instrument whose constant is 0.43 meters.

* The above demonstration is substantially that given by Mr. George J. Specht, in an article on Topographical Surveying in *Van Nostrand's Engineering Magazine* for February, 1880, though enlarged and corrected.

As stated in the preceding discussion (p. 314), the generally accepted formula expressing the relation between the distance in a horizontal sight, the reading on the rod, the distance of the stadia wires apart, and the focal length of the objective is

$$d=\frac{f}{I}\,a+f \quad . \quad . \quad . \quad (3)$$

where d, a, I and f represent these factors respectively.

This formula is derived from the conjunction of the two equations:

$$d=\frac{p}{I}\,a; \quad . \quad . \quad . \quad . \quad (1)$$

$$\text{and}\ \frac{1}{p}+\frac{1}{p'}=\frac{1}{f}; \quad . \quad . \quad . \quad (2)$$

p and p' in (2) being considered as equal to p and d in (1). p and d in (1), it will be remembered, are the distances from the *center* of the objective to the image and object respectively. But the general formula for lenses, (2), is derived on the supposition that p and p' are measured from the *exterior faces* of the lens, and therefore p and d in (1) are each greater, by half the thickness of the lens, than p and p' in (2). Further, this formula is derived on the supposition that the object glass of the telescope is a simple, biconvex lens, whereas, in fact, it is a compound lens composed of a plano concave and a biconvex lens. Now, though these points may seem insignificant in themselves, they may greatly influence the final result, as a difference of only 1 in the denominator of such a fraction as $\frac{1{,}000{,}000}{2}$ may alter the result by as much as 500,000. Considerable thought and time has, therefore, been given to the consideration of the effect of these corrections, and, as a result, it was found that the formula (3) does not express the true relation even within practical limits; and that if it were attempted to calculate the distance, d, by this formula, when the factors f, p and a were given, a result would be obtained which would differ considerably from the real distance. The inaccuracy lies in the expression $\frac{f}{I}$. The one to be substituted for it is, however, like it, a constant for each instrument; and, as we determine the value of this constant by actual trial and not from a knowledge of the values of f and I, the correction to be made will not affect the practice.

Considering first the case of a telescope with a simple, biconvex lens, the optical center being, here, in the center of the lens, d and p, in equation (1), as before stated, are measured from the center of the lens, while, in equation (2), p and p' are measured from the exterior faces. If the thickness of the lens be taken as $2x$, then

p in equation (1)$=p$ in equation (2), minus x; and

p' in equation (1)$=$d in equation (2), minus x.

Therefore, while (1) remains

$$d=\frac{p}{I}a,\ \text{or}\ p=\frac{I}{a}d \quad . \quad (1a)$$

by substitution, (2), becomes,

$$\frac{1}{p-x}+\frac{1}{d-x}=\frac{1}{f} \quad . \quad . \quad (2a)$$

Substituting $d\frac{I}{a}$ for p in (2a)

$$\frac{1}{d\frac{I}{a}-x}\times\frac{1}{d-x}=\frac{1}{f}$$

$$\therefore\ d-x+d\frac{I}{a}-x=\frac{1}{f}\left(d-x\right)\left(d\frac{I}{a}-x.\right)$$

$$=\frac{1}{f}d^2\frac{I}{a}-\frac{1}{f}d\frac{I}{a}x-\frac{1}{f}dx+\frac{1}{f}x^2$$

$$\text{whence,}\ -2x-\frac{1}{f}x^2=\frac{1}{f}d^2\frac{I}{a}$$

$$-d\frac{1}{f}x\left(\frac{I}{a}+1\right)-d\left(\frac{I}{a}+1\right)$$

$$\text{or}\qquad =\frac{1}{f}d^2\frac{I}{a}-d\left\{\left(\frac{I}{a}+1\right)\left(\frac{1}{f}x+1\right)\right\}.$$

Multiplying both sides by $\frac{I}{a}\frac{1}{f}$,

$$-\frac{I}{a}\frac{1}{f}\left(2x+\frac{1}{f}x^2\right)=\frac{1}{f^2}d^2\frac{I^2}{a^2}$$

$$-\frac{I}{a}\frac{1}{f}d\left\{\left(\frac{I}{a}+1\right)\left(\frac{1}{f}x+1\right)\right\}$$

Adding to both sides $\dfrac{\left\{\left(\frac{I}{a}+1\right)\left(\frac{1}{f}x+1\right)\right\}}{2}$ squared,

$$\frac{\left\{\left(\frac{I}{a}+1\right)\left(\frac{1}{f}x+1\right)\right\}^2}{4}-\frac{I}{a}\frac{1}{f}\left(2x+\frac{1}{f}x^2\right)$$
$$=\left(d\frac{1}{f}\frac{I}{a}\right)^2-d\frac{1I}{fa}$$
$$\left\{\left(\frac{I}{a}+1\right)\left(\frac{1}{f}x+1\right)\right\}\times\frac{\left\{\left(\frac{I}{a}+1\right)\left(\frac{1}{f}x+1\right)\right\}^2}{4}$$

Extracting the square root of both terms,

$$\sqrt{\frac{\left\{\left(\frac{I}{a}+1\right)\left(\frac{1}{f}x+1\right)\right\}^2}{4}-\frac{1}{f}\frac{I}{a}\left(2x+\frac{1}{f}x^2\right)}=d\frac{1}{f}\frac{I}{a}$$
$$-\frac{\left(\frac{I}{a}+1\right)\left(\frac{1}{f}x+1\right)}{2}$$

Therefore,

$$\left\{\sqrt{\frac{\left\{\left(\frac{I}{a}+1\right)\left(\frac{1}{f}x+1\right)\right\}^2}{4}-\frac{1}{f}\frac{I}{a}\left(2x+\frac{1}{f}x^2\right)}+\frac{\left(\frac{I}{a}+1\right)\left(\frac{1}{f}x+1\right)}{2}\right\}\frac{af}{I}=k$$

or, $$d=\frac{(I+a)\,(x+f)}{2I}+\sqrt{\frac{[(I+a)(x+f)]^2}{4I^2}-\frac{a}{I}(x^2+2xf)}\quad(3a)$$

This is the *exact* formula corresponding to (3), for biconvex lenses. This can, however, be considerably reduced without materially affecting its value. With a telescope of the dimensions of that of an ordinary engineer's transit, the term $\frac{a}{I}(x^2+2xf)$ diminishes the result by about $\frac{1}{3}$ of an inch and, therefore, may be neglected. Formula ($3a$), then becomes:

$$d=2\frac{(I+a)\,(x+f)}{2I}$$
$$=\frac{Ix+If+ax+af}{I}$$
$$=a\frac{x+f}{I}+f+x$$

The addition of x (half the thickness of the object glass) would be inappreciable in the length of any ordinary sight, and may be omitted. The final expression becomes, then,

$$d=\frac{x+f}{I}a+f\qquad(3b)$$

This formula, it will be observed, differs from (3) in that the reading on the rod (a), is multiplied by $x+f$ instead of f. The numerical difference between the results is seen in the following examples:

Consider first the case with a one-foot reading on the rod, and let $x=.18''$, $f=9.00''$, and $I=.08''$.*

Formula (3) becomes, then:

$$d=\frac{9.00''}{.08''}\,12.00''+9.00''=1359''=113.25';$$

Formula (3b) becomes:

$$d=\frac{.18''+9.00''}{.08''}12.00''+9.00''=1386=115.50'$$

Difference = 2.25′

When the reading on the rod is 5 feet (or 60″) then, (3) becomes:

$$d=\frac{9.00''}{.08''}60.00''+9.00=563.25';$$

and (3b) becomes:

$$d=\frac{.18''+9.00''}{.08''}60.00''+9.00=574.50'$$

Difference =11.25′†

The above demonstration shows, then, that, with a simple biconvex object glass, the usually accepted formula expressing the relation between the distance, the reading on the rod, the distance of the stadia wires apart, and the focal length of the objective, is not accurate even within the limits of accuracy of such measurements. With the usual combination of lenses in objectives this error would still remain. The derivation of a formula similar to ($3b$), for such lenses, would, however, be extremely difficult, and would only hold for the special lens in question. For, with such a combination of lenses, the optical center would no longer remain in the center of the lens, but would vary its position according to the relative thicknesses of the two glasses, their radii of curvature and their

* These are very closely the dimensions in Heller & Brightly's large Surveyor's Transit (5-inch needle), as kindly furnished me by Mr. Heller.

† As the difference is evidently proportional to the length of sight, with a 1000′ sight it would amount to 22.5′, etc.

indices of refraction; and, after its position had been determined by abstruse calculation and refined experiment, its distance from the two exterior faces of the compound lens would be expressed by *two different* values (x and x') instead of two equal values (x); and this would very much complicate further calculation.

It was seen that, in the newly deduced formula, for biconvex objectives, like that heretofore accepted, the factor by which the reading on the rod is multiplied is a constant for each instrument, and that the practical method of adjusting the instrument remains the same. The question now arises, does this remain the case with a compound objective?

In view of the difficulty of demonstrating this mathematically it was decided to make a practical test of this point with a carefully adjusted instrument. A distance of 500 feet was first measured off on a level stretch of ground, and each 50 foot point accurately located. From one end of this line three successive series of stadia readings* were then taken from the first 50 foot and each succeeding 100 foot mark. The following table contains the results:

Distances.	Spaces Intercepted on the Rod.			
	1st Series.	2d Series.	3d Series.	Mean.
Feet.	Feet.	Feet.	Feet.	Feet.
50.00	.4850	.4860	.4855	.4855
100.00	.9850	.9870	.9830	.9850
200.00	1.9850	1 9860	1.9840	1.9850
300.00	2.9890	2.9875	2.9870	2.9878
400.00	3.9830	3.9800	3.9890	3.9840
500 00	4.9850	4.9850	4.9900	4.9867

Multiplying the mean of these readings by 100, and subtracting the result from the corresponding distance, we obtain the following table:

Distances.	Mean of Stadia Readings times 100.	Differences.	Variations from Mean.
Feet.	Feet.	Feet.	Feet.
50.00	48.55	1.45	+.02
100.00	98.50	1.50	+.07
200.00	198.50	1.50	+.07
300.00	298.78	1.22	−.21
400.00	398.40	1.60	+.17
500.00	498.67	1.33	−.10

Sum of Differences = 8.60;
Mean of Differences = 1.43.

The variations between the numbers of the column of differences are slight, the maximum from a mean value of 1.43 feet being only .21 feet. A study of the tables will show that these variations have no apparent relation to the length of the sight; in the maximum case, the variation corresponds to a reading on the rod of only .0021 feet (an amount much within the limits of accuracy of any ordinary sight). We are, therefore, perfectly justified in concluding that these variations are acci-

* The readings were taken from two targets, set so that the sight should be horizontal and thus also preventing any personal error or prejudice from affecting the reading.

dental, and that the "difference" is, for all practical purposes, a *constant* value.

We thus see that with a telescope having a compound, plano-convex objective, whatever the formula may be expressing the relation between d, f, x, etc., the horizontal distance is equal to a *constant* times the reading on the rod plus a *constant*, and may, as in the other cases, be expressed by the equation,

$$d = ak + c^*$$

The many advantages of stadia measurements in surveying need not be dwelt upon here, both because attention has been repeatedly called to them, and because they are self-evident to every engineer. Neither will it be within the compass of this article to describe the various forms of rods and instruments, or the conventionalities of stadia work.

A few precautions, necessary for accurate work, should, however, be emphasized. First, as regards the special adjustments: care should be taken that in setting the stadia wires† allowance be made for the instrument constant, and that the wires are so set that the reading, at any distance, is less than the true distance by the amount of this constant.‡

For accurate stadia work it is better to take the reading for both distances and elevations only at alternate stations and then to take them from both back and fore sights, in such a manner that the vertical angle is always read from the same position on each rod, which should be the average height of the telescope at the different stations.

Cases will, of course, occur where this method will be impracticable, and then the mode of procedure must be left to the judgment of the surveyor. If it be desired to have the absolute elevation of the ground under the instrument, the height of the telescope at each station will have to be measured by the rod, and the difference between this measurement and the average height used in sighting to the rod either added or subtracted as the case may be. This difference will ordinarily be so small, that in a great deal of stadia work no reduction will be necessary. In sighting to the rod for the angle of depression or elevation, the center horizontal wire must always be used. By this means an exactly continuous line is measured.

For theoretical exactness it is necessary that the stadia wires should be equidistant from the horizontal center wire, for, if this be not the case, the distance read is for an angle of elevation differing from the true one by an amount proportional to the displacement of the wires.

With reasonable care a high degree of accuracy can be attained in stadia measurements. The common errors of stadia reading are unlike the common errors of chaining, the gross ones (such as making a difference of a whole hundred feet) being, in general, the only important ones, and these are readily checked by double readings. To facilitate the subtraction of the reading of one cross hair from that of another, one should be put upon an even foot mark, and in the check reading the other one.

A general measure of the efficiency of stadia measurements is furnished in the professional papers of the Corps of Engineers, U. S. A., for 1882, on the Primary Triangulation of the Lake Survey, where it is stated that in computing coördinates of stadia work for 1875, the average amount of discrepancy in 141 lines, varying between 965 and 6,648 meters (mean 2,450 meters) when compared with lines determined by triangulation or chaining, was found to be 1 in 649. The maximum limit of error is put at 1 in 300.

Mr. Benjamin Smith Lyman, who has made extensive use of stadia work both in this country and Japan, considers it decidedly more accurate than ordinary good chaining, if the gross errors be carefully avoided.

The results of stadia survey which have come to my notice fully support this view. During the past summer I had occasion to run a continuous stadia line

* This may seem a statement of what was already a well-known fact. But, heretofore, it has been assumed to be a direct deduction from optical principles, and as, according to the preceding article, this is not so clearly evident, it seemed necessary to redetermine the point.

† This applies to an instrument with movable stadia wires, and not to one with etched lines on glass. In the latter case the graduation of the rod is the adjustable portion. It has been claimed as an advantage for etched lines on glass, that they are not affected by variations of temperature while the distance between stadia wires is. A series of tests which I made with one of Heller & Brightly's transits, to determine this point, showed no appreciable alteration in the space between the wires, as measured on a rod 500 feet distant, with a range of temperature between that produced in the instrument by the sun of a hot summer's day, and that produced by enveloping the telescope in a bag of ice.

‡ This is assuming the measurements to be made by the ordinary method, and not by the approximate one of the U. S. Engineers.

between two points some twelve miles apart. It was necessary that the position of these points with reference to each other should be determined as closely as possible with the means at hand. In consequence, the work was checked by taking duplicate vernier and stadia readings, and by taking a series of check sights to prominent objects. The latitudes and departures of this survey were afterwards calculated out, and the check angles computed and compared with those observed. The results are shown in the accompanying diagram and table. From the results of the tests tabulated on p.323, Mr. Lyman has kindly furnished me with the following deductions, as an indication of the exactness of stadia measurements.*

Taking the mean, 1.43, as exactly correct, we see, then, that the total error of the eighteen sights was only .06 feet, or $\frac{1}{77500}$ of the whole distance measured, 4,650 feet, a precision (as it happens) seven times greater than I suggested in my paper for a telescope magnifying ten times. But the mean of the errors .000817 (or $\frac{1}{1224}$), which, so far as the insufficient number of eighteen sights can show, would be the mean of the errors of an infinite number of trials, would correspond to a probable error for any one of the number of trials (that is, in gene-

* I wish to take this opportunity to acknowledge my indebtedness to Mr. Benjamin Smith Lyman, for the kindly interest he has taken in the above discussion, for his valuable suggestions, and for his assistance in referring me to various sources for information.

DIAGRAM OF CHECK SIGHTS
SCALE, 10000′=1″

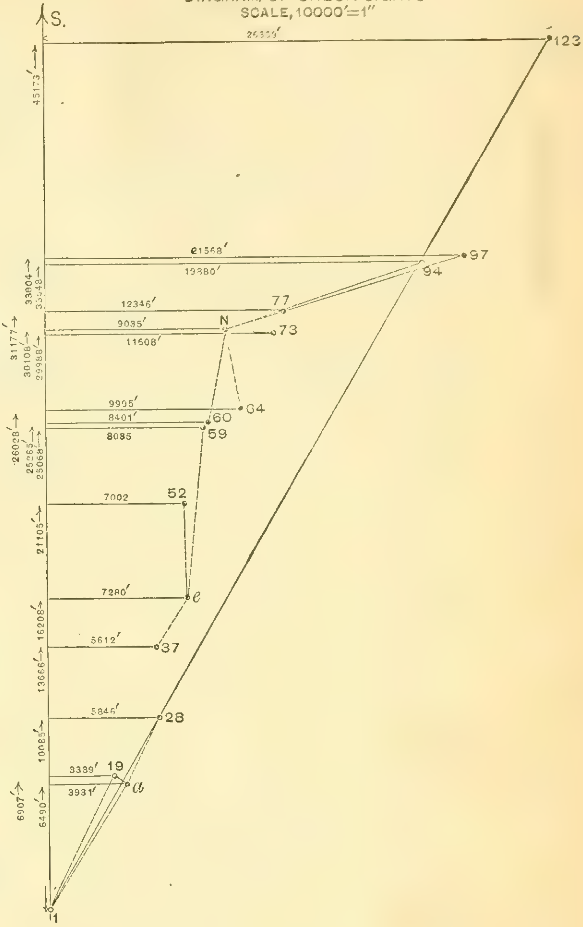


TABLE OF CHECK ANGLES.

Sights.	Course as read.	Course as Deduced.	Difference.	Minimum Proportional Linear Displacement.
19−1	N. 26° 9′ E.	N. 26° 8′ E.	0° 1′	1.3′ E.
28−*a*	N. 28° 8′ E.	N. 28° 3′ E.	0° 5′	5.6′ E.
59−*e*	N. 5° 9′ E.	N. 5° 12′ E.	0° 3′	7.6′ W.
73−N	S. 87° 14′ E.	S. 87° 19′ E.	0° 5′	3.6′ S.
77−N	N. 72° 8′ E.	N. 72° 7′ E.	0° 1′	1.0′ S.
94−N	N. 72° 26′ E.	N. 72° 24′ E.	0° 2′	6.0′ S.
97−N	N. 73° 35′ E.	N. 73° 34′ E.	0° 1′	3.6′ S.

"Distances.	Variations from Mean.	Error (or Variation from Mean) in Parts of the Distance Measured.	Mean Magnitude.
50 feet.	+.07 −.03 +.02	+.0014 −.0006 +.0004	.0008
100 "	+.07 −.13 +.27	+.0007 −.0013 +.0027	.0016
200 "	+.07 −.03 +.17	+.00035 −.0015 +.00085	.00045
300 "	−.33 −.18 −.13	−.0011 −.0006 −.00043	.00071
400 "	+.27 +.57 −.33	+.000675 +.0014 −.00082	.000965
500 "	+.07 +.07 −.43	+.00014 +.00014 −.00086	.00038
	+.22 +.27 −.43=6		6).004905
			.000817

ral) of the same kind, of .00069, or $\frac{1}{1450}$. This is within half the exactness I claimed as possible for the stadia in my paper. The difference may be due to several causes that I neglected to consider, such as a slight leaning of the rod forward or back, imperfect graduation of the rod, imperfect cleanliness or transparency of the glasses or of the air, imperfection in the shape of the lenses, or in their adjustments to one another, waviness from the varying refraction of the air with the heat from the sun and the ground, inaccurate focussing, inexact placing of the center hair upon the center of the target or graduation. This last difficulty might be avoided by taking one edge of the upper or lower cross hairs, and by special painting of the target for the center hair. * * * But at any rate the superior exactness of stadia measurement over chaining is shown, so far as eighteen trials could do it."

Tables of Horizontal Distances and Differences of Level for Stadia Measurements.

The formulæ used in the computation of the following tables, were those given by Mr. Geo. J. Specht in an article on Topographical Surveying, published in Van Nostrand's Engineering Magazine for February, 1880. These formulæ furnish expressions for horizontal distances and differences of level for stadia measurements with the conditions that the stadia rod *be held vertical*, and the stadia wires be equidistant from the centre wire. They are as follows:

$$D=c \cos n+ak \cos^2 n.$$

$$Q=D \tan n=c \sin n+\frac{ak \sin 2n.}{2}$$

D=Horizontal distance.

Q=Difference of level.

c=the distance from the center of the instrument to the center of the object glass, plus the focal length of the object glass.

k=the focal length of the object glass divided by the distance of the stadia wires apart.

a=the reading on the stadia rod.

n=the vertical angle.

ak is the reading on the rod multiplied by k, which is a constant for each instrument (generally 100.) In the tables the vertical columns consist of two series of numbers for each degree, which series represent respectively the different values of $ak \cos^2 n$ and $\frac{ak \sin 2n}{2}$ for every two minutes, when ak=100. To obtain the horizontal distance or the difference of level in any case, the corresponding value of $c \cos n$ or $c \sin n$ must further be added, and the mean of each of these expressions, for each degree, with 3 of the most common values of c, is given under each column.

As an example, let it be required to find the horizontal distance and the difference of level when, $n=+6° 18'$, $ak=570$, and the instrument constant, $c=.75$. In the column headed 6°, opposite 18′ in the series for "Hor. Dist.," we find 98.80 as the expression for $ak \cos^2 n$ when $ak=100$, therefore, when $ak=570$.

$ak \cos^2 n=98.80\times5.70=563.16$.

To this must be added $c \cos n$, which in this case is found in the subjoined column to be .75.

M.	0°		1°		2°		3°	
	Hor. Dist.	Diff. Elev.	Hor. Dist.	Diff. Elev.	Hor. Dist.	Diff. Elev.	Hor. Dist.	Diff. Elev.
0′....	100.00	.00	99.97	1.74	99.88	3.49	99.73	5.23
2.....	"	.06	"	1.80	99.87	3.55	99.72	5.28
4.....	"	.12	"	1.86	"	3.60	99.71	5.34
6.....	"	.17	99.96	1.92	"	3.66	"	5.40
8.....	"	.23	"	1.98	99.86	3.72	99.70	5.46
10.....	"	.29	"	2.04	"	3.78	99.69	5.52
12.....	"	.35	"	2.09	99.85	3.84	"	5.57
14.....	"	.41	99.95	2.15	"	3.90	99.68	5.63
16.....	"	.47	"	2.21	99.84	3.95	"	5.69
18.....	"	.52	"	2.22	"	4.01	99.67	5.75
20.....	"	.58	"	2.33	99.83	4.07	99.66	5.80
22.....	"	.64	99.94	2.38	"	4.13	"	5.86
24.....	"	.70	"	2.44	99.82	4.18	99.65	5.92
26.....	99.99	.76	"	2.50	"	4.24	99.64	5.98
28.....	"	.81	99.93	2.56	99.81	4.30	99.63	6.04
30.....	"	.87	"	2.62	"	4.36	"	6.09
32.....	"	.93	"	2.67	99.80	4.42	99.62	6.15
34.....	"	.99	"	2.73	"	4.48	"	6.21
36.....	"	1.05	99.92	2.79	99.79	4.53	99.61	6.27
38.....	"	1.11	"	2.85	"	4.59	99.60	6.33
40.....	"	1.16	"	2.91	99.78	4.65	99.59	6.38
42.....	"	1.22	99.91	2.97	"	4.71	"	6.44
44.....	99.98	1.28	"	3.02	99.77	4.76	99.58	6.50
46.....	"	1.34	99.90	3.08	"	4.82	99.57	6.56
48.....	"	1.40	"	3.14	99.76	4.88	99.56	6.61
50.....	"	1.45	"	3.20	"	4.94	"	6.67
52.....	"	1.51	99.89	3.26	99.75	4.99	99.55	6.73
54.....	"	1.57	"	3.31	99.74	5.05	99.54	6.78
56.....	99.87	1.63	"	3.37	"	5.11	99.53	6.84
58.....	"	1.69	99.88	3.43	99.73	5.17	99.52	6.90
60.....	"	1.74	"	3.49	"	5.23	99.51	6.96
c= .75	.75	.01	.75	.02	.75	.03	.75	.05
c=1.00	1.00	.01	1.00	.03	1.00	.04	1.00	.06
c=1.25	1.25	.02	1.25	.03	1.25	.05	1.25	.08

M.	8°		9°		10°		11°	
	Hor. Dist.	Diff. Elev.	Hor. Dist.	Diff. Elev.	Hor. Dist.	Diff. Elev.	Hor. Dist.	Diff. Elev.
0′....	98.06	13.78	97.55	15.45	96.98	17.10	96.26	18.73
2.....	98.05	13.84	97.53	15.51	96.96	17.16	96.24	18.78
4.....	98.03	13.89	97.52	15.56	96.94	17.21	96.32	18.84
6.....	98.01	13.95	97.50	15.62	96.92	17.26	96.29	18.89
8.....	98.00	14.01	97.48	15.67	96.90	17.32	96.27	18.95
10.....	97.98	14.06	97.46	15.73	96.88	17.37	96.25	19.00
12.....	97.97	14.12	97.44	15.78	96.86	17.43	96.23	19.05
14.....	97.95	14.17	97.43	15.84	96.84	17.48	96.21	19.11
16.....	97.93	14.23	97.41	15.89	96.82	17.54	96.18	19.16
18.....	97.92	14.28	97.39	15.95	96.80	17.59	96.16	19.21
20.....	97.90	14.34	97.37	16.00	96.78	17.65	96.14	19.27
22.....	97.88	14.40	97.35	16.06	96.76	17.70	96.12	19.32
24.....	97.87	14.45	97.33	16.11	96.74	17.76	96.09	19.38
26.....	97.85	14.51	97.31	16.17	96.72	17.81	96.07	19.43
28.....	97.83	14.56	97.29	16.22	96.70	17.86	96.05	19.48
30.....	97.82	14.62	97.28	16.28	96.68	17.92	96.03	19.54
32.....	97.80	14.67	97.26	16.33	96.66	17.97	96.00	19.59
34.....	97.78	14.73	97.24	16.39	96.64	18.03	95.98	19.64
36.....	97.76	14.79	97.22	16.44	96.62	18.08	95.96	19.70
38.....	97.75	14.84	97.20	16.50	96.60	18.14	95.93	19.75
40.....	97.73	14.90	97.18	16.55	96.57	18.19	95.91	19.80
42.....	97.71	14.95	97.16	16.61	96.55	18.24	95.89	19.86
44.....	97.69	15.01	97.14	16.66	96.53	18.30	95.86	19.91
46.....	97.68	15.06	97.12	16.72	96.51	18.35	95.84	19.96
48.....	97.66	15.12	97.10	16.77	96.49	18.41	95.82	20.02
50.....	97.64	15.17	97.08	16.83	96.47	18.46	95.79	20.07
52.....	97.62	15.23	97.06	16.88	96.45	18.51	95.77	20.12
54.....	97.61	15.28	97.04	16.94	96.42	18.57	95.75	20.18
56.....	97.59	15.34	97.02	16.99	96.40	18.62	95.72	20.23
58.....	97.57	15.40	97.00	17.05	96.38	18.68	95.70	20.28
60.....	97.55	15.45	96.98	17.10	96.36	18.73	95.68	20.34
c= .75	.74	.11	.74	.12	.74	.14	.73	.15
c=1.00	.99	.15	.99	.16	.98	.18	.98	.20
c=1.25	1.23	.18	1.23	.21	1.23	.23	1.22	.25

M.	4°		5°		6°		7°	
	Hor. Dist.	Diff. Elev.	Hor. Dist.	Diff. Elev.	Hor. Dist.	Diff. Elev.	Hor. Dist.	Diff. Elev.
0′....	95.51	6.96	99.24	8.68	98.91	10.40	98.51	12.10
2.....	"	7.02	99.23	8.74	98.90	10.45	98.50	12.15
4.....	99.50	7.07	99.22	8.80	98.88	10.51	98.48	12.21
6.....	99.49	7.13	99.21	8.85	98.87	10.57	98.47	12.26
8.....	99.48	7.19	99.20	8.91	98.86	10.62	98.46	12.32
10.....	99.47	7.25	99.19	8.97	98.85	10.68	98.44	12.38
12.....	99.46	7.30	99.18	9.03	98.83	10.74	98.43	12.43
14.....	"	7.36	99.17	9.08	98.82	10.79	98.41	12.49
16.....	99.45	7.42	99.16	9.14	98.87	10.85	98.40	12.55
18.....	99.44	7.48	99.15	9.20	98.80	10.91	98.39	12.60
20.....	99.43	7.53	99.14	9.25	98.78	10.96	98.37	12.66
22.....	99.42	7.59	99.13	9.31	98.77	11.02	98.36	12.72
24.....	99.41	7.65	99.11	9.37	98.76	11.08	98.34	12.77
26.....	99.40	7.71	99.10	9.43	98.74	11.13	98.33	12.83
28.....	99.39	7.76	99.09	9.48	98.73	11.19	98.31	12.88
30.....	99.38	7.82	99.08	9.54	98.72	11.25	98.29	12.94
32.....	99.38	7.88	99.07	9.60	98.71	11.30	98.28	13.00
34.....	99.37	7.94	99.06	9.65	98.69	11.36	98.27	13.05
36.....	99.36	7.99	99.05	9.71	98.68	11.42	98.25	13.11
38.....	99.35	8.05	99.04	9.77	98.67	11.47	98.24	13.17
40.....	99.34	8.11	99.03	9.83	98.65	11.53	98.22	13.22
42.....	99.33	8.17	99.01	9.88	98.64	11.59	98.20	13.28
44.....	99.32	8.22	99.00	9.94	98.63	11.64	98.19	13.33
46.....	99.31	8.28	98.99	10.00	98.61	11.70	98.17	13.39
48.....	99.30	8.34	98.98	10.05	98.60	11.76	98.16	13.45
50.....	99.29	8.40	98.97	10.11	98.58	11.81	98.14	13.50
52.....	99.28	8.45	98.96	10.17	98.57	11.87	98.13	13.56
54.....	99.27	8.51	98.94	10.22	98.56	11.93	98.11	13.61
56.....	99.26	8.57	98.93	10.28	98.54	11.98	98.10	13.67
58.....	99.25	8.63	98.92	10.34	98.53	12.04	98.08	13.73
60.....	99.24	8.68	98.91	10.40	98.51	12.10	98.06	13.78
c= .75	.75	.06	.75	.07	.75	.08	.74	.10
c=1.00	1.00	.08	.99	.09	.99	.11	.99	.13
c=1.25	1.25	.10	1.24	.11	1.24	.14	1.24	.16

M.	12°		13°		14°		15°	
	Hor. Dist.	Diff. Elev.	Hor. Dist.	Diff. Elev.	Hor. Dist.	Diff. Elev.	Hor. Dist.	Diff. Elev.
0′....	95.68	20.34	94.94	21.92	94.15	23.47	93.30	25.00
2.....	95.65	20.39	94.91	21.97	94.12	23.52	93.27	25.05
4.....	95.63	20.44	94.89	22.02	94.09	23.58	93.24	25.10
6.....	95.61	20.50	94.86	22.08	94.07	23.63	93.21	25.15
8.....	95.58	20.55	94.84	22.13	94.04	23.68	93.18	25.20
10.....	95.56	20.60	94.81	22.18	94.01	23.73	93.16	25.25
12.....	95.53	20.66	94.79	22.23	93.98	23.78	93.13	25.30
14.....	95.51	20.71	94.76	22.28	93.95	23.83	93.10	25.35
16.....	95.49	20.76	94.73	22.34	93.93	23.88	93.07	25.40
18.....	95.46	20.81	94.71	22.39	93.90	23.93	93.04	25.45
20.....	95.44	20.87	94.68	22.44	93.87	23.99	93.01	25.50
22.....	95.41	20.92	94.66	22.49	93.84	24.04	92.28	25.55
24.....	95.39	20.97	94.63	22.54	93.81	24.09	92.95	25.60
26.....	95.36	21.03	94.60	22.60	93.79	24.14	92.92	25.65
28.....	95.34	21.08	94.58	22.65	93.76	24.19	92.89	25.70
30.....	95.32	21.13	94.55	22.70	93.73	24.24	92.86	25.75
32.....	95.29	21.18	94.52	22.75	93.70	24.29	92.83	25.80
34.....	95.27	21.24	94.50	22.80	93.67	24.34	92.80	25.85
36.....	95.24	21.29	94.47	22.85	93.65	24.39	92.77	25.90
38.....	95.22	21.34	94.44	22.91	93.62	24.44	92.74	35.95
40.....	95.19	21.39	94.42	22.96	93.59	24.49	92.71	26.00
42.....	95.17	21.45	94.39	23.01	93.56	24.55	92.68	26.05
44.....	95.14	21.50	94.36	23.06	93.53	24.60	92.65	26.10
46.....	95.12	21.55	95.34	23.11	93.50	24.65	92.62	26.15
48.....	95.09	21.60	94.31	23.16	93.47	24.70	92.59	26.20
50.....	95.07	21.66	94.28	23.22	93.45	24.75	92.56	26.25
52.....	95.04	21.71	94.26	23.27	93.42	24.80	92.53	26.30
54.....	95.02	21.76	94.23	23.32	93.39	24.85	92.49	26.35
56.....	94.99	21.81	94.20	23.37	93.36	24.90	92.46	26.40
58.....	94.97	21.87	94.17	23.42	93.33	24.95	92.43	26.45
60.....	94.94	21.92	94.15	23.47	93.30	25.00	92.40	26.50
c= .75	.73	.16	.73	.17	.73	.19	.72	.20
c=1.00	.98	.22	.97	.23	.97	.25	.96	.27
c=1.25	1.22	.27	1.21	.29	1.21	.31	1.20	.34

M.	16° Hor. Dist.	16° Diff. Elev.	17° Hor. Dist.	17° Diff. Elev.	18° Hor. Dist.	18° Diff. Elev.	19° Hor. Dist.	19° Diff. Elev.
0′....	92.40	26.50	91.45	27.96	90.45	29.39	89.40	30.78
2.....	92.37	26.55	91.42	28.01	90.42	29.44	89.36	30.83
4.....	92.34	26.59	91.39	28.06	90.38	29.48	89.33	30.87
6.....	92.31	26.64	91.35	28.10	90.35	29.53	89.29	30.92
8.....	92.28	26.69	91.32	28.15	90.31	29.58	89.26	30.97
10.....	92.25	26.74	91.29	28.20	90.28	29.62	89.22	31.01
12.....	92.22	26.79	91.36	28.25	90.24	29.67	89.18	31.06
14.....	92.19	26.84	91.22	28.30	90.21	29.72	89.15	31.10
16.....	92.15	26.89	91.19	28.34	90.18	29.76	89.11	31.15
18.....	92.12	26.94	91.16	28.39	90.14	29.81	89.08	31.19
20.....	92.09	26.99	91.12	28.44	90.11	29.86	89.04	31.24
22.....	92.06	27.04	91.09	28.49	90.07	29.90	89.00	31.28
24.....	92.03	27.09	91.06	28.54	90.04	29.95	88.96	31.33
26.....	92.00	27.23	91.02	28.58	90.00	30.00	88.93	31.38
28.....	91.97	27.18	90.99	28.63	89.97	30.04	88.89	31.42
30.....	91.93	27.23	90.96	28.68	89.93	30.09	88.86	31.47
32.....	91.90	27.28	90.92	28.73	89.90	30.14	88.82	31.51
34.....	91.87	27.33	90.89	28.77	89.86	30.19	88.78	31.56
36.....	91.84	27.38	90.86	28.82	89.83	30.23	88.75	31.60
38.....	91.81	27.43	90.82	28.87	89.79	30.28	88.71	31.65
40.....	91.77	27.48	90.79	28.92	89.76	30.32	88.67	31.69
42.....	91.74	27.52	90.76	28.96	89.72	30.37	88.64	31.74
44.....	91.71	27.57	90.72	29.01	89.69	30.41	88.60	31.78
46.....	91.68	27.62	90.69	29.06	89.65	30.46	88.56	31.83
48.....	91.65	27.67	90.66	29.11	89.61	30.51	88.53	31.87
50.....	91.61	27.72	90.62	29.15	89.58	30.55	88.49	31.92
52.....	91.58	27.77	90.59	29.20	89.54	30.60	88.45	31.96
54.....	91.55	27.81	90.55	29.25	89.51	30.65	88.41	32.01
56.....	91.52	27.86	90.52	29.30	89.47	30.69	88.38	32.05
58.....	91.48	27.96	90.48	29.34	89.44	30.74	88.34	32.09
60.....	91.45	27.96	90.45	29.39	89.40	30.78	88.30	32.14
c= .75	.72	.21	.72	.23	.71	.24	.71	.25
c=1.00	.86	.28	.95	.30	.95	.32	.94	.33
c=1.25	1.20	.35	1.19	.38	1.19	.40	1.18	.42

M.	24° Hor. Dist.	24° Diff. Elev.	25° Hor. Dist.	25° Diff. Elev.	26° Hor. Dist.	26° Diff. Elev.	27° Hor. Dist.	27° Diff. Elev.
0′....	83.46	37.16	82.14	38.30	80.78	39.40	79.39	40.45
2.....	83.41	37.20	82.09	38.34	80.74	39.44	79.34	40.49
4.....	83.37	37.23	82.05	38.38	80.69	39.47	79.30	40.52
6.....	83.33	37.27	82.01	38.41	80.65	39.51	79.25	40.55
8.....	83.28	37.31	81.96	38.45	80.60	39.54	79.20	40.59
10.....	83.24	37.35	81.92	38.49	80.55	39.58	79.15	40.62
12.....	83.20	37.39	81.87	38.53	80.51	39.61	79.11	40.66
14.....	83.15	37.43	81.83	38.56	80.46	39.65	79.06	40.69
16.....	83.11	37.47	81.78	38.60	80.41	39.69	79.01	40.72
18.....	83.07	37.51	81.74	38.64	80.37	39.72	78.96	40.76
20.....	83.02	37.54	81.69	38.67	80.32	39.76	78.92	40.79
22.....	82.98	37.58	81.65	38.71	80.28	39.79	78.87	40.82
24.....	82.93	37.62	81.60	38.75	80.23	39.83	78.82	40.86
26.....	82.89	37.66	81.56	38.78	80.18	39.86	78.77	40.89
28.....	82.85	37.70	81.51	38.82	80.14	39.90	78.73	40.92
30.....	82.80	37.74	81.47	38.86	80.09	39.93	78.63	40.96
32.....	82.76	37.77	81.42	38.89	80.04	39.97	78.63	40.99
34.....	82.72	37.81	81.38	38.93	80.00	40.00	78.58	41.02
36.....	82.67	37.85	81.33	38.97	79.95	40.04	78.54	41.06
38.....	82.63	37.89	81.28	39.00	79.90	40.07	78.49	41.09
40.....	82.58	37.93	81.24	39.04	79.86	40.11	78.44	41.12
42.....	82.54	37.96	81.19	39.08	79.81	40.14	78.39	41.16
44.....	82.49	38.00	81.15	39.11	79.76	40.18	78.34	41.19
46.....	82.45	38.04	81.10	39.15	79.72	40.21	78.30	41.22
48.....	82.41	38.08	81.06	39.18	79.67	40.24	78.25	41.26
50.....	82.36	38.11	81.01	39.22	79.62	40.28	78.20	41.29
52.....	82.32	38.15	80.97	39.26	79.58	40.31	78.15	41.32
54.....	82.27	38.19	80.92	39.29	79.53	40.35	78.10	41.35
56.....	82.23	38.23	80.87	39.33	79.48	40.38	78.06	41.39
58.....	82.18	38.26	80.83	39.36	79.44	40.42	78.01	41.42
60.....	82.14	38.30	80.78	39.40	79.39	40.45	77.96	41.45
c= .75	.68	.31	.68	.32	.67	.33	.66	.35
c=1.00	.91	.41	.90	.43	.89	.45	.89	.46
c=1.25	1.14	.52	1.13	.54	1.12	.56	1.11	.58

M.	20° Hor. Dist	20° Diff. Elev.	21° Hor. Dist.	21° Diff. Elev.	22° Hor. Dis.	22° Diff. Elev.	23° Hor. Dist.	23° Diff. Elev.
0′....	88.30	32.14	87.16	33.46	85.97	34.73	84.73	35.97
2.....	88.26	32.18	87.12	33.50	85.93	34.77	84.69	36.01
4.....	88.23	32.23	87.08	33.54	85.89	34.82	84.65	36.05
6.....	88.19	32.27	87.04	33.59	85.85	34.86	84.61	36.09
8.....	88.15	32.32	87.00	33.63	85.80	34.90	84.57	36.13
10.....	88.11	32.36	86.96	33.67	85.76	34.94	84.52	36.17
12.....	88.08	32.41	86.92	33.72	85.72	34.98	84.48	36.21
14.....	88.04	32.45	86.88	33.76	85.68	35.02	84.44	36.25
16.....	88.00	32.49	86.84	33.80	85.64	35.07	84.40	36.29
18.....	87.96	32.54	86.80	33.84	85.60	35.11	84.35	36.33
20.....	87.93	32.58	86.77	33.89	88.56	35.15	84.31	36.37
22.....	87.89	32.63	86.73	33.93	85.52	35.19	84.27	36.41
24.....	87.85	32.67	86.69	33.97	85.48	35.23	84.23	36.45
26.....	87.81	32.72	86.65	34.01	85.44	35.27	84.18	36.49
28.....	87.77	32.76	86.61	34.06	85.40	35.31	84.14	36.53
30.....	87.74	32.80	86.57	34.10	85.36	35.36	84.10	36.57
32.....	87.70	32.85	86.53	34.14	85.31	35.40	84.06	36.61
34.....	87.66	32.89	86.49	34.18	85.27	35.44	84.01	36.65
36.....	87.62	32.93	86.45	34.23	85.23	35.48	83.97	36.69
38.....	87.58	32.98	86.41	34.27	85.19	35.52	83.93	36.73
40.....	87.54	33.02	86.37	34.31	85.15	35.56	83.89	36.77
42.....	87.51	33.07	86.33	34.35	85.11	35.60	83.84	36.80
44.....	87.47	33.11	86.29	34.40	85.07	35.64	83.80	36.84
46.....	87.43	33.15	86.25	34.44	85.02	35.68	83.76	36.88
48.....	87.39	33.20	86.21	34.48	84.98	35.72	83.72	36.92
50.....	87.35	33.24	86.17	34.52	84.94	35.76	83.67	36.96
52.....	87.31	33.28	86.13	34.57	84.90	35.80	83.63	37.00
54.....	87.27	33.33	86.09	34.61	84.86	35.85	63.59	37.04
56.....	87.24	33.37	86.05	34.65	84.82	35.89	83.54	37.08
58.....	87.20	33.41	86.01	34.69	84.77	35.93	83.50	37.12
60.....	87.16	33.46	85.97	34.73	84.73	35.97	83.46	37.16
c= .75	.70	.26	.70	.27	.69	.29	.69	.30
c=1.00	.94	.35	.93	.37	.92	.38	.92	.40
c=1.25	1.17	.44	1.16	.46	1.15	.48	1.15	.50

M.	28° Hor. Dist.	28° Diff. Elev.	29° Hor. Dist.	29° Diff. Elev.	30° Hor. Dist.	30° Diff. Elev.
0′....	77.96	41.45	76.50	42.40	75.00	43.33
2.....	77.91	41.48	76.45	42.43	74.95	43.30
4.....	77.86	41.52	76.40	42.46	74.90	43.36
6.....	77.81	41.55	76.35	42.49	74.85	43.39
8.....	77.77	41.58	76.30	42.53	74.80	43.42
10.....	77.72	41.61	76.25	42.56	74.75	43.45
12.....	77.67	41.65	76.20	42.59	74.70	43.47
14.....	77.62	41.68	76.15	42.62	74.65	43.50
16.....	77.57	41.71	76.10	42.65	74.60	43.53
18.....	77.52	41.74	76.05	43.68	74.55	43.56
20.....	77.48	41.77	76.00	42.71	74.49	43.59
22.....	77.42	41.81	75.95	42.74	74.44	43.62
24.....	77.38	41.84	75.90	42.77	74.39	43.65
26.....	77.33	41.87	75.85	42.80	74.34	43.67
28.....	77.28	41.90	75.80	42.83	74.29	43.70
30.....	77.23	41.93	75.75	42.86	74.24	43.73
32.....	77.18	41.97	75.70	42.89	74.19	43.76
34.....	77.13	42.00	75.65	42.92	74.14	43.79
36.....	77.09	42.03	75.60	42.95	74.09	43.82
38.....	77.04	42.06	75.55	42.98	74.04	43.84
40.....	76.99	42.09	75.50	43.01	73.99	43.87
42.....	76.94	42.12	75.45	43.04	73.93	43.90
44.....	76.89	42.15	75.40	43.07	73.88	43.93
46.....	76.84	42.19	75.35	43.10	73.83	43.95
48.....	76.79	42.22	75.30	43.13	73.78	43.98
50.....	75.74	42.25	75.25	43.16	74.73	44.01
52.....	76.69	42.28	75.20	33.18	73.68	44.04
54.....	76.64	42.31	75.15	42.21	73.63	44.07
56.....	76.59	42.34	75.10	43.24	73.58	44.09
58.....	76.55	42.37	75.05	43.27	73.52	44.12
60.....	76.50	42.40	75.00	43.30	73.47	44.15
c= .75	.66	.36	.65	.37	.65	.38
c=1.00	.88	.48	.87	.49	.86	.51
c=1.25	1.10	.60	1.09	.62	1.08	.64

$563.16+.75=563.91$, which is the required horizontal distance.

In a similar manner,

$10.91\times5.70+.08=62.27$ is the required difference of level. One multiplication and one addition must be made in each case.

It is to be noticed, that, with the smaller angles, cos n may be neglected in the expression c cos n, and c sin n may be entirely neglected, without appreciable error.

For values of c which differ from those given, an approximate correction proportional to the amount of difference may very easily be made in these two expressions.

TESTING MACHINES, THEIR HISTORY, CONSTRUCTION AND USE.

By ARTHUR V. ABBOTT.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

II.

QUALIFICATIONS OF TESTING MACHINES.

A testing machine should possess the following qualifications:

FIRST, ACCURACY.—The accuracy of a testing machine should be commensurate with the work that it is required to perform. A testing machine used for breaking bridge bars would not need that accuracy or sensitiveness which would be necessary in a contrivance designed to test silk thread.

SECOND, THE CAPABILITY OF BEING TESTED.—Even the most perfect mechanical contrivances are liable to derangement, and the testing machine, used for heavy work, constantly being subjected to the most severe shocks, its parts being strained to an amount exceeding that provided for in the original design, is especially liable to get out of order. Obviously, a machine so designed as to be tested with ease and facility is exceedingly desirable.

THIRD, A DESIGN EMBRACING A READY ADAPTABILITY TO VARIOUS SHAPES AND SIZES OF SPECIMENS.—In the miscellaneous practice of making experiments on various kinds of material in various stresses, it becomes exceedingly important to a rapid and economical use of the testing machine that it may be readily changed from tension to compression, from compression to torsion, from torsion to transverse stress, and that it should be so planned as to take in a long or short specimen with equal facility.

FOURTH, A RAPID MEANS OF MANIPULATION.—The method of applying the stress, of adjusting the specimen in the machine, and of arranging the various parts, one with reference to another, should be so designed that even in the largest machines all the manipulation that is necessary to making a test itself can be accomplished by a single skilled operator.

FIFTH, SELF-RECORDING DEVICES.—As far as is possible in mechanical construction, the machine should be so planned as to make its own record. The advantages of this may be readily seen in the fact that it prevents the possibility of mistakes on the part of the operator, who, even with the most perfect machines, has so much to do in watching the behavior of the specimen during the test, in regulating the rapidity and direction of application of the stress, as to render it expedient to make the machine itself record, as far as possible, all the important facts that are developed by the experiment. In the light of the foregoing qualifications a number of machines may be now considered.

The Gill machine may be regarded as an exceedingly compact and in many respects desirable form of testing machine for investigating the qualities of specimens of materials. The machine occupies comparatively little room. It may be readily manipulated by means of a hand wheel and belt for communicating power to the screw. The two cross-heads

of the machine are accurately bored after all parts of the machine are set up. As a consequence of this, the axis of the machine coincides almost exactly with the axis of stress applied by the machine, and if the specimen to be tested be carefully turned or planed from end to end, and if exceeding care be exercised in setting the piece in the jaws so that the center of the piece shall agree with the center of the testing machine, it is probable that the axis of stress will nearly or quite coincide with the axis of the piece. The stress produced on the specimen is estimated by the system of levers placed above it. There is, however, no method of testing the accuracy of the machine, except by calculating the ratios of the levers or by taking the machine apart and testing each lever separately, and when first built there is no doubt of a reasonable amount of accuracy. Still there being no method of testing the co-efficients of friction of the levers in their actual place, and no method of ascertaining how accurately the entire apparatus weighs, there would arise in minds at least some question as to the results. The Gill machine is simply arranged for tension and requires additional appliances occupying some considerable time in attaching and adjusting to enable it to operate in compression, while the other stresses, such as shearing, bulging, punching, torsion and the like require special appliances not furnished with the machine.

The Olsen machine possesses some points of advantage over the Gill. Here the stress is transmitted through the specimen to the small platform mounted on a different lever contained underneath. By removing the columns, and the crossheads with the screws from the machine, weights to any amount may be piled on the platform by placing thereon a support large enough to contain them. By this means it is possible to test the accuracy of the machine. The use of the differential lever may, however, be somewhat criticised as being liable to a much greater variation in accuracy than the ordinary simple lever system. Here the change from tension to compression is comparatively easily made, for all that is necessary is to place the specimen under the cross-head instead of on the top of it.

The change to transverse, shearing and torsion specimens is made with considerable difficulty. In fact the transverse specimens of any great length are with difficulty accommodated.

In the Riehle machine, tension and compression specimens are easily arranged, but like the Gill machine there are no facilities for testing the machine itself, and reliance must be placed on the accuracy and the care of the makers.

The Emery machine exemplifies one of the best possible contrivances for rapidly changing from tension to compression. Here the hydraulic support is so arranged as to weigh equally as well on either side of it. The jack is sustained on the adjusting screws of the machine by two large bronze nuts, so that all that is necessary to do to change from tension to compression is to reverse the direction of the fluid in the jack so as to cause it to pass from the front to the back of the piston. The method of providing for different lengths of specimens is exceedingly rapid. Along the bottom of the machine there passes a driving shaft connecting with a system of gear work for turning the bronze nuts. By simply starting the belt for communicating power to the shaft, these nuts may be turned either backward or forward at pleasure, and the jack in a short time adjusted to any different length of specimen. The machine, however, has no appliances for making tests in other stresses than those of tension and compression. Being a horizontal machine, it is not susceptible of an easy test. In order to ascertain whether the accuracy of the machine is maintained, it is necessary to take the weighing end of the machine apart, turn the hydraulic support from its seat, and build a platform to contain the necessary amount of standard weights. While there is every presumption that the Emery machine, having been once carefully and accurately standarized, will for a long time retain this accuracy, there will arise in the minds of those subject to accurate scientific considerations, that the change in the molecular constitution of the diaphgram and of the suspended metallic strips, may sooner or later introduce some (be it very slight) error in the accuracy of this contrivance, so that without some easy method of frequent standardization, the results of the machine are liable to criticism. The only method of insuring the coincidents of the axis of

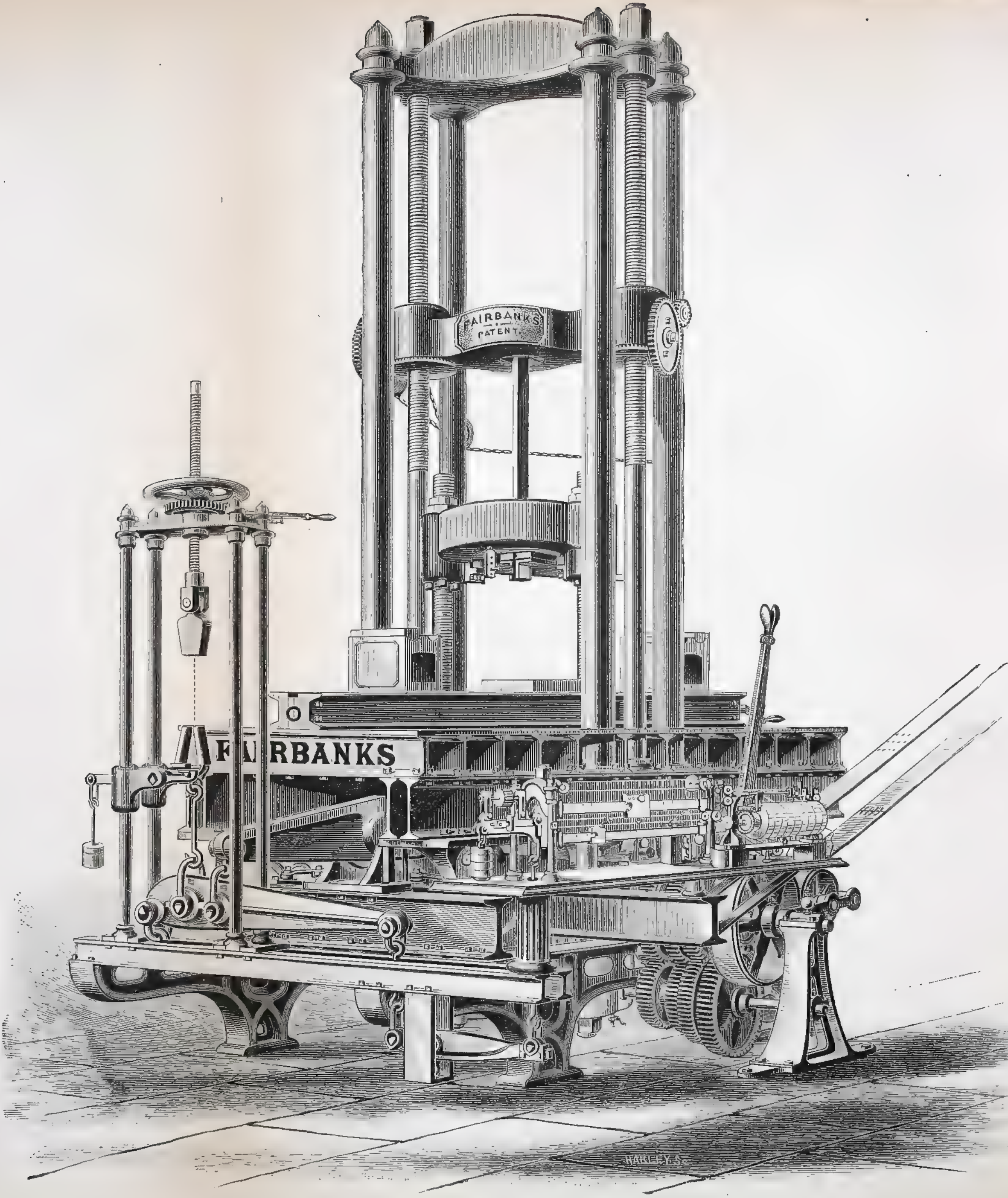
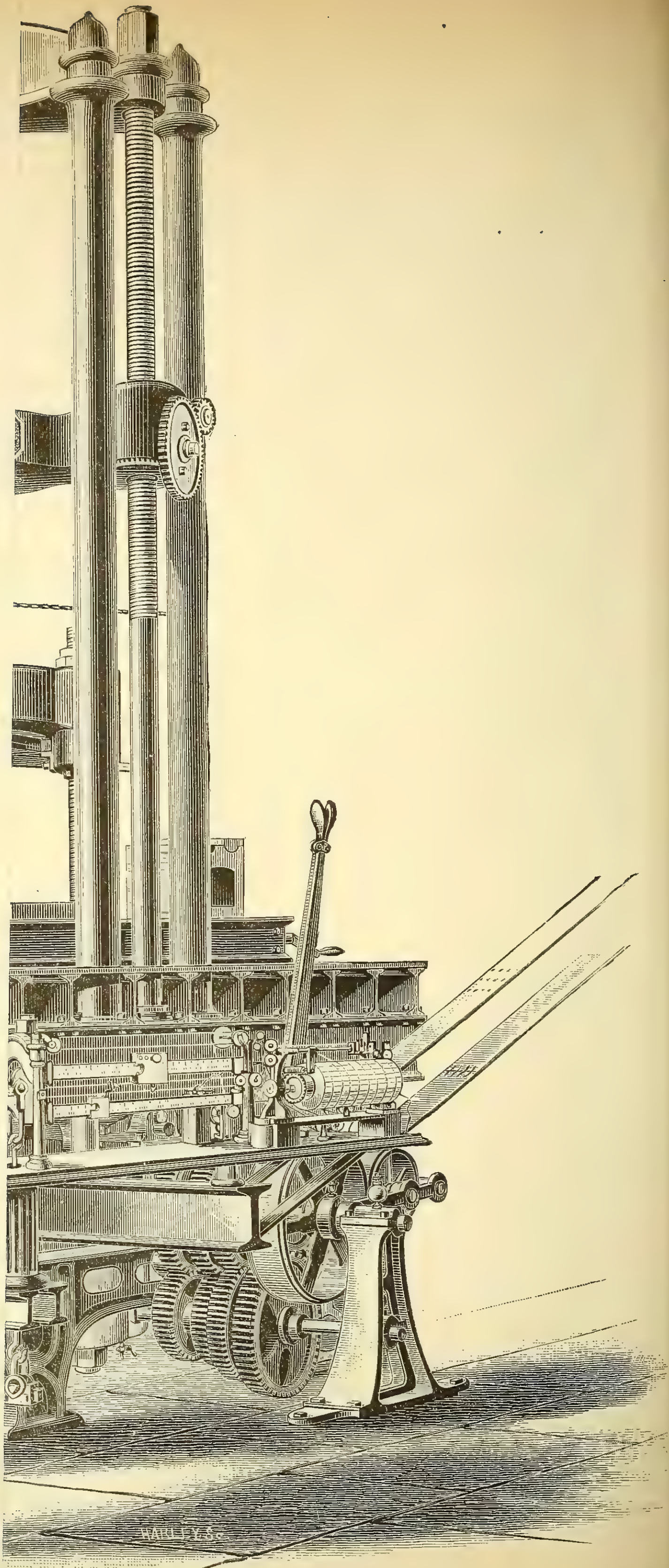


Fig. 8.



stress and the axis of the specimen is by carefully measuring and centering the piece with reference to the axis of the machine, and in one built on so large a scale as that at the Watertown Arsenal, much more time may be consumed in this process of centering than in any other part of the test.

In Fig. 8 a large machine on the Fairbanks' system may be seen, which has recently been constructed with a view to improve on previous machines, and to fulfill the foregoing conditions as far as possible. The lower part of the machine consists of a frame-work of wrought-iron I-beams, standing on two heavy cast iron legs. This frame-work of I-beams supports in the center two screws that may be rotated by the belt or a gearing shown at the left hand. These screws rise in the center of the testing machine and carry the lower cross-head to which one end of the specimen to be examined is attached. Just outside these screws stand a second pair of screws, carrying a top cross-head for the reception of the other end of the test piece. These screws are suspended from a large casting supported on four columns that stand on a second frame-work of I-beams that form a platform of the machine. This large platform some 4 ft. by 10 ft. stands on four levers in a manner similar to that described in the previous testing machine of this system. These levers transfer their stress to two central levers, whence it runs to a couple of multiplying levers, shown in the front of the machine, and then directly to the scale beam. Just in front of the testing machine, standing over the first one of the multiplying levers, may be seen a small frame-work composed of iron columns, carrying on its top a hand wheel and lever. This is an auxiliary testing machine adapted to making tests on small specimens. The large machine has a capacity of 200,000 lbs., and is adapted to take in tension specimens up to 8 ft. in length, transverse specimens to 10 ft., compression specimens to 4 ft., and specimens in sheering, bulging, punching and torsion to the 200,000 lbs. capacity, the smallest reading on the beam being 10 lbs. The auxiliary machine is adapted to making tests in tension and compression up to 2 ft. in length, and has a capacity of 10,000 lbs., and reads to $\frac{1}{2}$ lb. Thus it will be seen that the apparatus is susceptible of doing work of all kinds up to its maximum capacity. Should a specimen of wire be presented, it may be tested in the auxiliary part of the apparatus to a half-pound, rapidly and easily, while if the next specimen be a 15-in. I-bar, 10 ft. in length, it may be placed on transverse blocks of the large machine and broken in a few moments. The general features of the machine being here illustrated, a more complete idea may be obtained by reference to the following cross-sections: the first one being taken longitudinally through the center of the machine, and the second transversely through the main levers of the scale system. The cast iron legs supporting the platform will be seen here in side elevation and in cross-section, the main levers c and c hang from the ends of these I-beams, supported by two cast iron blocks placed over the swinging bearing pivots from the loops suspended by pins placed through these blocks. The ends of these main levers may be seen to be attached to the two center levers e and e', whereby the stress is conveyed to the end of the testing machine, and to the two multiplying levers previously alluded to, and thence to the weighing beam. Directly under the I-beams, supporting the scale may be seen a secondary framework, carrying the screws with their appropriate worm-gears. By means of the main shaft l, with its system of gearing, the worms meshing into the worm-gears at the base of the screws may be rotated. The power is derived from the belt l^3, which derives a counter shaft set directly over the main shaft l. By means of the gears on the shaft, several different speeds may be obtained according to the judgment of the operator as he may deem it best to apply the stress to the specimen rapidly or slowly. Directly at the right of these multiplying gears is a system of reverse gears very similar to those employed in a large engine lathe. A lever at the side of the testing machine enables the operator at pleasure to reverse the direction of rotation of the main screws. The worm-gears and main screws are cut respectively right and left-handed, so that the friction of one neutralizes that produced by the other, and all tendency on the part of the piece to twist or get out of order is thereby avoided. On the pivot c of the main

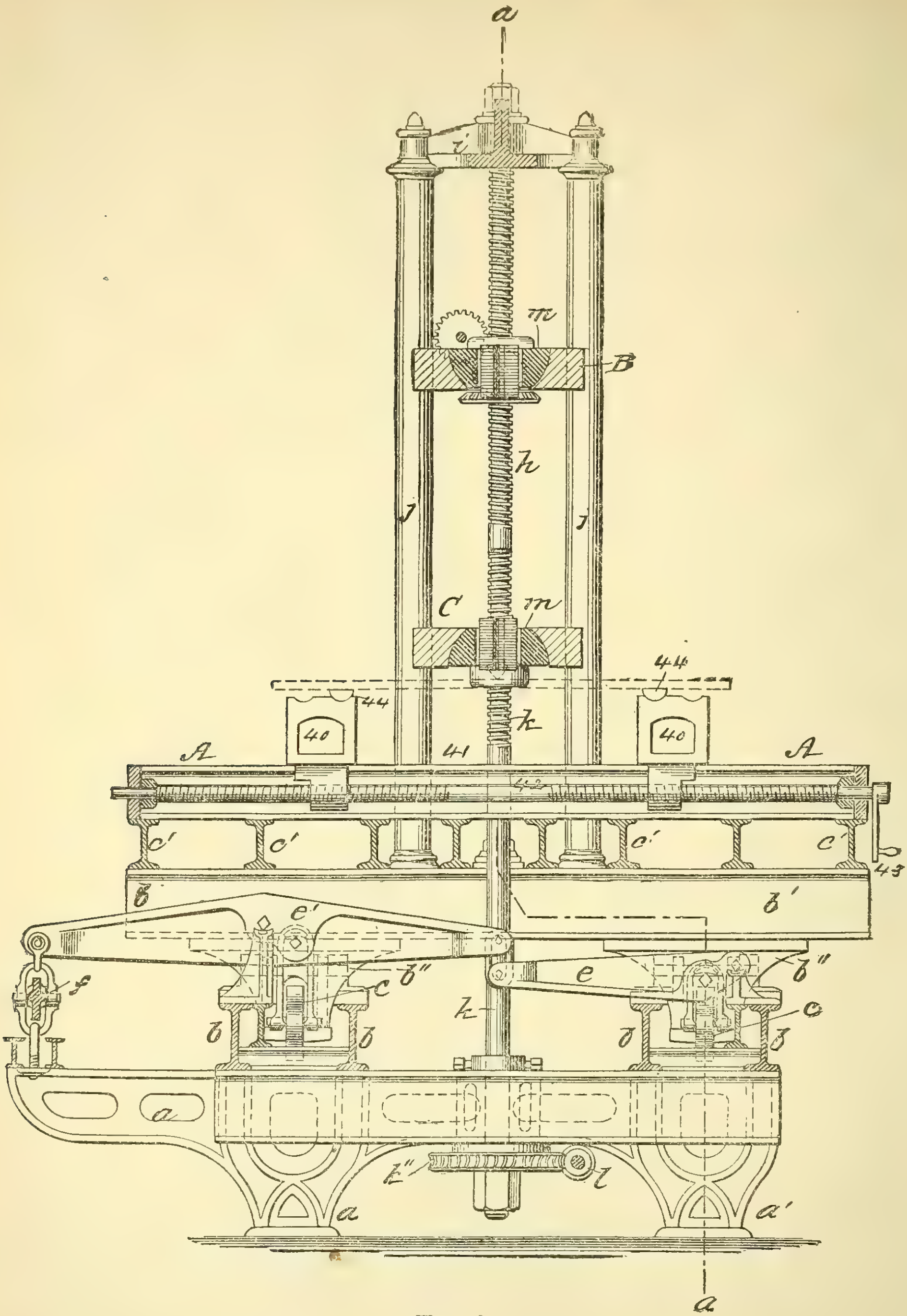


Fig. 9.

levers there stand two castings b^2. These support frame-work of I-beams b^2 that forms the platform of the testing machine. On these I-beams stand a second set of beams, c', and also the columns j and j that support the castings from which hang the adjusting screws H. The object of these secondary screws is

to form a means of rapidly and conveniently adjusting the machine to all the lengths of specimens. For example, the piece in the machine may be 8 or 10 in. in length. At the completion of the test on this piece a bar 5 or 8 ft. will be the next in order. To make the main screws long enough to accommodate such a variety of length of specimens, would require them to be made too heavy, in order to insure sufficient stiffness for preventing lateral vibration. By means of the crank and bevel gears on the upper cross-head, this part of the machine can be rapidly raised and lowered so as to make the different adjustments with great speed. As the upper cross-head hangs on the screws H, they are always subjected to tension, and the compressive reaction comes on the platform by means of the columns *j* and *j*.

In this machine, while strict attention is paid to the relative centering of the cross-heads, there is a device to make the testing machine automatically self centering, so that any possible error in the setting of the specimen or in the alignment of the machine may be corrected by the apparatus itself. It will be noted that the cross heads *b* and *c* are extremely large and are provided in the center with a spherical socket. In this socket there stands a segment of a sphere, carrying the wedges for gripping the test piece.

Let it be supposed that the specimen is placed in the machine eccentrically. As soon as any application of stress occurs the piece swings, and moving the spherical segments in their sockets, adjusts them so as to render the axis of stress coincident with the axis of the piece. The spherical sockets are lined with an anti-friction metal, and the spherical segments are of polished steel constantly lubricated. As a result, the coefficient of friction falls as low as about 5 per cent., and the weight of the segments being less than 200 lbs., the total cross stress that could come on any piece previous to the movement of the segment is about 10 lbs. Inasmuch as the specimens to be tested in these machines will always rise nearly, or up to, 100,000 lbs., this possible cross strain of 10 lbs. can be regarded as an error not large enough to take into account. The facility with which tests in other stresses may be made can be readily understood from the examples in compressive and transverse stress. The two anvil blocks 40 slide on the screws 42, and then the I-beams A may be readily and quickly run in and out by means of the crank 43, so as to adapt the testing machine to any length of span either equal or unequal from the center of the cross-heads. On the top of the anvil blocks the piece for transverse examination is placed, resting on the jaws 44, which are semi-cylindrical pieces of hardened tool steel. The object of these jaws is to facilitate the deflection of the test piece under stress. Right under the cross-head *c* a triangular nose iron is placed, which, by the action of the cross-head, is depressed and brought on the top of the transverse specimen. As fast as the piece deflects the jaws 44 swing in their sockets and adjust themselves to the constantly increasing depression of the piece, swinging around their center and so preserving a uniform length of span, while a broad bearing is constantly maintained under the test piece, and any tendency to cut at its edges is thereby avoided. Simply removing the transverse specimen, a compression test may be made by placing under the cross-head *c* a heavy iron plate for the reception of the specimen, and continuing the downward motion of the cross-head. It will thus be seen that the changes from long to short specimens, and from one kind of stress to another may be readily and easily made in this form of testing machines, requiring only manipulation enough simply to introduce the piece to be tested in its appropriate place, while the machine itself is constantly ready for any of the different forms of stress. The same may be said of the appliances for shearing, bulging, punching, torsion and the like.

The platform of the machine, as has already been stated, has considerable size, and when the machine is built each lever is sealed separately, that is, the lever is hung up from its fulcrum, and standard weights are placed on the pivots, and the distance between the edges slightly varied until these weights exactly balance each other. After the machine is erected in place, standard weights are piled on the platform, and every graduation on the beam is adjusted

to the weights previously placed on the platform. It is obvious that the accuracy of the machine is not dependent on the calculation of lever ratios, or on any alteration in the parts of the testing machine, but inasmuch as the platform gives sufficient size on which to place any desired number of standard weights, the machine can at any time easily and inexpensively be proved to be either right or wrong; in fact the weighing of onesself on the testing machine by simply stepping on the platform is a simple, accurate, and very expeditious mode of making an examination of this kind. Regarding the sensitiveness and the accuracy of the lever system of testing machines, the following experiments may be quoted as derived from actual practice:

A machine, similar to the one illustrated in Fig. 8, has been constantly in use in the "Department of Tests and Experiments" for the past two years. The following experiment was made within a month: When the machine was entirely unloaded, weight of $\frac{3}{4}$ lb. of the platform caused the beam to rise promptly. A test piece was then introduced into the machine, and 100,000 lbs. placed on the platform, when a weight of 6 lbs. caused the beam to move equally with the original $\frac{3}{4}$ lb. Inasmuch as this machine has been subjected to the wear and tear of all sorts of tests, it may be considered that a weight of 6 or 7 lbs. is probably its maximum error, even under the most severe loads. The least reading on the poise of the beam in question is 10 lbs. so that its sensitiveness and accuracy are within about 60 per cent. of the least reading on the beam; consequently if all coefficients of friction and inaccuracies of mechanical construction are eliminated within half the smallest beam reading the machine may be considered practically correct. Again, with regard to what extent it is desirable to carry the accuracy and sensitiveness of a testing machine, it will be generally conceded that engineers rarely care for anything in full-sized pieces beyond the hundredths place of figures. There are few lathes in the country that may be relied upon to turn a test piece perfectly round. There are fewer men who, with a tool capable of such extreme accuracy, have the skill of manipulation sufficient to produce a perfectly round piece, and there are still fewer gauges whose delicacy and accuracy are sufficient to measure a test piece of an inch or more in diameter to an error less than a thousandth of a square inch. Now, if it is impracticable to measure the area of the test piece less than one thousandth of a square inch, which in iron corresponds to a variation in the testing machine of 50 lbs., and in steel to a variation of 80 or 90 lbs., and if the testing machine reads to $\frac{1}{8}$ or $\frac{1}{10}$ of the possible error of measurement in the test piece, is it not more desirable to direct endeavors to a more accurate gauging, rather than to a refinement of the weighing capacity and sensitiveness of the testing machine? Of course, in making experiments on turned or machined test pieces, accuracy of measurement and of weighing are both desirable, but the knowledge to be derived from such test pieces is simply the knowledge of the character of the material of which the piece consists. While the quality of the materials is a very important item, the consideration in the design of a structure the actual strength of the various members employed is, to the practical engineer and architect, of far greater importance. To cut a piece out of the center of an I-beam and to break it in a testing machine may give the investigator a knowledge of the material of which a beam is composed, but when it is wished to know what load may be safely put on that beam when placed on the floor of a building, it is necessary to know the actual strength of the beam as a whole, and not the theoretical quantity obtained by calculation from a test piece, broken under circumstances entirely different from those which occur in actual use of the beam. Consequently the information desired at the present day is of the actual strength of members employed in construction, just as they are used, rather than that of carefully prepared test pieces which are broken under especial circumstances.

The difficulty of measuring a test piece increases about as the square of its size. If the piece be carefully machined, the errors of measurement may be estimated to be from $\frac{1}{1000}$ to $\frac{5}{1000}$ of a sq. in. per inch of area, while if bars direct from the mill are to be experimented on, the errors induced by variations in rolling, by flaws, seams and inequalities, by oxida

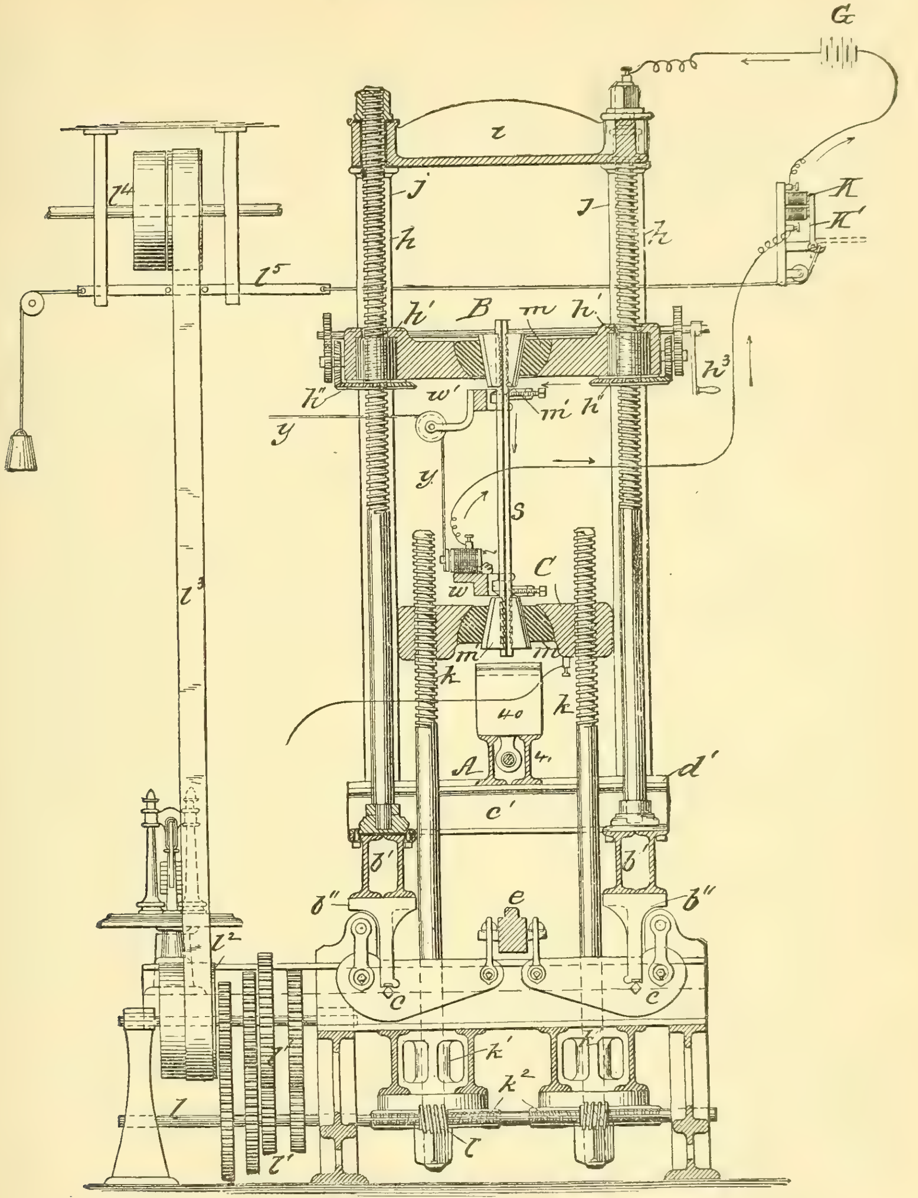


FIG. 10.

tion and scale on the piece, may arise to an extent so large, that in I-bars of from 4 to 8 square inches in area there may be an error of $\frac{5}{100}$ or $\frac{8}{100}$ of a square inch. Thus, a possible error in the measurement of the test piece corresponding to from 2,500 to 4,000 lbs. may be introduced; consequently, if the testing machine is reliable to 25, 50, or even 100 lbs., and the error in the measurement of the test piece may amount to 10 or 20 times that quantity, the refinement of the testing machines seems to be hardly so much desired as refinement of measuring facilities.

One of the largest items of expense in making tests has been the necessity of carefully preparing each test piece to adapt it to the jaws of the testing machine. In many machines

it has been necessary to select a piece considerably larger than was desired, and turn or plane it in the center so as to secure it to the testing machine. In fact, some specimens not long ago tested on one of the Rodman machines in Washington, the author was informed that some small test pieces of steel about 2 in. long and half an inch in diameter cost $10 each, simply to prepare them to be placed in the testing machine. This large item of expense has a very depressing effect upon the tendency to make experiments on the strength of material. The machine now under consideration has been specially planned to take in sections of any shape whatsoever in original size, so that those desiring to make experiments on real shapes, such as I-beams, channels, stars, and even railway rails, can simply cut a piece off the bar in question, send it to the testing machine, have it broken without the necessity of any machine work.

The spherical sockets previously alluded to for suspending the gripping wedges have four sides. These sides are inclined to the axis of the machine at an angle of about 12 degrees—two of them being curved and two of them being straight. By means of a series of wedges having curves and straight backs, pieces of any section whatsoever may be placed in the testing machine and completely surrounded by the gripping wedges, so that the stress of the testing machine is firmly and equitably distributed over the entire test piece. Of course this applies simply to tests made in tension, while it will be perceived that transverse and other tests may be accomplished by similar contrivances.

Referring to Fig. 10 (the cross-section of the machine), it will be seen that the battery G is attached to the top of the adjusting screws HH. These screws are carefully insulated from the rest of the machine by standing on rubber bases and passing through rubber bushings held in the interior of the top casting, consequently these screws, with their corresponding cross-head, are electrically insulated from the rest of the testing machine, and being joined to one pole of the battery, form the only means by which the current can flow into the machine itself. As soon as the test piece is connected with the top cross-head it becomes thereby connected with the battery. On the lower end of the specimen may be seen a small clamp carrying an electro-magnet. One end of the wire of this magnet is in connection with the specimen, while the other end of the wire is joined to a little binding screw on top of the magnet, to which the other pole of the battery is attached, so that the current actuating this magnet flows through the test piece under examination. A magnetic clutch M, for holding the driving belt on the tight pulley, is also included in this part of the battery circuit. As long as the specimen remains intact, the current flowing from the battery, excites the two magnets and attracts their armatures. When the rupture of the test piece occurs, the current is at the same instant broken, the magnets are demagnetized, the magnetic clutch M is released, the belt slides, by means of a counterpoise weight, to the loose pulley, and the testing machine stops. On the top of the specimen nearest to the upper cross head is attached a second clamp, carrying a small sheave or pulley. Around this pulley, parallel to the specimen and attached to the armature of the lower clamp magnet, passes a flexible steel tape y. Referring to Fig. 11 an enlarged view of the specimen and clamp with its magnet may be seen. Here it will be noticed that the tape, after passing alongside the specimen, runs down to a pencil, or stylographic pen that is held in a carriage, placed over a metal cylinder carrying a sheet of cross-section paper. It is at once obvious that as fast as the specimen elongates under the action of the stress, the pencil is drawn along the cylinder parallel to its axis. This axis (the axis of x in analytical geometry) is assumed to be the axis of elongation. Inasmuch as the cross-section of the tape is very large in comparison with the friction of the pencil carriage and the supporting pulleys, the tape itself is subjected to comparatively little stress, and is always kept tight and in its place by means of the counterpoise weight y'; consequently, every deformation of the specimen is accurately recorded on the cross-section paper by a corresponding motion to and fro of the pencil. In actual practice it may be said that the record of the cross-section paper corresponds within $\frac{1}{100}$ of

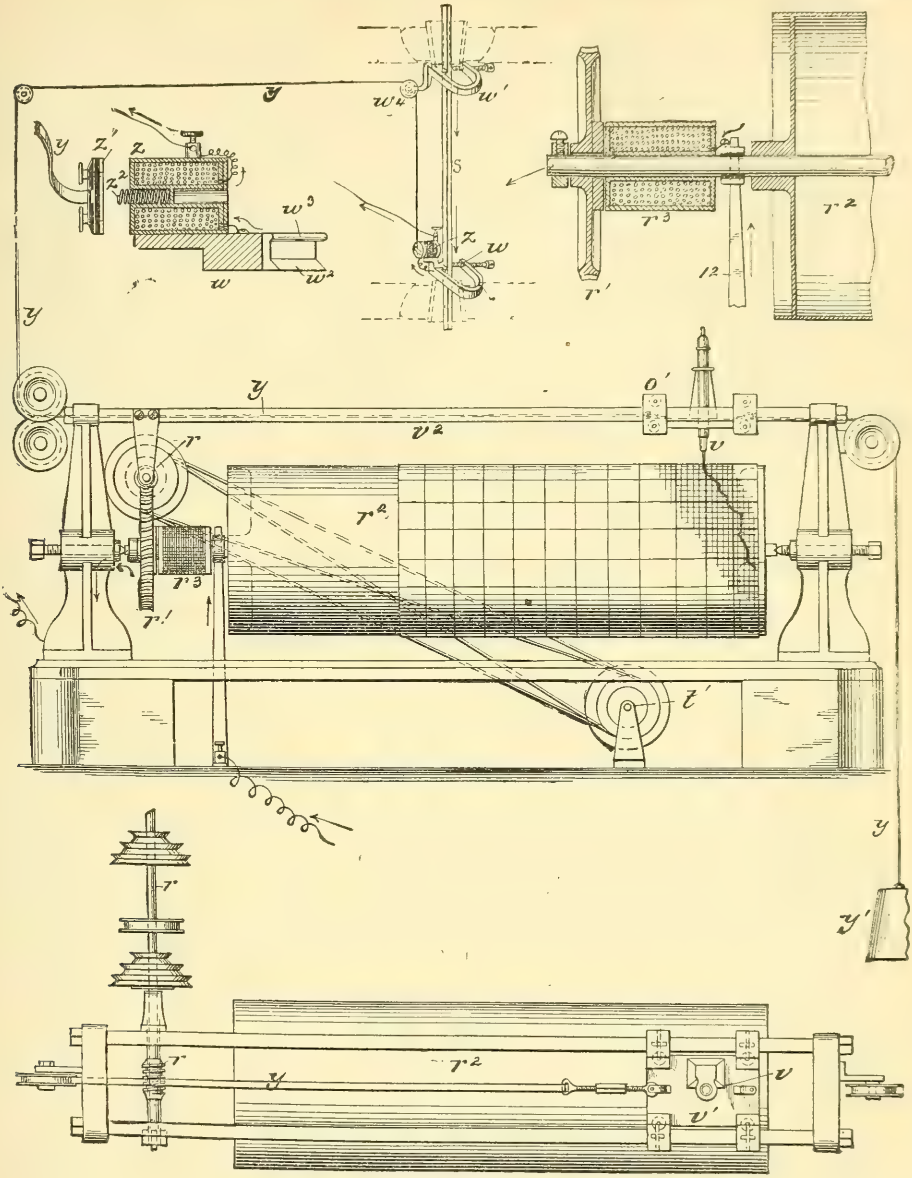


FIG. 11.

an inch to the elongation measured on the specimen, and this for ordinary experimental work is sufficiently near. It will be noticed that the clamp is supplied with a spring and screw *w*. These screws are employed for securing the clamp to the specimen, and the spring serves to take any reduction of area caused by the drawing down on of the piece, and to constantly keep the clamp tightly secured in its place. Nearest to the magnet the side of the clamp is supplied with two edges, one rounded and one sharp. The sharp edge slightly indents itself into the specimen and secures the clamp rigidly into its place, while the round edge forms a zero mark from which the percentage of stretch may be readily calculated. The top clamp is made in a corresponding manner, and the sharp edges are placed next to the jaws of the cross-heads. Consequently it is very

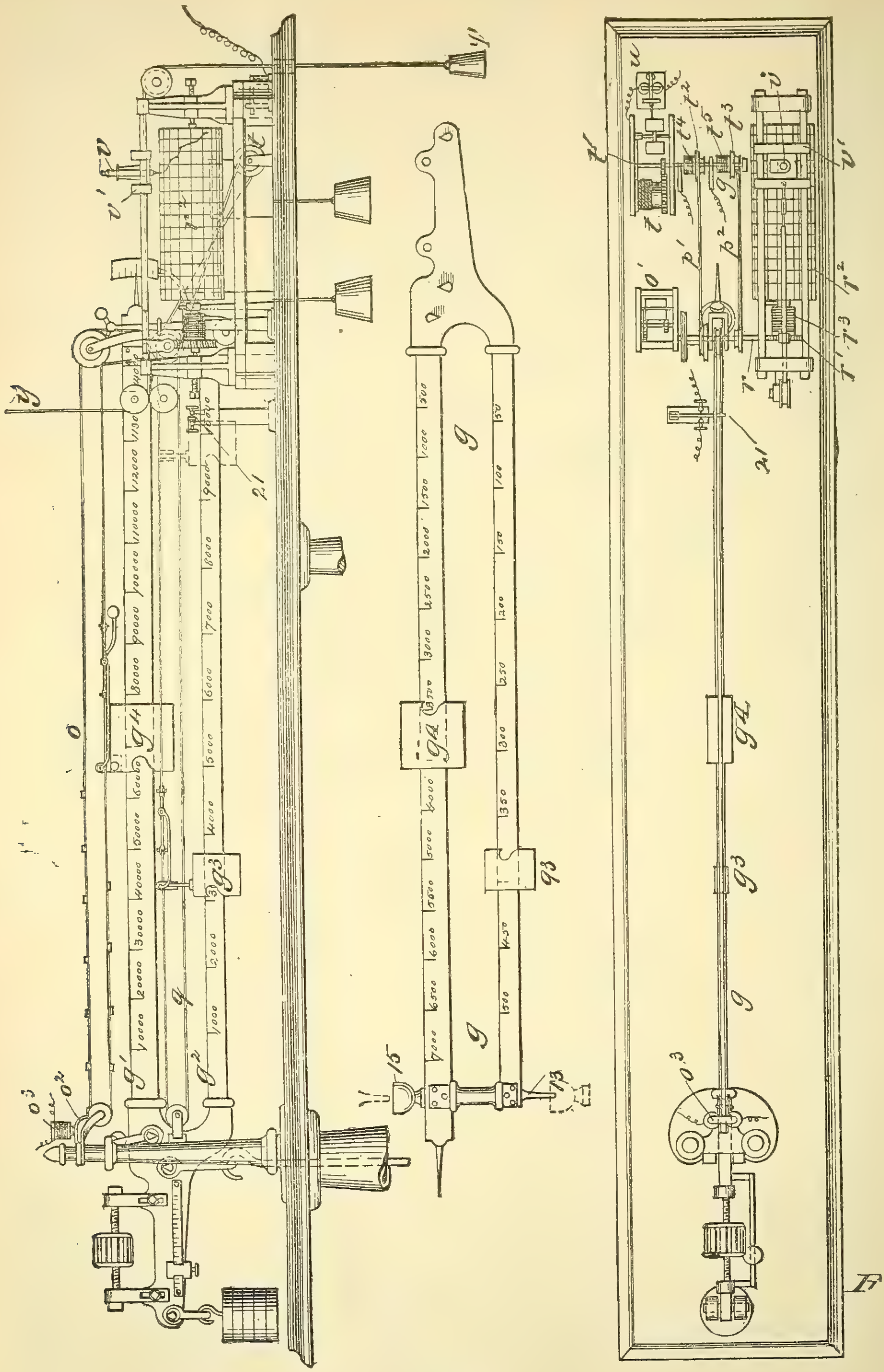


FIG. 12.

rare that a specimen can break outside of these edges, and they then form data marks of reference, from which the deformation of the specimen may be calcu-

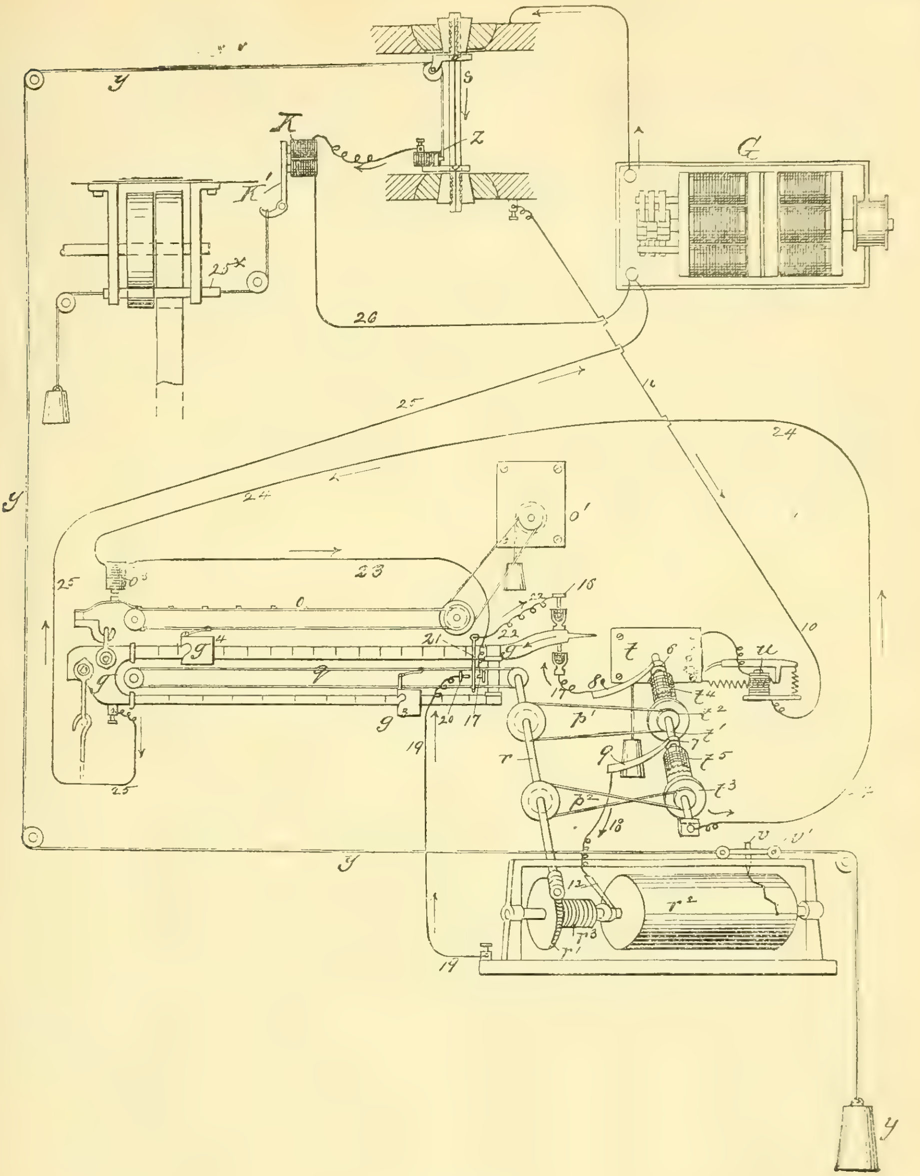


FIG. 13.

lated. The autographic record of the deformation of the specimen is by this means made plain. It now simply becomes necessary to record, at the same time the stress producing the deformation. Turning to Fig. 12 an enlarged view of the beam with the registering cylinder may be obtained. From this illustration it is perceived that the beam is composed of two parts, a top bar and a lower bar, each carrying its appropriate poise. The large poise is ten times as heavy as the small one, consequently the small one must move ten times as far as the large one to produce a corresponding effect on the scale. The entire travel of the small poise is equivalent to a weight of 10,000 in the testing machine, while the entire travel of the large one is equivalent to the entire capacity of 200,000 lbs. On the end of the beam will be seen two mercury cups 16 and 17. The skeleton

view of the beam with its apparatus shown in Fig. 13 may perhaps render this operation a little more obvious. From the lower cross-head of the testing machine, connected with the specimen, the electrical current flows into the butt of the beam. The two mercury cups at the end of the beam are so arranged that when the beam is in the center neither cup is included in the electrical circuit which is consequently broken. If the force on the platform increases, the beam rises and the upper cup is brought into circuit and the current flows. Should the weight in the testing machine decrease, the beam falls into the lower cup, and the electric circuit is also completed by the drop of the beam. The lower poise is connected by means of the steel take with a little countershaft r. This countershaft is joined by an open and closed belt to two magnetic clutches t^4 and t^5. These clutches are placed upon the shaft that is driven by the clockwork t. When the beam rises, the magnetic clutch t^4 is excited by the completion of the circuit through the mercury cup 16. As a consequence the small poise is immediately drawn out along the beam, tending to rebalance it. Should the motion of the poise equal the weight on the platform, the beam then sinks to the center, the circuit is broken, and the poise stands still. If, in any case, the force on the platform decreases, the beam drops into the lower cup, the magnetic clutch t^5 is excited, the cross belt p^2 begins to move, and the poise is moved backwards on the beam, tending again to rebalance it. Returning to Fig. 12 a switch will be seen at 21, so placed that when the small poise reaches the maximum extent of its travel it strikes against this switch and automatically closes the electrical circuit through the magnet o^3. The effect of this circuit is to excite the magnet, release the large poise g^4, and cause it to move out $\frac{1}{10}$ of the travel on the top beam, which is exactly equivalent to the total travel of the small poise on the lower beam. Instantaneously with the motion of the large poise the beam, superweighted, drops, closing the circuit in the lower cup and returns the small poise to the butt of the beam. It is plain that the rise and fall of the beam absolutely controls the motion of the poises, as the beam forms an automatic shunt for so circulating the electric current as to cause the poises to move to and fro. This motion of the beam is entirely dependent on the pressure exerted on the platform, so that a piece being placed in the testing machine the weighing may be done by the machine automatically in a way far more sensitive and accurate than is possible to accomplish by any hand labor however skilled.

In order to accomplish the registration of the motion of the poises which is all that is necessary to record the stress on the specimen, the cylinder previously mentioned is magnetically connected with the shaft r, so that as fast as the poises travel out, a worm-gear connected with the magnet on the cylinder causes the cylinder to revolve circumferentially, thus making the axis of y the axis of stress. The motion of the pencil, as previously has been shown, records the deformation of the specimen, while the motion of the cylinder records the motion of the poises. By so proportioning the pitch of the worm-gear that an inch on the circumference of the cylinder corresponds to a definite number of pounds on the testing machine, it becomes an easy matter to read from the motion of the cylinder the amount of force which has been applied to the specimen, consequently the curvelinear line that is marked on the cross-section paper by the combined motion of the pencil under the influence of the tape and of the cylinder, gives a record whose abscissæ and ordinates are measures respectively of the stress to which the piece is subjected and the resulting strain. As soon as the piece breaks, the current which actuates both the motion of the testing machine, the motion of the tape and the motion of the cylinder is ruptured. The machine stops, the poises stand still and the pencil comes to rest, leaving the record on the cross-section paper for any future inspection.

The preceding method of obtaining autographic diagrams possesses many advantages for adapting it to testing machines that are to be built especially compactly. There is another method of obtaining the same results, whereby the registering cylinder may be located at any distance from the the testing machine. in Figs. 14 and 15 are two photo-engravings from the beam and the registering cylinder employed in the "Department of

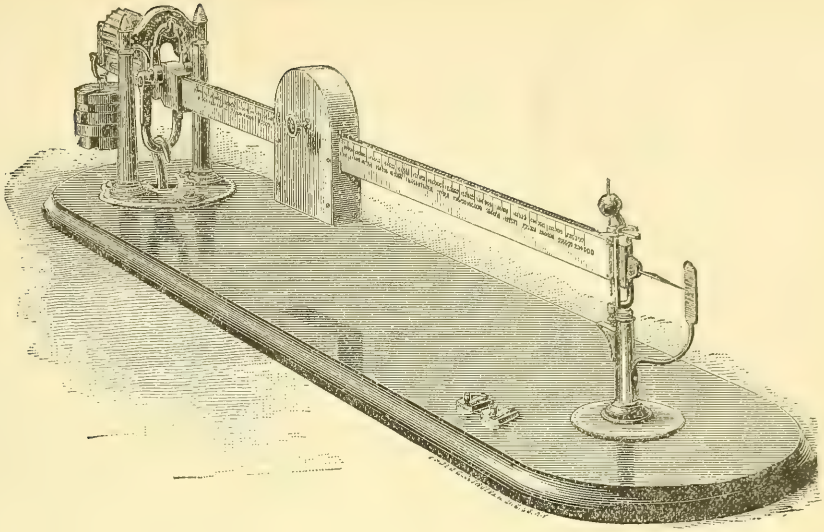


FIG. 14.

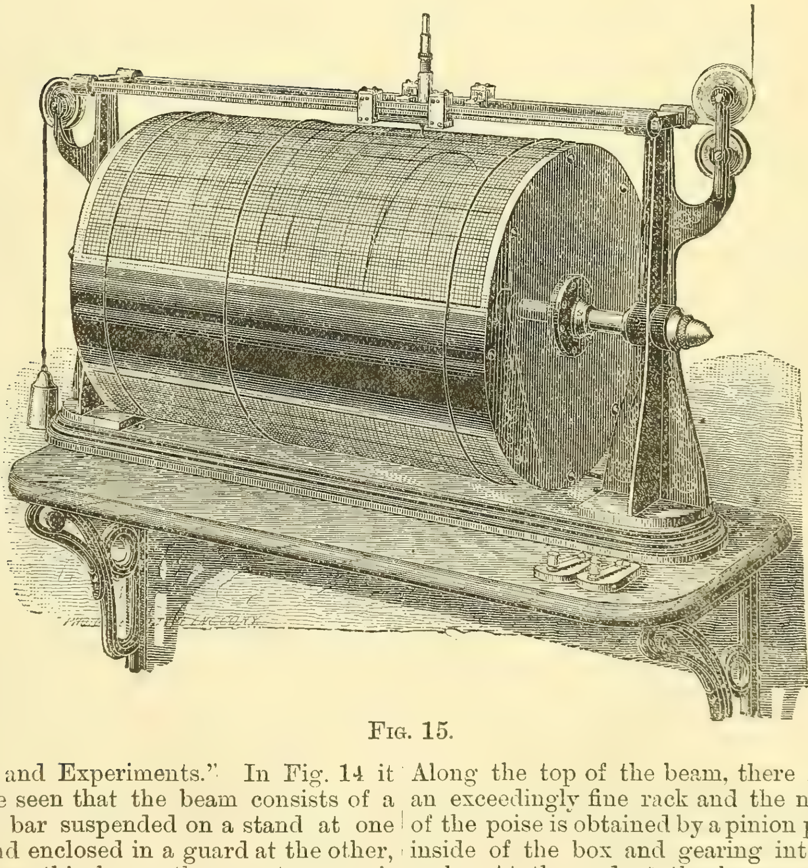
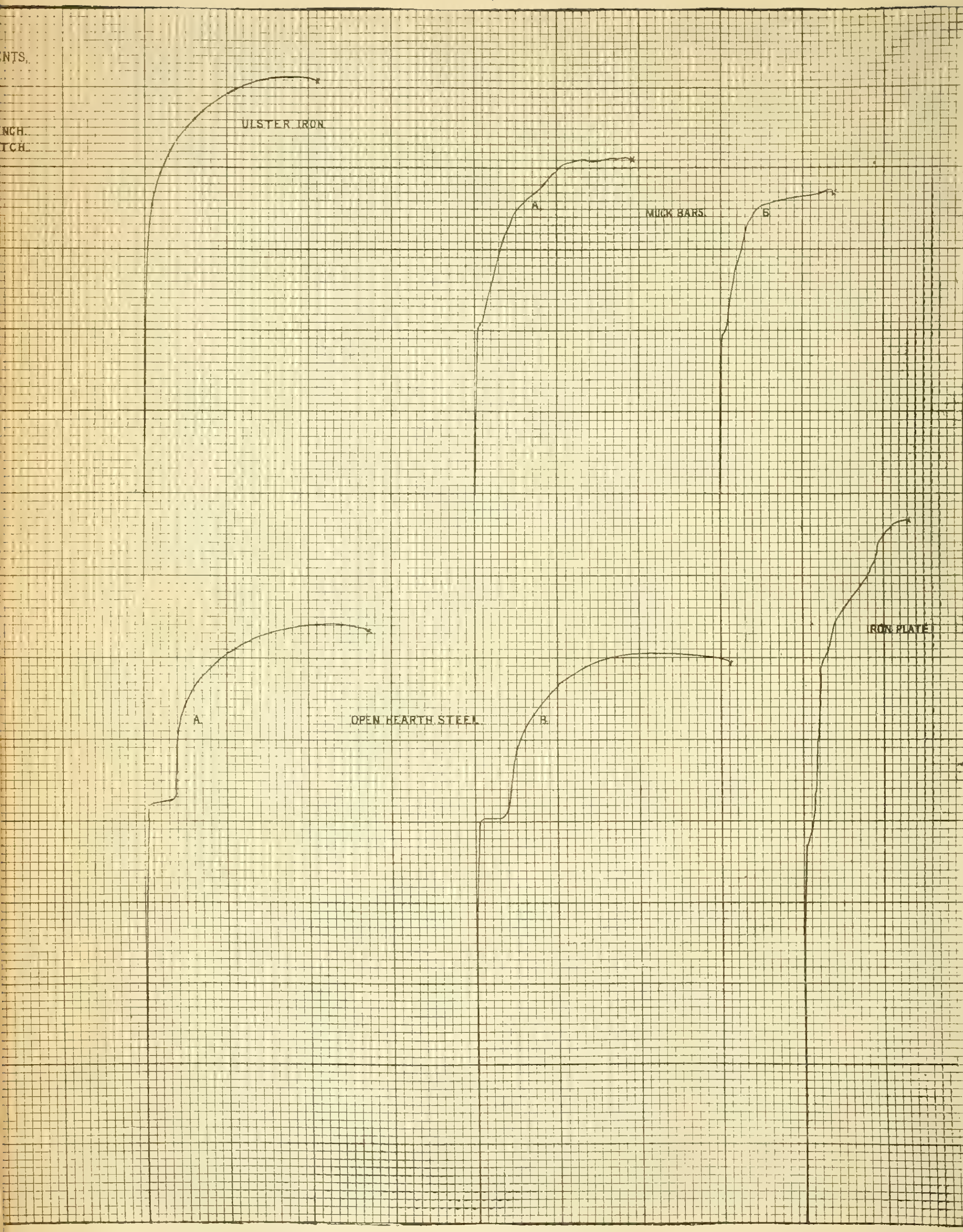


FIG. 15.

Tests and Experiments." In Fig. 14 it will be seen that the beam consists of a single bar suspended on a stand at one end and enclosed in a guard at the other, while on this beam there rests a semi-circular brass box forming the poise. Along the top of the beam, there is cut an exceedingly fine rack and the motion of the poise is obtained by a pinion placed inside of the box and gearing into this rack. At the end of the beam may be seen the mercury cups alluded to in the

former method for making the electrical connection as the beam rises and falls. The operation of this piece of aparatus is substantially as follows: The clock-work motor for driving the poises to and fro on the beam, is connected with the mercury cup by means of some brass strips placed in the rear of the steel bar forming the beam. These strips are connected with two electro magnets on the inside of the poise, consequently when the beam either rises or falls, one or the other of the magnets is excited, the corresponding train of clock-work thrown into action and the poise gradually rolls to and fro until a balance is re-established. This part of the apparatus, that is the accomplishment of the motion of the poise to and fro on the beam, is exceedingly simple. The knotty part of the problem being to correlate the motion of the poise with the motion of the cylinder exactly so that in a given travel of the poise along the beam, the cylinder may move a corresponding quantity. Of course the ratio between the two movements is simply a matter of proportioning so as to accommodate the ordinary cross-section sheet to the circumference of the cylinder, but an exact and constant ratio were very important. To solve this problem was to accomplish the solution of one similar to the autographic telegraph or electric clock and similar pieces of mechanism, but with some peculiar features arising only in this special instance. To go a little more into detail of the poise, there are enclosed in a brass box two large wheels about 8 in. in diameter. The wheel placed nearest the front of the poise is graduated with a series of numbers, corresponding to similar weights in the testing machine. The pinion carrying the poise along the beam is an inch in circumference, and consequently a single revolution of the pinion carries the poise one inch along the top of the beam. The dial wheel is secured directly to the pinion shaft so that there can be no backlash between the two, and being 8 in. in diameter one revolution of the pinion causes the dial wheel to travel 25 in. in circumference. In the testing machine in question, a motion of an inch along the beam corresponds to a weight of 4,000 lbs. in the machine; and the dial wheel being graduated into 400 parts, each graduation corresponds to a weight of 10 lbs. in the machine. The other wheel is constructed in precisely the same manner as the dial wheel, excepting that in the place of the graduated marks there are little strips of india rubber so that the wheel presents a series of teeth, alternately made of india rubber and of brass. On the circumference of this wheel, there presses a brass commutator strip so arranged as to include the cylinder in its electrical circuit. When the poises commence to move along the beam, the teeth, alternately of metal and rubber, move under the commutator strip and with every passage of a brass tooth under the strip, an impulse of electric force is transmitted into the cylinder. In Fig. 15 a detailed view may be seen taken from the working drawing the cylinder. Inside of the cylinder are two toothed wheels, which are mounted on a central shaft and are capable of being ratcheted round by means of the little lever arm and pawl, operated by a magnet placed directly under each of the wheels. One of these wheels is intended to drive the cylinder in one direction and the other in a contrary one. One electro-magnet is connected with a mercury cup on the bottom of the beam and the other with a mercury cup on the top. Consequently, when the beam makes connection with either cup and the beam commences to move, the corresponding electro-magnet in the cylinder is excited, the armature is attracted and commences to ratchet the cylinder wheel around. By means of a series of gears placed at the extreme right-hand of the cylinder, the drum is moved one way or the other in such a proportion that the motion of the poise may be accurately correlated with the motion of the cylinder. If the gears in the cylinder are accurately cut and properly proportioned, and if the armature of each magnet makes one stroke for every tooth on the poise wheel, the cylinder will be moved to an amount that is exactly commensurate with the poise itself. At first it may perhaps seem that so accurate an arrangement would be almost impossible to construct, yet the same obstacle is presented in all machines of precision, but nobody objects to an engineer's transit or to astronomical telescope in consequence of difficulties in construction.

In machines already built it has been found perfectly possible to so correlate



NTS.
NCH.
TCH.
ULSTER IRON.
A.
MUCK BARS.
B.
IRON PLATE.
A.
OPEN HEARTH STEEL.
B.

Fig. 17.

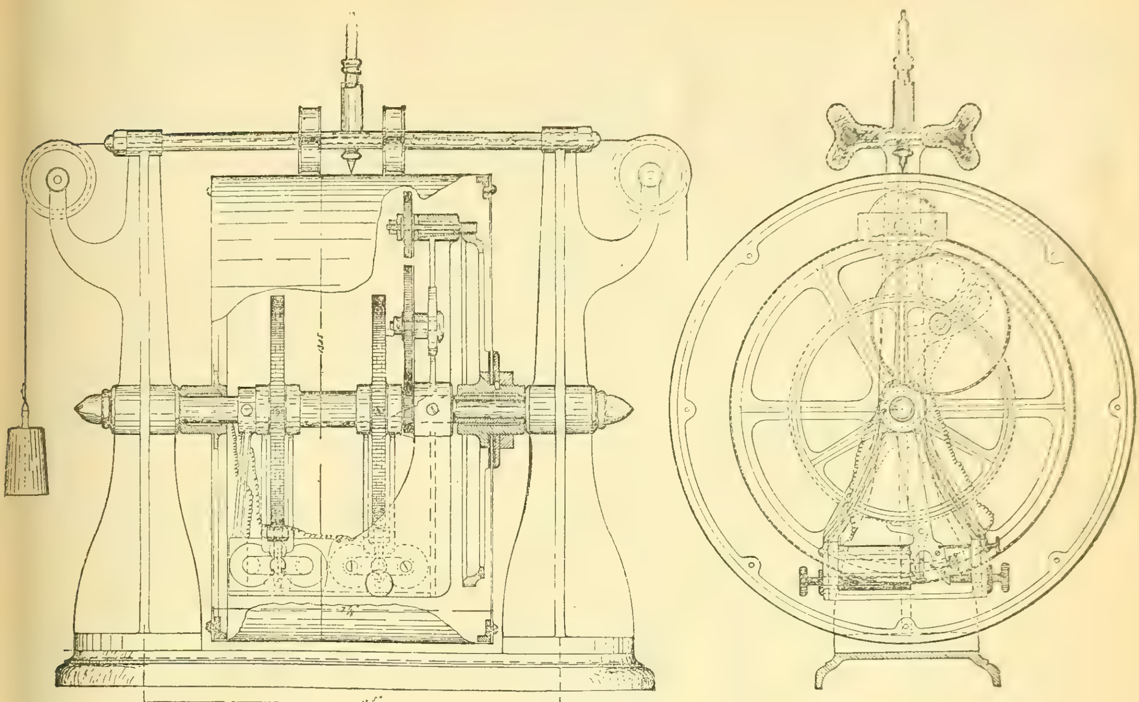


FIG. 16.

the motion of the poise with the motion of the cylinder that in the entire travel of each one, there should be a discrepancy between the two of less than 1-100 of an inch.

If the strip of cross-section paper on the cylinder be 40 in. long giving a stress of 4,000 lbs. to the linear inch, 1-100 of an inch corresponds to 40 lbs. This is about as fine a reading as can practically be made on cross-section paper, and as it is 4 times as large as the smallest reading of the poise it is obvious that this error of 1-100 of an inch on the entire travel of the cylinder is too small to be of any practical value.

Over this cylinder is a track carrying a little carriage and pencil connected with the specimen by means of a steel tape, in a manner similar to that previously described. The cylinder and the poise are only connected, in a manner similar to the connection existing between two telegraph stations, consequently, if desired, the cylinder may be placed in one locality and the testing machine in another, so that it is perfectly possible for the cylinder to be entirely exterior to the control of the operator manipulating the testing machine.

Fig. 17 gives a photo-engraving taken from a sheet of cross-section paper bearing a half dozen of the curves autographically drawn by the testing machine. In this illustration the cross-section paper has been ruled in feet and decimals of a foot, and each of the specimens were one foot between the stretch clamps; consequently the percentage of stretch of the specimen may be at once read from the cross-section paper. The lower half of the sheet contains three steel curves which it will be readily seen, bear to each other a strong family resemblance. The curve commences with a straight line slightly inclined to the axis of stress at a constant tangent of one whose equation may be described as $y=ax$. When the elastic limit is reached a sudden point of inflection occurs, the tangent swinging around and becoming nearly or quite parallel to the axis of strain. Very soon, however, a second point of inflection makes its appearance, and the tangent nearly returns to its former inclination and the curve takes on a general parabolic form. These steel curves as well as the curve given by the specimen of Ulster iron may be taken as typical forms of curves to be obtained from a material which is nearly or quite

homogeneous. The lines are quite true and regular, and the curve manifests little indications of non-uniformity in the material. As soon as the maximum stress in the specimen is reached, the material begins to reduce more rapidly at some point or other of its length than the testing machine can keep pace with. Consequently, the stress on the specimen decreases gradually, while at the same time the strain rapidly increases. As a consequence of this, a third point of inflection occurs, the tangent inclining toward the axis of strain, and changing from a positive to a negative value. This proceeds until the specimen is ruptured and the apparatus comes to a stand still.

The other three curves given here were made by a piece of boiler plate and two specimens of muck-bar. These are exceedingly good illustrations of the value of the autographic method. It is well known that both boiler plate and muck-bar are decidely non-homogeneous, and as a result here the curves are exceedingly irregular, especially after passing the elastic limit. While a general resemblance may be traced to the preceding ones, they are full of points of inflection, and would seem to indicate that the material, being made up of fibres which were alternately elastic and brittle, was ruptured by the continuing action of the stress in the testing machine, breaking one fibre after another, in very much the same way that a rope or cable is parted. To give a general record of a test it is believed that the autographic method is without parallel. As here described, however, it is not sufficiently accurate for careful investigation, into the limit and the modulus of elasticity. For instance, it is impossible to read on the cross-section paper nearer than about 100th of an inch, and as any arrangement to magnify the stretch up to the elastic limit would so increase the elongation after passing that point, as to extend the curve beyond the capacity of the cross-section paper that is of an ordinary sheet. The autographic diagram will give the elastic limit within 1000 lbs. per square inch of cross-section and the modulus of elasticity within 100,000 lbs. Yet for many purposes it is desired to obtain a more accurate approximation to these quantities, and in Fig. 18 may be seen an illustration of a piece of apparatus devised by Col. William H. Paine, of the East River Bridge, and used by him there in the investigations of the steel to be used in the trusses. It will be seen that the apparatus consists of two steel bars *a* and *b*, so arranged as to slide parallel to each other. At the ends each bar is supplied with a knife edge, *f* held in the sliding piece of brass *m*, which may be adjusted at any end of the bar. By means of these knife edges the whole apparatus may be clamped to the specimen by the use of the springs *g*. On the bar *a*, a small steel projection *o*, bears against one of the knife edges of the multiplying lever *e*, and the other end of this lever comes in contact with a supplementary vernier and scale *d*, *e*. Now as the piece under examination elongates, the bars move by each other, the multiplying lever *c* turns on its fulcrum, pushing the vernier *d* outwards along its scale. Thus it is obvious that the whole apparatus after being placed on the specimen works entirely without any intervention from the operator..

In the case of micrometer screws it becomes a pretty difficult matter for the same person to make readings twice alike, on account of the varying coefficient of friction and of the varying personal equation. And again the operation of making a reading with micrometer screws is exceedingly slow, and should, as in the hydraulic machines, the pressure have any tendency to relax, it is almost impossible to obtain a correct reading, whereas in the gauge in question the reading may be made as fast as the eye can gauge the coincidence between the vernier and the scale. In fact, in making many thousands of tests in the East River Bridge, at the Cambria Iron Works, the author used this instrument, and employing one man to place weights upon the beam of the testing machine, was accustomed to sit where he could see the vernier of the stretch gauge, and the moment that the beam raised to catch the reading on the scale, so that the entire operation of making a test on a bar one foot long, and an inch square, included the taking of some 20 to 30 readings, did not occupy a time of more than 8 to 10 minutes, which is a speed never to be obtained by any arrangement of micrometer screws. By means of the extension piece and clamp *l* and *m*, the sliding bars of this apparatus may be ex-

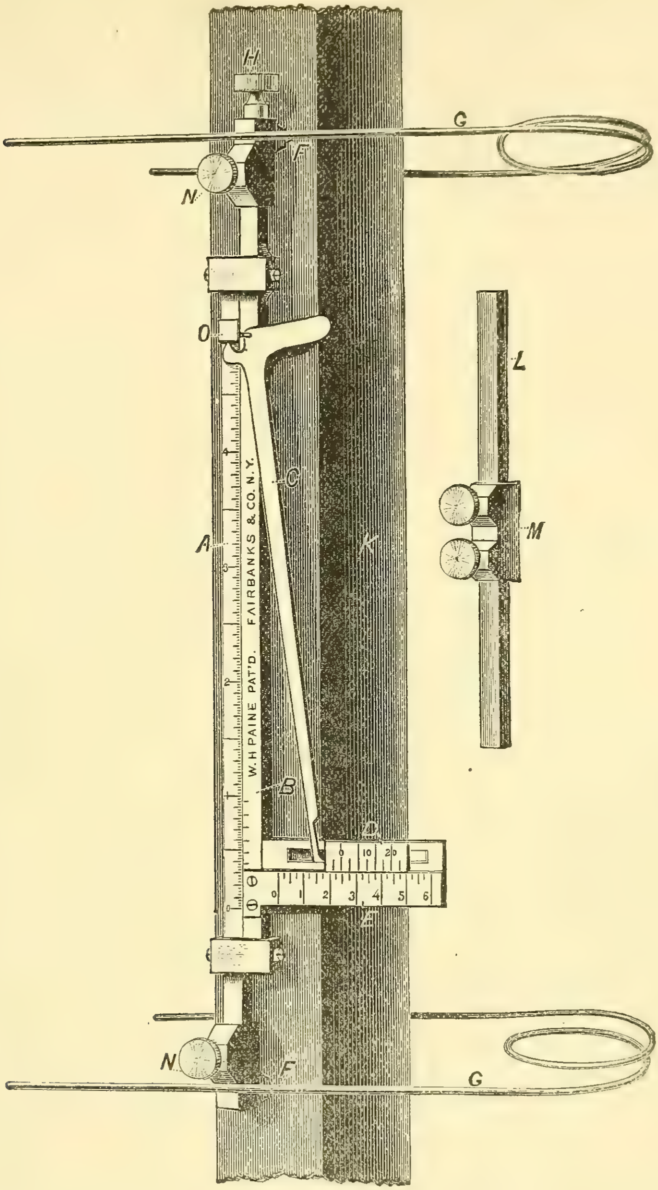


FIG. 18.

tended to any entire length, and by this means it becomes an exceedingly valuable instrument in the investigations of stress which occur in structures already existing, such as bridges, roofs and the like. As a case in point may be mentioned an examination made by the author at the opening of the new cantilever bridge at Niagara. Two gauges were employed at the test of the cantilever bridge, vernier *a* being screwed to a tie-bar of the truss extending over one panel, and vernier *b* attached to the bar extending over two panels from the anchorage end of the cantilever on the American side. Vernier *a* extended over a distance of 4.75 feet, and vernier *b* over a distance of 5 feet of the bars. Readings taken at the various points at which the loading trains came to a stand, and gave the extensions

produced by a quiescent load, during the movement of the trains, the verniers fluttered a little, showing even at slight speed that there is a sensible effect from the shock, and as a loaded train moved off on the American side there was a negative reading of about $\frac{25}{1000000}$ of a foot, showing a slight compression in the tie-bar. The following are the readings of the verniers reduced to a length of one foot of bar:

Train at Canadian abutment,
vernier a $\frac{10}{1000000}$.
vernier b $\frac{12}{1000000}$
Train at Canadian tower,
vernier a $\frac{74}{1000000}$
vernier b $\frac{72}{1000000}$
Train at Canadian end of center span,
vernier a $\frac{106}{1000000}$
vernier b $\frac{132}{1000000}$
Train at American abutment, giving the maximum stress in the rods under examination, . . vernier a $\frac{65}{1000000}$
vernier b $\frac{212}{1000000}$
Center span entirely loaded,
vernier a $\frac{106}{1000000}$
vernier b $\frac{102}{1000000}$

Bridge unloaded, verniers returned to zero. Assuming the modulus of elasticity on the eye-bars at 32,000,000 lbs., the following stresses per square inch are obtained from the preceding measurements:

Bar extending from two panels, 6780 pounds per square inch.

Bar extending from one panel, 5280 pounds per square inch.

Thus far vertical testing machines have occupied our attention. With the exception of the Emery testing machine, there are few machines of this class whose points of excellence demand attention.

During the construction of the St. Louis bridge. Capt. Eads built a couple of large horizontal machines, having a capacity of 500,000 lbs. While these machines, however, were superior to anything of the kind at that time, they were not sufficiently accurate to give results which were received with great confidence. They simply consisted of a long heavy framework, carrying a cross head at the one end, with a hydraulic jack, and a pressure gauge attached for straining the specimen.. Consequently the only method of estimating the amount of stress applied was by calculating the pressure per square inch on the ram. As was conclusively shown in experiments with the Watertown machine, the coefficients of friction in the jack are constantly varying, sometimes introducing errors as high as 40 per cent. of the actual stress. Several long horizontal testing machines have been built by the Riehle Bros. for the purpose of making tests of chains, &c. These, however, were of small capacity, rarely rising over two or three hundred thousand pounds. One of the oldest horizontal machines of large capacity is that used in the Washington Navy Yard for investigations on chain cables, and with which all of the preliminary work of the U. S. Board to test iron and steel was made. This machine consists of along horizontal framework of masonry, carrying a hydraulic jack on one end with a scale at the other composed of two levers. On the extremity of the second lever there hangs a large platform upon which a series of weights may be piled as fast as the stress upon the specimen increases, due to the action of the hydraulic jack. Of course this machine is laborious and cumbersome to manipulate, on account of the large number of heavy weights that have to be actually lifted and piled on the platform of the scale.

In Fig. 19 may be seen an illustration of a large horizontal machine which has just been constructed by Messrs. Fairbanks & Co. for the Edgemoor Iron Co. This machine consists of a long horizontal framework of wrought iron channels, upon which travels a hydraulic jack, carrying a three-piston pump, and driven by a belt supported on two idlers at the end of the frame. To accommodate the different lengths of specimens the jack can be run to and fro along the frame at pleasure, and be secured by means of 4 heavy steel pins which pass directly through each of the four columns composing the frame, between the parts of which run steel legs connected rigidly to the jack. A system of valves on the jack provides for the admission of fluid on both sides of the piston, so that the jack may be used either in tension or compression at the pleasure of the operator. At the front end of the testing machine there stands a large bed plate of cast iron, to which the columns are attached, and which also supports the weighing

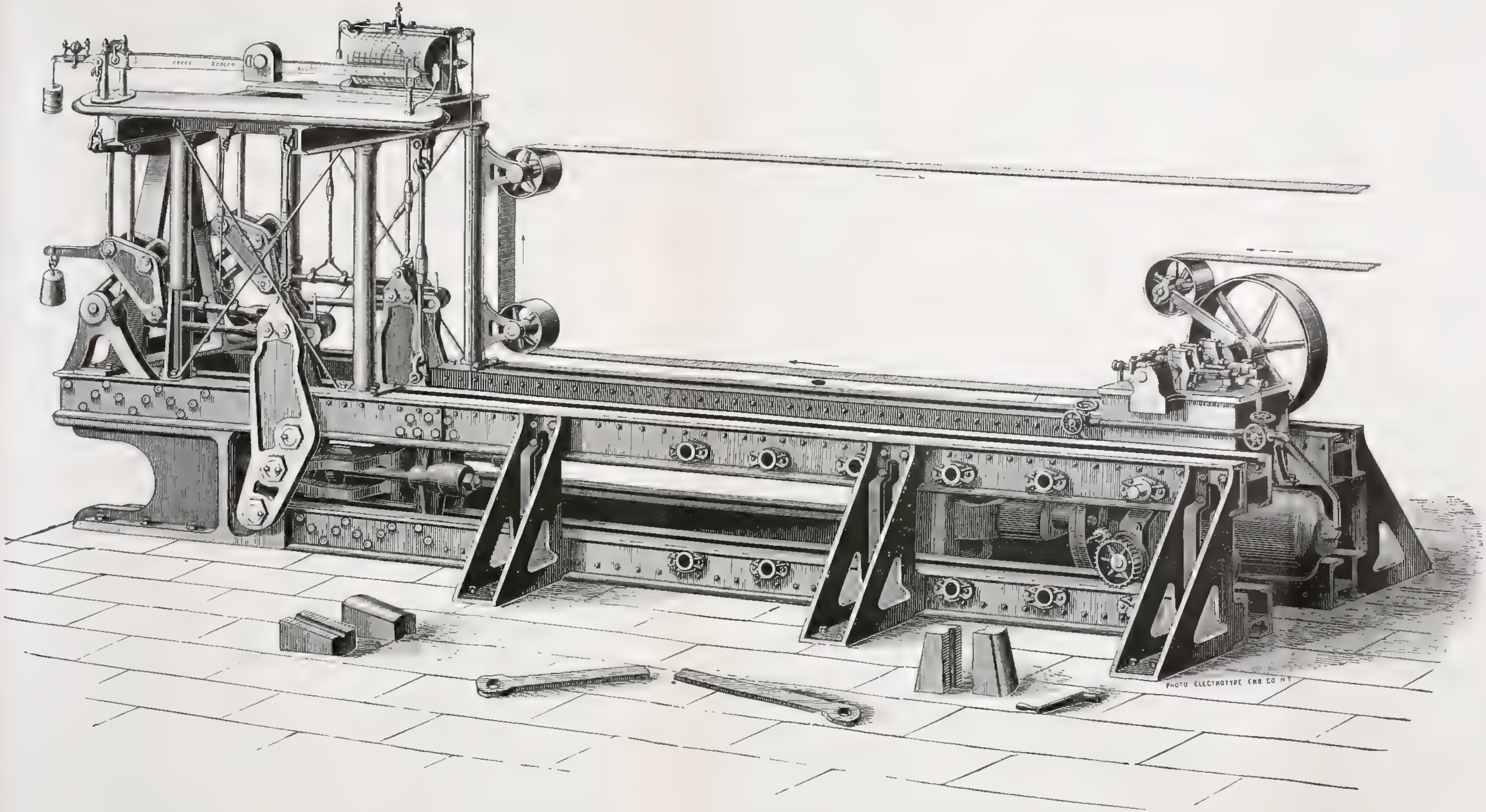


FIG 19.

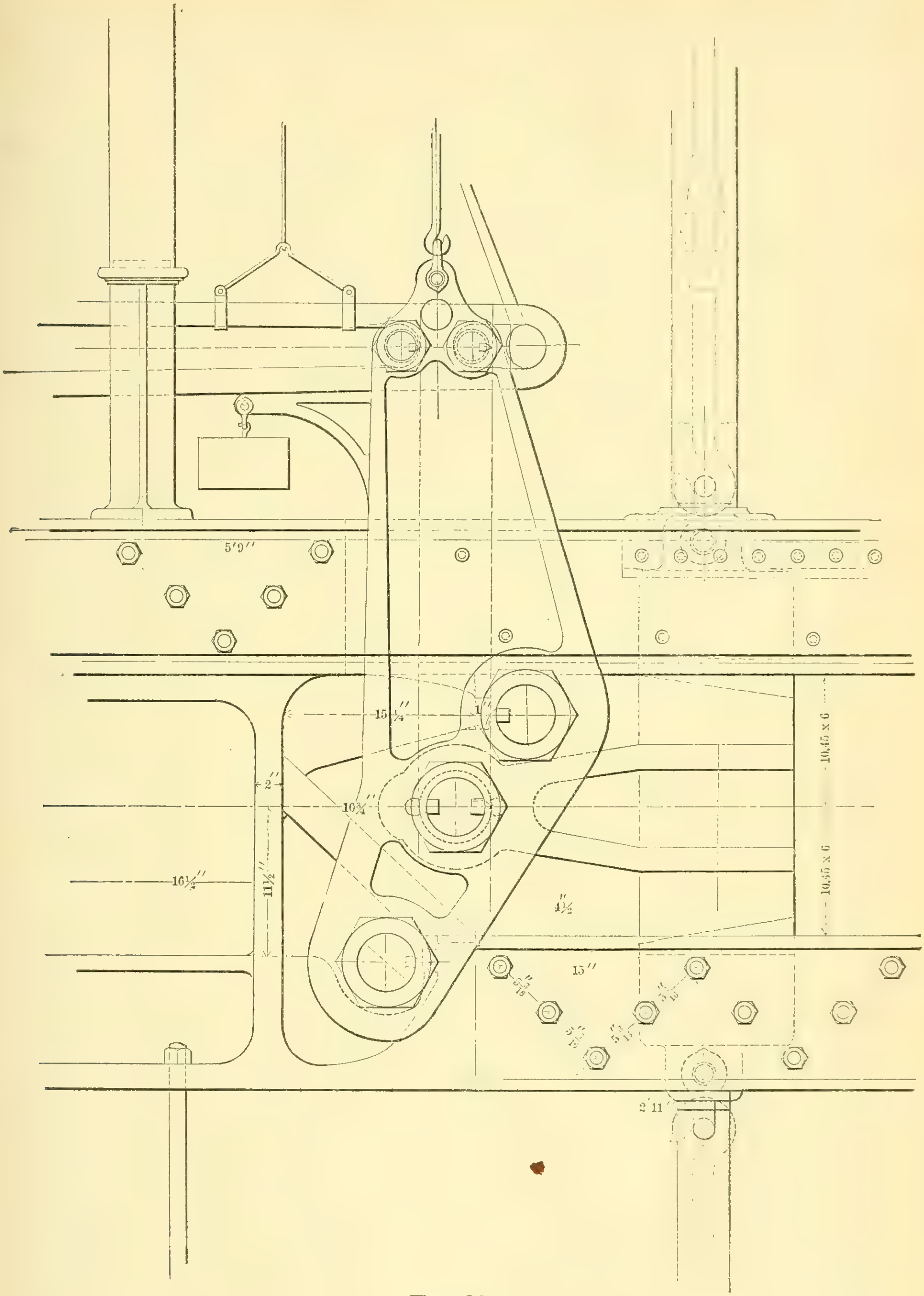


Fig. 20.

mechanism for estimating the stresses. This mechanism consists of two heavy levers hung from the framework of I-beam, so as to swing directly in front of the cross-head of the jack. On the center pivot of these levers there is hung a

second cross-head for the reception of the other end of the test piece. By means of two horizontal links these levers are attached to two knee levers for transforming the horizontal stress into a vertical one and carrying it to a differential lever which conveys it directly to the beam. The beam in this machine is an autographic beam, and the registering cylinder may be seen just behind it. The principal feature of this machine, which is different from those heretofore described, is the ease and facility with which it may be changed from tension to compression and conversely. Inasmuch as the jack is the double-acting one, all that is necessary in the straining mechanism is to alter the position of the valves at either end, so as to change the direction of the fluid in the pumps. The jack being secured to the frame by means of pins passing directly through the columns no change at all is necessary in this part of the apparatus, and the only remaining detail is to arrange the scale system so as to weigh, equally, stresses in tension or compression.

In Fig. 20 may be seen an enlarged detail of the main levers, with the device whereby this change is readily accomplished. Here it is seen that these levers are furnished with 5 pivots, the lower one bearing against a projecting toe on the main casting. The center has two edges instead of one and is surrounded by the casting forming the cross-head. The next one bears against a projection issuing from the front of the main casting, while the two on the top of the lever have seats in the link for conveying the stress to the knee levers. Suppose, for example, a piece in tension: as soon as the jack commences operation, the tendency is to pull the lever towards the jack, the lower pivot catches against the toe of the main casting, and the linear motion is now converted into a rotary one around this pivot as a center, and the stress of the jack is conveyed to the knee lever by means of the link joining the two. Suppose the tension piece to be removed and the compression test substituted therefor, the tendency then upon the operation of the jack is to push the main lever away from it, and towards the large casting, $\frac{1}{8}$ of an inch play being allowed between the vertical lines of the pivots; the lower edge moves away from the toe of the casting, and the third edge strikes against its projecting seat. The result, as in the previous case, is to change the motion of the lever into a rotary one around its pivot edge, and the stress produced on the jack is again converted into tension on the link connecting the top of the main lever with the knee lever, or, in other words, when the machine is acting in tension, the main levers are levers of the second class, and when it is acting in compression they are levers of the first class. By proportioning the pivot distances so that the multiplication is in both cases the same, a single set of levers answers both for tension and compression by simply turning down the little eccentric handle shown at the top, and throwing either one or the other of the link pivots into play. This machine in question has a capacity of 600,000 lbs. in either direction, and is capable of testing specimens between the first of the cross-heads up to 12 feet in length. A peculiar feature, however, is that the scale cross-head has an extremely large opening, and the framework is entirely open directly behind it, so that eye-bars or tension pieces of any length may be easily tested. If, for example, it is wished to test an eye-bar 40 feet long, the bar is introduced through the opening in the scale cross-head and the eye screwed to the cross-head of the jack, while the metal of the bar is gripped by means of wedges placed in the scale cross-head. That this is a fair test and gives all the information to be obtained from a testing machine of much longer dimensions will be readily seen from this fact: First, if the material of a bar is to be tested it can be done equally as well in a length of ten ft. as in a length of 40 ft., and if the construction, method of formation, or proportion of the eye is to be tested in relation to the bar, this is secured by the presence of one eye attached to the cross-head of the jack. Should it be desirable to test both eyes of the bar, this can readily be done by first breaking one and then turn the bar end for end and breaking the other one. Of course, this double test of a single bar requires a little more time, but to compensate for this the space occupied by the machine, the expense of construction, and the labor of manipulation, are reduced to an enormous extent.

REPORTS OF ENGINEERING SOCIETIES.

American Society of Civil Engineers, February 20, 1884.—A Paper on Structural Steel, by E. B. Dorsey, M. Am. Soc. C. E., was read. The paper gave the results of an examination by the writer into the subject during two recent trips to Europe. The steel used for structural purposes is called generally in England mild steel, and in Germany, homogeneous iron. Experts in Great Britain generally rely more upon physical tests and the reputation of the manufacturer, than upon chemical composition. The physical requirements are stated, and the manufacturer uses his discretion as to the composition which will answer these requirements. The rules for testing steel adopted by the British Admirality, by Lloyd's Register, and by the British Board of Trade, were given. The tendency among English engineers is to use steel still softer than has heretofore been thought best. Some large builders use nothing in their boilers over 26 long tons tensile strength per square inch, and 25 per cent. elongation in 8 inches. Others advise the use of steel of from 23 to 25 long tons tensile strength, with the same elongation.

American engineers require from 15 to 20 per cent. higher tensile strength than the English. The Siemens-Martin, or open heart steel, is preferred by nearly all experts for structural purposes, the Bessemer steel being principally used for rails. Ship builders are decided in their preference for the open hearth steel. A much larger number of plates would be condemned of the best wrought iron than of steel. Data were given as to loss of strength in steel plates by punching. Steel can be manufactured into much heavier, longer and wider pieces than wrought iron. Steel rivets are used on the Clyde exclusively in riveting steel. The new Forth bridge is to be built of mild steel. The use of mild steel is extending very rapidly in Europe, and has fast superseded iron for structural purposes.

The paper was discussed by members present. During the discussion Mr. Theodore Cooper referred to the conservative stand taken by him in a paper presented to the Society some four years since, and expressed the opinion that at the present time he would feel still more conservative in regard to the use of iron instead of steel for structural purposes, particularly for bridges or similar constructions.

For boilers, for ships, &c., steel has answered very well, but for structures he would be inclined as yet to advise the use of wrought iron. In compressions, in his opinion, steel has not been proved to be as strong as wrought iron, and the necessity for most careful inspection is greater for steel than for wrought iron.

Mr. M. N. Forney referred to the increasing use of steel for rails, for wheel-ties, and for various parts of locomotive machinery. He referred to the record of accidents which showed that some 66 per cent. of accidents in this country are due to derailment, and only 8 per cent due to the same cause in England. In this country the number of broken wheels is very great, and the tendency towards the use of steel for tires is decided.

Vice-President Paine gave details of the methods of tests of steel in use during the construction of the Brooklyn Bridge, and expressed an opinion favorable to the use of steel.

The paper was also discussed by Messrs. Collingwood, Frith and North.

March 5, 1884.—A paper by Hiram F. Mills, C. E., describing the construction of the Pacific Mills Chimney at Lawrence, Mass., was read by the Secretary. This chimney was built by Mr. Mills in 1873, and consists of an outside octagonal shell 222 feet high above the ground, with a distinct interior core 8 feet 6 inches in diameter inside, extending one foot above the top of the outer shell and eleven feet below the ground. The chimney is founded 19 feet below the ground, upon coarse sand, the foundation being 35 feet square enclosed by pine sheet piling. The base is concrete, one foot thick, then rubble masonry of large pieces of granite in cement, this stone work being 7 feet high. Upon the stone work is placed the brick chimney, the outer shaft being at the base 20 feet wide, and at the top under the projecting cornice 11 feet 6 inches wide. This brick work is 28 inches in thickness at the base; at 12 feet in height it becomes 24 inches, which continues 18 feet; then 20 inches for 20 feet; then 16 inches for 40 feet; then 12 inches for 60 feet; then 8 inches to the top. The inside core is two feet thick to a height of 27 feet and one foot thick for the remaining height of 154 feet. The top of the chimney is of cast iron plates $\frac{3}{4}$-inch thick. The horizontal flue entering the chimney is 7 feet 6 inches square. The vertical flue of the chimney is a cylinder 8 feet 6 inches in inside diameter, and 234 feet high with walls 20 inches thick for 20 feet, 16 inches thick for 17 feet, 12 inches thick for 52 feet, and 8 inches thick for 145 feet. The foundations were laid in mortar of Rosendale cement and sand; the outer shell in mortar of Rosendale cement, lime and sand; and the flue walls in mortar of lime and sand.

During the winter of 1873, the flue being 90 feet above the ground, boilers having 452 square feet of grate surface were connected with the chimney with satisfactory results. Between June and September, 1874, the chimney was finished. The approximate weight of the chimney is 2,250 long tons, the number of bricks being about 550,000. The chimney is opposite the middle of a line of 28 boilers, and 210 feet distant from them. It was designed to serve for boilers having 700 square feet of grate surface, burning about 13 tons of Anthracite Coal per square foot of grate surface per hour.

A description was then read of the chimney of the Merrimack Manufacturing Co., at Lowell, Mass., built under the direction of J. T. Baker, C. E., in 1882. This chimney is founded on a ledge of sand stone. The foundation, 30 feet in diameter, is built of granite blocks laid as they come from the quarry. At the surface of the ground there is a dressed granite base two feet six inches in height, laid in clear Portland cement, the remainder of the foundation being in Rosendale cement and sand. Upon this base is placed the brick work consisting of three cylinders, the outside one 28 feet in diameter, 24 inches thick, the middle one 18 feet in diam-

eter 8 inches thick, the core 12 feet inside diameter and 16 inches thick. The middle cylinder is carried up vertically 75 feet 6 inches; the outside ring has a batter of $\frac{4\frac{2}{3}}{100}$ of an inch per foot to a height of 100 feet. At the height of 75½ feet the middle ring connects with the exterior ring, making the masonry at that point 36½ inches thick; it is then 20 inches thick for an additional height of 60 feet; 16 inches thick for 70 feet; and 12 inches thick thence to the enlargement for the chimney head. The core is uniformly 12 feet inside diameter to the top, the first 100 feet being 16 inches thick; then 12 inches thick for 60 feet; then 8 inches thick for 90 feet; and then 4 inches thick for 29½ feet to the top. It is entirely separate from the outside masonry except about the door-ways and openings for the flues. The core was laid in mortar of lime and sand; the outside shell in lime, cement and sand.

A description was then given by Dr. Charles E. Emery, M. Am. Soc. C. E., of the construction of the chimney built under his direction, of the Greenwich Street Boiler House of the New York Steam Heating Company. This chimney was a creature of circumstances, it being necessary to place within a very limited area, a very large boiler capacity, viz., 16,000-horse power. This was done by making four stories of boilers —the chimney was therefore necessarily located with reference to these boilers, and the plan of the chimney was determined by the shape of the lot. The beach of the Hudson River was at some time at this locality, and the foundation of the chimney was placed in fine clear beach sand with some packets of coarser sand and a little stone. The foundation is one foot below high water. The chimney is 27 feet 10 inches in the clear inside, and 8 feet 4 inches wide. The height is 220 feet above high water—221 feet above the foundation.

Mr. F. L. Griswold, M. Am. Soc. C. E., described a chimney erected in Mexico for a cotton factory about 160 feet high, which had been in use for over 12 years, which was built of apparently sun-dried bricks, and which seemed to be now in excellent condition. This chimney was built by Indians and seemed to be very symmetrical and well made. The bricks were about 10x3x7.

Mr. H. W. Brinkerhoff, M. Am. Soc. C. E., described a chimney constructed of old rails, which was in successful use in Pennsylvania. It was generally known as a Crinoline chimney.

Mr. Wm. E. Worthen, M. Am. So. C. E., referred to several chimneys built by him, and expressed a doubt as to the necessity of very great height in chimneys.

ENGINEERS' CLUB OF PHILADELPHIA.—Record of regular meeting, February 16, 1884.

Mr. Chas. A. Ashburner detailed his experience in the use of Aneroids in Leveling. He exhibited a collection of various forms of instruments and described their advantages and errors, and the means of correcting the latter, the graphical method being specially recommended.

The Secretary presented, from Mr. Edwin Ludlow, a description of the Exhaust Injector, with results of experiments therewith.

The Secretary presented, from Prof. J. A. L. Waddell, an illustrated paper upon Lateral Systems for Iron Pratt Truss Highway Bridges. The accompanying tabular data are based upon a lower than maximum wind pressure, upon the assumption that travelers do not venture upon highway bridges during gales of maximum velocity, and could not escape injury if they did, even if the bridge were proportioned as he considers railroad bridges should be, for maximum wind pressure when covered by a train. In the same connection the relative inconvenience of loss of highway and railroad bridges is also considered.

MARCH 1st, 1884.—President Ludlow described tests of the crushing strength of ice which were made by him in order to learn, approximately, the strength required for an ice harbor of iron screw-piles, in mid-channel, at the head of Delaware Bay. Eighteen pieces were tried with government testing machines at Frankford, Philadelphia, and at Fort Tompkins, Staten Island. The specimens were, carefully prepared 6 inch and 12 inch cubes, and roughly cut slabs about 3 inches thick, of different qualities and from different localities. For pure Kennebec ice the lowest strength obtained was 327 pounds, and the highest 1,000 pounds per square inch. For inferior qualities the strengths varied from 235 to 917 pounds. The higher results were obtained, generally, when the air temperature in the testing room was from 29° to 36° F.; as against 55° to 68° F. for the lower results. The pieces generally compressed ½ to 1 inch before crushing.

The Secretary exhibited for Mr. C. A. Ashburner a set of blues of some yet unpublished details of the Chicago Cable Railways.

The Secretary presented a note, by Prof. W. S. Chaplin, upon a prevalent error in data given for determining the true meridian, by observing the instant at which Polaris and Alioth come into the same vertical, and then following Polaris for a *certain time* at the expiration of which it is said to be on the meridian. He gives, as the true time, the following: Latitude 40°, 25 minutes, 36 seconds; Lat. 50°, 25 m. 24 s.; Lat. 60°, 25 m., 5 s.; Lat. 70°, 24 m. 29 s.

Mr. C. J. Quetil, visitor, exhibited models of the Wire Truss recently described by him.

Prof. Mansfield Merriman presented a statement of the progress of the triangulation carried on in Pennsylvania by the United States Coast and Geodetic Survey, and exhibited a map showing its present condition. The stations located are twenty-seven in number, of which nineteen have been occupied for the measurement of angles. These are in the southeastern part of the State, in the Counties of Bucks, Montgomery, Delaware, Chester, Lancaster, York, Dauphin, Lebanon, Berks, Lehigh, Northampton, Monroe, Carbon and Schuylkill. The geographical positions of the stations have been computed from two of the sides of the primary cost triangulation, one near Elkton, Md., and the other near Trenton, N.J., which serve as bases for the work thus far executed.

ENGINEERING NOTES.

THE leading organ of German free trade, commenting on the recent discussion in the Chamber of Deputies on the coalition of the steel rail works, remarks that since the new iron duties came into operation in 1879, the German steel works have drawn about sixty millions of marks from the ratepayers in the shape of subventions. That is to say, that almost invariably, when quoting for foreign contracts, German steel manufacturers have quoted about 30 to 60 per cent. less than they charged for the same goods to German railways, being enabled by the heavy customs duties to take advantage of the latter.

THE Danube Regulation Committee has decided to spend the sum of 1,488,000fl. on works of regulation along the river in 1884. These works will extend from Stein to Deutsch-Altenburg, on the Hungarian frontier. The chief of them are the continuation of the inundation dikes below Vienna, to which 600,000 fl. are to be devoted, and the great regulation works at Deutsch-Altenburg, on which, as a first instalment, 168,000fl. are to be expended this year. According to the opinion of experts, the imperfections experienced near Vienna are entirely owing to the works not having been carried out to a sufficient distance down stream.

A JOINT for driving bands, in which great strength is combined with an even surface, has lately been offered to the public, although it has been used privately for upwards of five years. Messrs. Bailey Bros., of Chancery Lane, were applied to by a large firm of mill-band makers to supply a cement, so modified from that which they have long made for mending china and glass, that it might be used for joining machine belts. The ends of the belt to be joined are cut to a long scarf, not, however, so long as the width of the belt. The cement is supplied hot, the two parts are put carefully together and the joint is subjected to pressure from a weight or in a vice. The joint becomes set in twelve hours; but it is preferable not to use the belt before twenty-four hours have elapsed, when, it is said, the joint is the last place at which the belt will break. The practice is to suspend the belt, with a heavy weight on the joint, for the double purpose of testing the joint and of stretching the belt. We are assured that the joint will stand both heat and damp, and that one has never been known to give way.

THE trial at Lyons has been announced of a large dredger for the Corinthian Canal, which is to connect the Gulf of Athens with the Lepanto. This canal, according to measures more recently given in continental journals, is 7600 metres in length, 8 metres deep, and 23·5 metres in width. Amongst the largest dredgers in use on the works are two, one of which has, as above stated, been recently tried by the makers, MM. Demange et Satre. Each dredge is to extract 5000 cubic metres of material per day of ten hours; the buckets each have a capacity of 750 litres. A compound engine of 300-horse power raises fourteen of these buckets per minute. The hulls of the dredger were made by M. Debianne at Vaise. At the trial, M. de Lesseps is said to have expressed himself so astonished with the quantity of work done by it that he means to have the same kind to dig out the central parts of Africa so as to make that central sea, which, says a French paper, "he has in preparation with his ordinary activity—without neglecting the Suez Canal or the works at Panama— and which is to crown his career according to the opinion of the Grand France."

THE Macquarie Lighthouse, at the entrance to Port Jackson, is one of the largest and finest in the world; it is also the finest example of electric lighting of which the southern hemisphere can boast. It was commenced on March 1st, 1880, and the light in connection with it was brought into operation on 1st June, 1883. The old lighthouse, which the new one has replaced, was built in 1816, and was the first structure of the kind in the southern hemisphere. The light is of the first order, a sixteen-sided, dioptric, holophotal revolving white light, of the system of Teulere, commonly attributed to Fresnell, showing a flash of eight seconds in every minute, and having a range of twenty-five miles seaward. It is, however, discernible for a considerably greater distance, owing to the luminosity produced in the atmosphere by the electric beam before the direct rays become visible. It was constructed by Messrs. Chance Bros. & Co., of Birmingham, under the supervision of Sir Jas. N. Douglass, engineer to the Trinity Board. The lighthouse is fitted with gas and oil apparatus as well as electric light. The Macquarie Lighthouse is intended only to illumine half the horizon, it is therefore possible to make use of the landward rays by means of a dioptric mirror. This is probably the first instance of the use of a dioptric mirror for an electric light.

IRON AND STEEL NOTES.

THE BESSEMER STEEL INDUSTRY OF GREAT BRITAIN IN 1883.—The following report has just been published by the Secretary of the British Iron Trade Association, Mr. J. S. Jeans.

The total production of Bessemer steel ingots in the United Kingdom in 1883 was 1,553,380 tons, against a total of 1,673,649 tons in 1882. This amounts to a decrease of 120,269 tons, or 7 per cent.

It is probable that 1883 was the first year during which there was a decreased production of Bessemer steel in the United Kingdom since that industry became fairly established. It is, at any rate, the first year that has shown a decrease since 1878, when the returns of the production were first collected by the British Iron Trade Association, as the following figures show :

Production of Bessemer Steel Ingots in the United Kingdom in each year from 1878 *to* 1883, *both inclusive.*

Year.	Make of Ingots, Gross Tons.	Year.	Make of Ingots, Gross Tons.
1878	807,527	1881	1,441,719
1879	834,511	1882	1,673,649
1880	1,044,382	1883	1,553,380

The principal decrease of make in 1883 has taken place in the Sheffield district, and is, of course, due mainly to the removal of one of the largest works to another part of the country. In the Cleveland district the decline of 25,018 tons is mainly due to labor difficulties.

The production of Bessemer steel rails in the United Kingdom in 1883 was 1,097,174 tons, against 1,235,785 tons in 1882. There has therefore been a diminished make of 138,611 tons in 1883.

RAILWAY NOTES.

An electric locomotive, fitted with the apparatus of Mr. Leo Daft, is now running experimentally on the Saratoga, Mt. McGregor and Lake George Railway, a somewhat steep and crooked 3 ft. gauge line 10 miles long, laid with 32lb. rails. The following particulars are given in the *Electrician and Electrical Engineer:* "A mile of this road was fitted for electric operation by tightening the joints of the existing track, and laying a special middle rail or conductor upon wooden blocks, which were saturated with pitch. Starting from the terminus the track ascends gradually for 600 ft., and then descends for 2,000 ft. more, when a sharp curve and an ascending grade of 93 ft. to the mile is encountered. The electric motor is designed for heavy work. It bears the name of 'Ampére.' The motor is 9 ft. 6 in. long, 5 ft. wide, stands 3 ft. above the rails, and weighs 4,500 lbs. The armature and field magnets are inclosed in a box at the rear of the platform, in front of which is the driver's seat, and a dashboard carrying three controlling switches and a keyboard. The right switch makes and breaks the current, the left switch controls an electric brake, and the center switch and keyboard control the combinations of the coils of the motor answering to the cut-off of the steam locomotive. The reverse lever is on the right. Two phosphor-bronze wheels press firmly upon the center rail by the action of a spring upon their pivoted supports. The current passes from this rail through the wheels to the switches and keyboard, thence to the electric engine, and through the driving wheels to the outer rails. The generators are two in number, of Mr. Daft's manufacture, each occupying a space 5 ft. by 4 ft., and weighing 1,200 lbs. They were driven by a 25 horse-power Fitchburg engine, and the current led thence to the rails—a distance of about 200 ft.—through underground conductors. On the day of the trial the motor was attached to an ordinary 10-ton passenger coach, into which sixty-eight passengers—nearly double its ordinary capacity—were crowded, while the motor carried six more, making a total load of about 17 tons. The 'Ampère' started slowly, but surely, and ran to the end of the mile without stopping, at the rate of about 8 miles an hour. Descending the 93 ft. grade on the return trip, a considerable speed was attained, causing the motor to jump the track at the curve, by which accident its running gear was slightly damaged. The trip was made in the presence of nearly 2,000 spectators, many of whom amused themselves by trying to obtain shocks from the track. But the sensation was found to be scarcely perceptible. Since the first trial the 'Ampere' has been run in wet and snowy, as well as dry, weather, with satisfactory results. The results of this trial show that an electric locomotive under perfect control, weighing 2 tons, will haul an ordinary passenger coach, weighing, with its load, 15 tons, over heavy grades and sharp curves at a speed of eight miles per hour, by means of an electric current, generated by a 25 horse-power engine, and transmitted one mile."

ORDNANCE AND NAVAL.

New Cruisers.—The Senate, by a vote of 38 to 13, passed the bill for the construction of steel cruisers for the navy on Feb. 29th. The bill as passed authorizes the President to direct the construction of seven steel vessels for the navy, consisting of one cruiser of 4,500 tons displacement, one cruiser of 3,000 tons, one dispatch vessel of 1,500 tons, two heavily-armed gunboats of 1,500 tons each, one light gunboat of 750 tons, and one gunboat not to exceed 900 tons. It further authorizes the construction of one steel ram, one cruising torpedo boat, and two harbor torpedo boats. It provides that the Naval Advisory Board shall examine all the plans, specifications and contracts, and materials for the vessels, and shall give the Secretary of the Navy advice and assistance thereon, but that the Board shall have no authority to enter into contracts; that no contract shall be entered into until full and complete plans and specifications shall have been prepared and approved in writing by the Board or a majority of its members.

Mr. C. Moller's steamship circular for January says, the extraordinary amount of tonnage which has been constructed during the last three years both in this kingdom and abroad has far exceeded the requirements; and although many new trades have been opened and the losses been very heavy, it has become impossible to find profitable employment for many of the ships now afloat. Instead of laying them up in the same way as the sailing vessels used to be in former times during dull periods, owners have kept them running at freights which, in many instances, left a loss, and the result has been amply verified by the small dividends received by the shareholders. During the past year no less than 720 steamers with a total of 1,102,801 tons, were added to the mercantile fleet in the United Kingdom alone, and if to this be added 674 steamers of 982,961 tons, built in 1882, and 630 steamers of 925,000 tons in 1881, we obtain the respectable total of 2,024 steamers and 3,010,762 tons in three years. And although small in comparison, yet the number of ships constructed on the Continent, especially in France, Sweden and Germany, form also a not inconsiderable fleet.

The Armament Question.—A lecture on this subject was delivered at the Royal United Service Institution by Captain C. O. Browne, R. A.; this officer occupies the position of lecturer on armor plates at Woolwich, and he may therefore be looked upon as an authority on the subject.

The lecturer commenced by stating that his chief object was to call attention to features in the position of the armor question that were peculiar and of importance. He then proceeded to give the data on which he based his arguments, and which were the results of experiments carried out at Meppen in March, 1882; at Spezzia at the end of 1882; at St. Petersburg at the beginning of 1883; and at Shoeburyness in August and September, 1883.

We can summarize these trials as follows:

At Meppen the trials were of perforation of soft armor.* A 110 lb. shot, with a velocity of 1,749 ft., and striking energy of 2,330 foot-tons, went through a target of two 7 in. wrought iron plates, with 10 in of wood between them, and passed on over 800 yards, uninjured. This projectile would probably have penetrated $11\frac{1}{2}$ in. of wrought iron, and the extra 3 in. accomplished here is due to the two plates being so far separated. The same gun fired at 35 deg. to the normal, against an 8 in. plate, with 10 in. backing of wood, penetrated. There were also trials made of large bursting charges, and stronger envelopes for common shell.

At Spezzia the trials were of hard armor steel-faced plates, 19 in. thick, from Cammell & Co. and Sir John Brown & Co., and steel plates from Creusot. The 100-ton gun was used, and the result of two rounds on each target was decidedly in favor of the Schneider steel.

The St. Petersburg trials were carried out between Cammel's steel-faced armor and Schneider steel plates, 12 in. thick, an 11-in. gun being used. The result of three rounds was that the latter plate fell to pieces, while the former target had by far the best of it.

The experiments carried out by our engineers were to ascertain the protection afforded to granite walls by iron plates. The 80-ton gun was used, the targets being of 2.8 in. iron plates with 5 inches of wood between them, and 12 in. Wilson's steel-faced iron.

The battering projectile from the 80-ton gun passed clean through the plate and 10 feet into the masonry beyond. Another round being fired at the 12-in. plates broke up in the iron. This projectile was supposed to be capable of penetrating 25 in. of iron. The fact of a 12-in plate being able to keep it out must, the lecturer considered, be attributed to the hard granite backing.

Captain Browne then went on to discuss our system of calculating effects. In Russia, Italy and England the blow is estimated on the power of the shot to perforate, and this, he says, is true enough when wrought iron is the target, but where steel faces are used, and perforation is not effected, the action is more that of a pointed wedge striking and splitting the plate before it has entered very far. The effect would probably be proportional to the total energy of the blow delivered, but without reference to the full diameter of the shot, which never comes into play; thus the advantage of a small diameter would not be apparent. With a steel-faced target the shot are generally matched against the plate on two standards. On the lower one the shot has just sufficient power to perforate wrought iron of the same thickness. On the higher standard it has sufficient penetrating power to perforate a wrought-iron plate of perhaps 20 per cent. more thickness than the plate. The lecturer illustrated his meaning by the following example:

A 9-in. Woolwich gun and $5\frac{3}{4}$-in. Krupp were fired in comparison; the former had 118 foot-tons of energy per inch circumference, and the latter 123. Their penetration, according to the wrought-iron standard, would therefore be about the same, but their total energies were respectively 16,400 and 5,800 foot-tons. It seems absurd that these two should be taken as having the same effect on steel armor. The lecturer then illustrated his meaning practically with three bullets of the same weight, shaped like punches, with diameters of $\frac{1}{4}$ in., $\frac{1}{2}$ in. and 1 in. respectively. His object was to test the two systems by comparing the projectiles. The equation of drop being $W\,h=\pi\,D\,t^2\,K$, where

W=weight
h=height of fall
D=diameter of projectile
t=thickness of sheet punched
K=some constant,

it follows that everything else being constant the height from which the projectiles must be dropped in order to perforate a soft material will vary with the diameter. The experiment was then shown as follows: A piece of felt was used as representing the soft armor, and the 1 in. diameter projectile perforated it when dropped from a height of 60 in.; the projectile with the $\frac{1}{4}$ in. diameter being then substituted, it was found that a drop of 15 in. sufficed for perforation. In order to show the difference in the case of hard plates, brick was taken as the material, and the $\frac{1}{4}$ in. broke it from a height of 19 in.; the same weight of projectile with the 1 in. head broke another similar plate from 25 in., hence the law of perforation cannot be taken as giving a measure of the relative powers of the projectile in this case. The lecturer then proposed, as a check on the perforation standard, to divide stored-up work by the number of tons in each plate and thus get the shock per ton of plate. He admitted, however, that the question is full of difficulties, and that it is by no means easy to fix a fair standard.

Turning to the consideration of projectiles, he maintained that more experiments should be tried with our projectiles against foreign armor, such as Gruson's, and that we should ourselves further the manufacture of steel projectiles by all means in our power. He attributed the hanging back of the manufacturers in submitting steel projectiles for trial, to their inability to obtain plates to test these projectiles on beforehand. Hence, when the projectile is submitted it is really only in an experimental state, and manufacturers are not willing that their reputation should be endangered in this way. He suggested that plates left from previous experiments should be used for this purpose. As a matter of fact, we understand that this has already been done with the plates tested in the Nettle at Portsmouth.

Captain Browne considered that experiments

* By soft armor is meant that which yields to perforation. By hard armor that which cannot be perforated, but is destroyed by breaking up.

against hard armor are specially needed in this country, and maintained that our service projectiles have never been tried against either Schneider steel or Gruson's chilled iron. As the lecturer very wisely remarked, it is against foreign armor we will have to contend, and therefore our projectiles should be capable of damaging it. He said that Krupp had tried chilled projectiles against Gruson's armor, and they failed to do anything, simply breaking up, and that eventually he had to use steel shot. Finally, the lecturer summed the points of his lecture as follows:

1. The need of a better system of estimating effects on hard armor.
2. The need for developing the manufacture of steel projectiles.
3. The necessity of making trials against Gruson's or other very hard armor.

An animated discussion followed, in which Admirals Boys, Hamilton, and Selwyn, Sir Chas. Nugent, Captain McKinley, R.A., and Mr. Walter Browne took part. Nothing throwing much light on the points raised came out in this discussion. Admiral Selwyn considered that the principle of the anvil was the one we ought to go on, but Mr. Walter Browne and the lecturer in his reply did not think the two cases analogous. It appears to us that all the lecturer's points are worthy of serious attention. Krupp appears to have manufactured good steel projectiles, and there is certainly no sound reason why we should be behindhand in this matter.

Steel Deckplate Trials.—Further experiments with steel deckplates have taken place at Eastney. The targets in this instance consisted of five two-inch armorplates, strongly set in a wooden frame and placed at a slight angle to the line of fire. They were attacked with the 9-inch muzzle-loader, with uniform battering charges, each plate receiving a couple of rounds. Considering the severe test to which they were subjected at a range of about 100 yards, the targets withstood the ordeal remarkably well. One place only succumbed to the attack, the damage being inflicted apparently by the blow of the butt end of the projectile as the point glanced off. Two of the plates were not perforated, the graze of the shot where it struck and glanced off to sea being only marked by a well-defined bulge. The result of the experiments demonstrates the advantage which is likely to attend the peculiar contour which has been given to the under-water deck of the *Imperieuse.*

Recent Trials of Machine Guns.—Some doubts having been expressed as to the accuracy of the published results of the trials instituted by the Greek government with the Nordenfelt 1½-inch machine shell-gun at Athens in May and June last, on which occasion this gun succeeded in penetrating a 3.4-inch target (made up of several thinner plates), at a range of 210 yards, further experiments were made with a similar weapon at Dartford, on October 16, in the presence of numerous foreign attaches, colonial representatives, and others, the particulars of which we (*United Service Gazette*) herewith give. The Nordenfelt 1½-inch gun, placed at a distance of 50 yards, was fired against a target made up as follows: Four iron plates, with an aggregate thickness of 2.68 inches; five steel plates, with an aggregate thickness of 0.59 inch; thus the total thickness of the compound target was 3.27 inches. The steel bullet fired from this gun completely penetrated this target, and buried itself to a depth of four feet in the sand butt behind. This result confirms in the most practical and conclusive manner the accuracy of the Greek experiments. Further confirmation of the great penetrative power of the Nordenfelt 1½-inch gun is afforded by the official trials, which have been lately carried out in Roumania and in Brazil. The Brazilian target No. 2 was formed of four ½-inch iron plates, with a space of 15½ inches between the first and second and second and third, the last two plates being placed close together. All the targets were easily perforated, with the exception of the Brazilian 4-inch solid iron one; in the latter case the steel bullet entered the plate to a depth of 2.7 inches. An ordinary 9-pounder gun was also fired against this 4-inch plate at the same range, but only penetrated 2.28 inches, showing nearly ½ inch less penetration than the Nordenfelt 1½-inch gun, which throws a projectile of 1.75 lb. weight. In the Roumanian trials the accuracy of fire of the Nordenfelt 1½-inch gun was also tested with the following results: At a range of 1,432 yards 14 out of 15 shots hit the target, fired with deliberate aim, with a mean deviation of 12 inches, and at a range of 820 yards 16 out of 22 shots, fired as rapidly as possible, struck the target, 16 feet by 16 feet, the aim being unaltered after the first shot. In the Dartford experiments the rapidity of fire of this Nordenfelt gun was also tested, on which occasion seven shots were fired in 12 seconds, and six shots in 10½ seconds, or at the rate of 35 shots per minute. Further, it was shown how easily and rapidly the mechanism of this machine gun, consisting of only ten pieces, could be taken to pieces and put together, the times being respectively for each operation 24 seconds and 64 seconds. Another very important question was also considered at the late Dartford trials, viz., the possibility of firing Boxer cartridges from rifle calibre machine guns. Two series of 100 rounds of Boxer ammunition were fired from the Nordenfelt 10-barreled gun, each in seven seconds; and from the Nordenfelt 5-barreled gun 150 rounds of the same ammunition were fired in twelve and six seconds respectively; that is for the former weapon a rate per minute of 855 shots, and for the latter one of 500 shots, which must be considered satisfactory when the nature of the cartridges used is considered. The importance of these experiments lies in the fact that the Boxer cartridge must remain for some years longer as the ammunition for the Martini-Henry rifles used by our volunteers, Indian and colonial forces, and also for some considerable time as the rifle ammunition of our regulars at home. Recently, by order of the Sultan, trials of the Nordenfelt gun took place at Tchataldja, in presence of a committee composed of His Excellency Ristow Pasha, three generals of artillery, and Mehemet Bey, Edham Bey, and Ali Bey (captains in the Imperial Ottoman navy)

In the trials the target, composed of four plates of Cammell's compound, was placed at the distance of 500 meters, when the bullets penetrated three of the plates and smashed the fourth. Amongst the trials for precision, one of the most important was at 2,000 meters, against a target 5 meters square. At this long range the result was most satisfactory, two of six shots having struck. The report of the committee has not yet been submitted to his majesty, but it is understood that the adoption of the Nordenfelt gun will be recommended for the Turkish navy, as it is the only one satisfying the conditions required for modern warfare, especially in its defensive power against torpedoes. We published the results of a series of trials with Nordenfelt guns, which took place at Dartford on July 26, in *Iron* of August 10.

BOOK NOTICES.

The Creators of the Age of Steel. By W. T. Jeans. London: Chapman & Hall.

This is a series of short biographical sketches of modern workers and inventors in the field of metallurgy of iron and steel.

The greater portion of the work is devoted to Sir Henry Bessemer, Sir William Siemens and Sir Joseph Whitworth. A less amount of space is awarded to Sir John Brown, Mr. S. G. Thomas and Mr. S. G. Snelus.

The whole is related in a pleasing style, omitting irrelevant details, and reciting fully the successive steps of development of the important inventions and discoveries.

The book is without illustrations; the topography is good.

A Bibliography of Electricity and Magnetism, 1860 to 1883, with Special Reference to Electro-Technics. Compiled by G. May. London: Trubner & Co.

A convenient hand book for scientific writers. The index is classified under the languages in which the works are written, and enables, therefore, the reader to find under one heading all works on any specialty.

Although prepared for students, mere school books are omitted from the list.

Absolute Measurements in Electricity and Magnetism. By Andrew Gray, M.A., F.R.S.E. London: Macmillan & Co.

This book is not offered as a full treatise on Electrical and Magnetic Measurements, but is designed to give a clear account of the system of absolute units as now adopted, and of some methods and instruments by which the system can be applied in practical work.

Separate chapters treat of Determination of the Horizontal Component of Earth's Magnetic Field, Absolute Unity of Magnetic Pole and Field, Measurement of a Current in Absolute Units, Definition of Absolute Units, and Derivation of Practical Units, Graded Galvanometers and their Graduation, Potentials in Derived Circuits, Comparison of Resistances, Measurement of Energy in Circuits, Measurement of Intense Magnetic Fields, Dimensions of the Units of Physical Quantities.

Electric Lighting: Its Principles and Practice. By A. A. Campbell Swinton. London: Spottiswoode & Co.

A summary of the methods at present employed in Electric Lighting is a convenience to any reader of current scientific literature. The author of this treatise begins by an elucidation of the theory of the production and transmission of Electric Energy, defining accurately the technical terms, and describing with precise phraseology the mechanical appliances which are common to all devices employed for such purpose. The question of quantitative measurements of electricity is next treated in a satisfactory manner. The "Sources of Power" for Electric Machines occupy but little space, and deserve perhaps less. The *principles* of the various Dynamo Machines are presented without mention of the bewildering complexity of mechanical details, which so often obscures the description in current technical literature.

Arc Lamps and Incandescent Lamps are next treated, and with full descriptions of all the devices that have sufficiently stood the test of trial to deserve mention.

"Accumulators" are briefly described, and then "arrangement of the different complete systems," and "applications, advantages and cost" complete the work.

The book deserves success for the clear, concise treatment of an interesting but technical subject.

Topographical Surveying. Science Series No. 72. By Geo. J. Specht, Prof. A. S. Hardy, John B. McMaster, C.E., Henry F. Walling, C.E. 18mo. Price 50 cents.

It is seldom that so much that is valuable to the student of surveying is found in so small a compass.

The separate parts were originally prepared as contributions to this magazine, and while compactly written in the interest of economy of space, they were full enough to clearly set forth the newer methods of surveying.

The Stadia method is fully described in Mr. Specht's article, and the proper time and method of its application clearly explained. This method is now widely used in general surveys. Prof. Hardy's essay is an elucidation of the method of topographical surveying as practiced in France; that the author is a master of the art of teaching this essays plainly shows.

Mr. McMaster's portion of the book relates to special cases that frequently occur in any general survey. As neat applications of Modern Geometry they are no less interesting than useful.

The last essay is a valuable contribution to science from a surveyor of wide experience in the art of measuring large areas, with reference to the correct delineation on maps.

The illustrations of this little book are numerous and good.

Mulhall's Dictionary of Statistics. By Michael G. Mulhall, F. S. S. London: George Routledge & Sons.

The author, in a brief preface, says: "Hitherto every science has had its Dictionary except Statistics," and claims this is the first Statistical Dictionary in any language.

The subjects are presented in alpbabetical order, beginning with Abbeys and ending with Zinc Industry. Variable statistics are presented for several dates ending with 1880.

Statistics relating to Debts, Banking, Steam Power, Cattle Raising and money are represented graphically by full-paged colored diagrams.

It is a book which a large number of people, whether writers, teachers or students, may find exceedingly useful.

MISCELLANEOUS.

M. Mauser has succeeded in sending a telephonic message to 100 listeners at the same time, at a distance of 250 kilometres—155 miles—namely, from Paris to Nancy. This he is said to effect by the use of stronger currents than usual, but this must involve special construction of the telephone.

Dr. Hammerl has devised an ingenious method for measuring the intensity of the light of electric lamps, by which the necessity for placing them at a great distance from the standard candle is avoided. He interposes a revolving disc, in which are cut out sectors, allowing only a portion of the rays to pass. For instance, if the sum of the angles of the sectors be 180 deg., half the light will be intercepted. It has been found that three sectors are sufficient to give a uniform light with a moderate speed of rotation. Consequently, to reduce the light to a third three sectors of 40 deg. are employed. With three sectors of 12 deg. the light is reduced to one-tenth of its actual power. By employing two discs, each provided with three sectors of 60 deg. cut out, and arranged one behind the other on the same axis, they may be made to give as great a reduction of the luminosity as may be desired.

In an article on Gutta-Percha in the *Journal of the Society of Arts*, Mr. James Collins says:—"Dr. Oxley calculated that to supply the 6918 piculs—1 picul equal 133⅓ lb.—exported from Singapore, from the 1st of January, 1845, to 1847, 69,180 trees were sacrificed; and, according to the *Sarawak Gazette*, 3,000,000 trees were required to supply the 90,000 piculs exported from this district during 1854 to 1874. These are only two instances, the first, showing the trade in its infancy; and the second, that of a limited and comparatively small locality. In fact, the gutta-percha tree has only been saved from utter annihilation because trees under the age of twelve years do not repay the trouble of cutting down. Still, it is clear that the growth of young trees of the best varieties has not kept pace with the destruction, but are becoming much scarcer, so that recourse now, more than ever, has to be had to the products of very inferior varieties. At the present time there is a great difficulty in obtaining sufficient supplies of the best varieties, especially for telegraphic purposes. The Indian Government, acting on the advice of the late Mr. Howard, F.R.S., Mr. Markham, Dr. Spruce, and others, have taken up the india-rubber question. The Colonial Government should now take up the question of gutta-percha."

A pigmy voltaic battery of great power for its size has been devised by M. Skirwanov, and is now employed to furnish the star lights on the heads of the ballet of "La Farandole," Paris. The *Times* says, it gives an ampere for one hour, and has an electro-motive force of 1.45 volts. Two of these cells, contained in ebonite cases buckled to the belt of the performer, keep a star light going, and the light is readily controlled by the wearer. Each cell consists of a zinc plate bent into the form of a U, and holding in its inside a plate of silver, surrounded by chloride of silver, as in the ordinary De La Rue chloride of silver cell used by electricians for testing purposes. The zinc plate forms one pole of the cell and the silver the other. A solution of caustic potash—75 parts potash to 100 water—is filled in, and as a porous diaphragm, the chloride of silver is covered with parchment paper. The vessel is of ebonite, with closed mouth, which, however, is opened when fresh liquid is put into the cell. This is necessary after every hour's run, and the chloride of silver has to be replaced after three or four hours of use. The cell is thus expensive, but this is of minor importance in theatrical work as compared with its small size and weight—100 grammes.

Will electricity enable us to transmit power in large quantities more efficiently than other means? Will it enable us to transmit small quantities? These questions were put to the Society of Arts, and answered by Professor Osborne Reynolds in his Cantor lectures as follows: Thanks to the experiments of M. Deprez, we can say that a current of electricity, equivalent to 5-horse power, may be sent along a telegraph wire ⅙-inch in diameter, some ten miles long—there and back—with an expenditure of 29 per cent. of the power, because this has already been done. Compared with wire rope, this means falls short in actual efficiency, as Messrs. Hems send 500-horse power along a ¾-in. rope. To carry this amount, as in the experiment of Deprez, 100 telegraph wires would be required; these wound into a rope would make it more than 1.4in. in diameter, four times the weight of Mr. Hems' rope. With the moving rope the loss per mile is only 1.4 per cent., while with the electricity it was nearly 6 per cent.; so that, as regards weight of conductor and efficiency, the electric transmission is far inferior to the flying rope. Nor is this all. With the flying belt, Mr. Hems found the loss at the ends, in getting the power into and out of the rope, 2½ per cent.; whereas, in M. Deprez's experiments, 30 per cent. was lost in the electric machinery alone, which is very small as such machinery goes. But this is not all. No account is here taken of the loss of power in transmission to and from the electric machinery. Taking the whole result, it does not appear that more than 15 or 20 per cent. of the work done by the steam engine could have been applied to any mechanical operation at the other end of the line, as against 90 per cent. which might have been realized with wire rope transmission. To set off against this, electricity has the enormous advantage in the conductor being fixed, and in the fact that it is likely to be, if anything, less costly and more efficient for small quantities of power than for large.

VAN NOSTRAND'S

ENGINEERING MAGAZINE.

NO. CLXXXV.—MAY, 1884.—VOL. XXX.

ON THE EMPLOYMENT OF MATHEMATICAL CURVES AS THE INTRADOS OF ARCHES.

By WILLIAM HY. BOOTH, of Manchester.

Contributed to Van Nostrand's Engineering Magazine.

Rankine, in his work on "Civil Engineering," treats upon the employment of the Hydrostatic arch modified or transformed to suit earth pressure for the profile of a tunnel. This transformed curve he terms the Geostatic arch, and he gives approximate formulæ for its calculation. He also goes pretty fully into the use of the Catenarian curve as a figure for bridges when suitably transformed to suit the depth of load. He does not, however, place the matter in a very clear light, and, as a consequence, it is to be feared that little or no use is made of his deductions in practice, but that too frequently the arches of bridges and tunnels are struck merely to the circular curve without reference to the question of equilibration, the possibly excessive depth of arch ring being sufficient to contain the curve of equilibrium within or near to the middle third of its thickness.

In Rankine's work, also, this subject is treated somewhat disconnectedly, being spread over different pages in various sections of the book, so that considerable labor is involved in collecting the full amount of material necessary to complete the calculation of an arch.

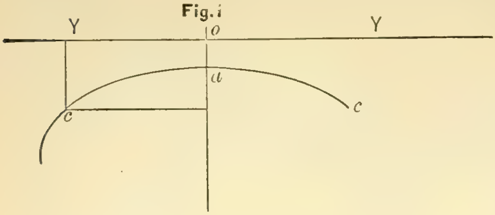
The writer therefore makes no apology for the present essay, for the materials of which he has drawn upon Rankine's "Civil Engineering," but hopes that the examples he has worked out may serve to show the real simplicity and elegance of the methods employed by Rankine, and be of assistance to students who would closely follow the natural line in the design of engineering structures.

The symbols employed are the same as those used in Rankine, which may be of assistance in further study.

Commencing with the tunnel, the intrados of which should be a transformed hydrostatic arch, the words used by Rankine may be quoted here. Describing the hydrostatic arch he says it "is a linear arch or rib, suited for sustaining normal pressure at each point proportional, like that of a liquid in repose to the depth below a given horizontal plane.

"The radius of curvature at the given point in the hydrostatic arch being inversely proportional to the intensity of the pressure, is also inversely proportional to the depth below the horizontal plane at which vertical ordinates representing that intensity commence." Thus, if *c.c* be a submerged arch, and YY be the surface of the water above it, the radius at the point a will bear the ratio to the radius at $c = \frac{\mathrm{Y}c}{oa}$, being greater than the radius at c in proportion as it is nearer to the surface. This arch then is

Fig. 1



exposed to a constant thrust throughout every part, the intensity of the thrust being simply the product of the intensity of pressure at any point by the radius of curvature at that point, and the arch can yield only by the crushing of the material of which it is composed.

Liquid presses equally in every direction, but the pressure of earth varies according to its nature, and the intensity of the lateral pressure of earth upon a buried arch is less than the vertical pressure in a ratio dependent upon the angle of repose φ of the material comprising the strata through which the tunnel passes.

Calling px the vertical pressure and py the horizontal pressure against a tunnel or culvert, the width of a hydrostatic arch suitable for a liquid load, the intensity of which is px at the crown, must be contracted in the ratio $c=\sqrt{\frac{py}{px}}$ at points below the crown when intended to withstand earth pressures. The transformed archway is therefore much less in width than its corresponding hydrostatic linear arch, the reduction being greater in the more stable earths than in those whose angle of repose is low, *e*, near the horizontal. Calling the angle of repose of earth φ the value c may be thus written:

$$\mathrm{C}=\sqrt[2]{\frac{\mathrm{P}y}{px}}=\sqrt[2]{\frac{(1-\mathrm{Sin}:\varphi)}{(1+\mathrm{Sin}\ \varphi)}}$$

Now, Rankine states that in designing a tunnel by transformation from the hydrostatic arch, knowing the depth x_0 of the tunnel crown below the origin of pressure, u the surface above, and also the depth x_1 of the greatest horizontal diameter below the same datum, it is merely requisite to design the hydrostatic arch as for fluid pressure, and then to contract the horizontal ordinates in the ratio $\frac{c}{1}$ to obtain the profile of the tunnel.

This would certainly obtain such a tunnel as would be perfectly safe, and it might happen that the various dimensions would be correct also; but this would not always be the case, in fact seldom so.

As a rule—an almost invariable rule—the data given to the designer would be the width and the height of the tunnel, and the depth x_0 of the crown below the surface would also be known.

If then, the greatest horizontal diameter of the tunnel was taken at the floor, this would give with a height from floor to crown$=h$, $x_1=x_0+h$.

Now, the width of the tunnel is fixed by two axiomatic rules. In the first place it must be sufficient to allow of the required traffic through the tunnel, whether this be by boat, or one or more railway trains, or otherwise. In the second place, it is requisite, as a matter of structural economy, both in excavating and masonry, that the width should not exceed the minimum safe dimensions.

But the ratio $\frac{c}{1}$ of the $\frac{\text{Geostatic ordinate}}{\text{Hydrostatic ordinate}}$ is a fixed quantity depending upon the frictional stability of the earth through which the tunnel is pierced, and if x_0 and x_1 are also fixed there would be no escape from the necessity of making the tunnel either too wide or too narrow, unless the alternative of increasing or diminishing the height were resorted to.

If such a way out of the difficulty had to be followed, the only two methods practically available would be an increase in either height or width to maintain a minimum possible traffic requirement of with, width a corresponding increase in cost of excavating and masonry or brickwork.

To the student of Rankine, the omission on his part to indicate the method of procedure is often embarrassing, but the solution is simple, and consists in fixing the greatest horizontal diameter above the floor line instead of at that level. The total height of the tunnel being h, if we made $x_1=x_0+h$ we should find that our geostatic arch, unless in very stiff material, would be far wider than we required, but by making $x_1=x_0+a$ we may obtain any suitable width we please, a being anything less than h, and representing the distance of the greatest

horizontal width of tunnel below the crown of the archway.

It is a difficult problem to accurately calculate the hydrostatic arch, but we may make our calculations with a degree of exactness quite sufficient for practical purposes.

Greater clearness will be attained if we take a numerical example. Let it be required to run a tunnel through sand, which has an angle of repose $\varphi=23°$, the height and greatest width to be 27 feet each. Required the form of the archway at a depth at the crown=40 feet below surface. The following will be our data:

Height from crown to invert $= h =$ 27 ft.
Extreme width . . . 27 ft.
Half span . . . $S=$ 13.5 ft
Depth of crown below surface$=x_0=$40ft.
$\varphi=23_0$

From the above we must obtain the following dimensions:

Depth below surface of greatest horizontal diam $= x_0$
Rise of arch above greatest horizontal diam $= a$
Thickness of brickwork at crown $= t$
Ditto at depth x_1 . . $= t_1$
Radius at crown . . . $= r_0$
Radius at depth x_1 . . . $= r_1$
Hydrostatic half span . . $= y$

The first operation necessary is to find the value of the coefficient C.

$\varphi=23° \therefore \text{Sine } \varphi= .39073$
$1-\text{Sin } \varphi= .60927. \quad 1+\text{Sin } \varphi=1.39073$

$$\therefore \sqrt{\frac{1-\text{Sin } \varphi}{1=\text{Sin } \varphi}} = \sqrt{.4381} = .662 = \text{C}$$

Now C is the ratio of the $\dfrac{\text{half geostatic span}}{\text{half hydrostatic span}}$

or $\text{C}=\dfrac{\text{S}}{y}$

S has been fixed at 13 ft. 6 in.

$$\therefore y=\frac{13.5}{.662}=20.4 \text{ ft.}$$

We have now to design a hydrostatic arch with a half span of 20.4 feet, and a crown depth of load of 40 feet, from which, by a horizontal contraction of $\frac{13.5}{20.4}$, we obtain the tunnel we require.

The one dimension we require for this is $x^0=x_l+a$, a being the depth of greatest width below the crown, and x_1 below the surface. This dimension a must be obtained by a process of trial and error, the first trial often being sufficient.

The full height h being 27 feet we will approximate to a by making it to equal,

$$h\times\frac{\text{S}}{y} = 27\times\frac{135}{204}$$

or nearly, 18 feet, and we will take it as 18 feet. Then, using Rankine's approximate formula, make

$$b=y+\frac{y^2}{30a}= 20.4+\frac{416.16}{540}= 21.17$$

this gives one value to b calculated with the factor a as in the denominator. Another value of b is obtained from the formula

$$b=a\sqrt{\left(\frac{x_0+a}{x_0}\right)} = 18\sqrt[3]{\frac{58}{40}}=$$

$18\times1.13185=20.3733$. Here the factor a is in the numerator.

Taking the real value of b as a mean between these two we have

$$b= .5\ (21.17+20.3733)=20.77$$

Now we have also from Rankine a value of $y=b-\dfrac{b^2}{30a}$ and taking b at 20.77 we may place the formula, thus assuming still that we retain 18 as the value of a

$$y=20.77-\frac{431.4}{540} \text{ or } y=20.77-.799=$$

19.97 feet. Now, the true value of y as calculated from c and the proposed half span of the tunnel is 20.4 feet, the result obtained as a first approximation is thus .43 of a foot too narrow.

Multiplying 19.97 by c we get

$19.97\times.662=13.22$, as compared with the intended half span $s=13$ ft. 6.

a discrepancy of less than three inches and a half on the half span. We may judge from this that the first approximation to the height a of the crown above the greatest horizontal diameter was slightly in error, and that a should be a little over 18 feet, but it is unnecessary in the present instance to obtain a nearer approximation to the exact dimension, because if the half span of the tunnel is

made $13\frac{1}{2}$ feet instead of 13.22, the increased outward thrust of the side walls will be sustained by the resistance of the earth; whereas, if made exactly to theoretical dimensions, there is a greater probability of lateral collapse taking place, and though we may not exactly in theory build to theoretical lines, it is as well to know exactly what these lines are, so that with the knowledge of the strata obtained by experience we may make a just and proper allowance for this contingency, the sidewalls of a tunnel being more liable to fail by crushing inwards than the roof. Thus we see that the greatest diameter is by no means necessarily at the floor level, but the sidewalls incline inwards in a curve below the level of greatest depth, their tendency to slip inwards being resisted by the invert, which it is essential should be well bedded on the substratum, and curved to such a radius that if the material underlying it tends to force it upwards it may resist that tendency equally with the sidewalls' resistance.

A very near approximation to a would be obtained by taking a mean value between $a=h\times c$ and y.

Thus in the present case the true value of a is very nearly 19 feet or

$$\frac{18+20.4}{2}=19.2$$

We will, however, adhere in our drawing to the first approximation, 18 feet, as allowing the above margin for lateral thrust.

It may be as well remarked here that for deeply buried tunnels the profile may be made truly elliptical, no deference being paid to the geostatic form, because the ratio of $x_0 : x_1$ is so nearly unity in such a case, and a circular profile transformed into an ellipse merely be an alteration of the horizontal widths in the ratio $\frac{c}{1}$ is the required section for such an archway.

In designing an elliptic tunnel the half span s is first to be decided upon, and from this the circular radius $y=\frac{s}{c}$ is to be found, then y is also the major axis of the ellipse, of which the tunnel is a portion, just so much of the ellipse being appropriated as is required to give the required height. When the depth of earth above the tunnel crown is very small the ellipse is quite unsuited, and it is important that the geostatic form be considered, for it is evident that if the tunnel crown be only 5 feet from the surface of the ground the value of the expression

$$\sqrt[3]{\left(\frac{x_0+a}{x_0}\right)}$$

becomes much increased.

With the prevalence of underground railways at small depths this becomes important, and in the tunnel above considered we should get, as the value of a, about 14 feet, or, in other words, the line of greatest width of the tunnel would be but little above its half height.

Hence we see the necessity for strong wiry walls at the mouths of tunnels, and a rapid surchargement of the earth above, so that the necessity of so nearly a circular section may be surmounted for. The profile of the above tunnel would evidently be nearly circular, the greatest horizontal diameter being so nearly at half height, and the side walls below this point sloping inwards with almost the same slope as their inclination above the same point.

For a double track tunnel especially, this would be very inconvenient, necessitating a great amount of ballast filling to give room for the track and vehicles. Above the line of greatest width, however, there would be better accommodation for the upper part of the vehicles than in a tunnel like that shown in Fig. 2, for instance.

When, therefore, it becomes requisite to cut a tunnel very near the surface, it may sometimes be found better to give it a width greater than is absolutely required, in order that the dimension a, being thus increased, the figure of the tunnel may include a smaller part only of that portion below the greatest diameter. Such a correction is less difficult on account of the greater headroom allowed in the more circular archway. In fact, the nearer we approach the surface the more does the figure of the arch approximate to the hydrostatic arch, and the greater is the extra [otherwise unnecessary] width of excavation necessary.

A glance at Fig. 2 will show this, for, supposing the figure of the tunnel were made to accord with the hydrostatic

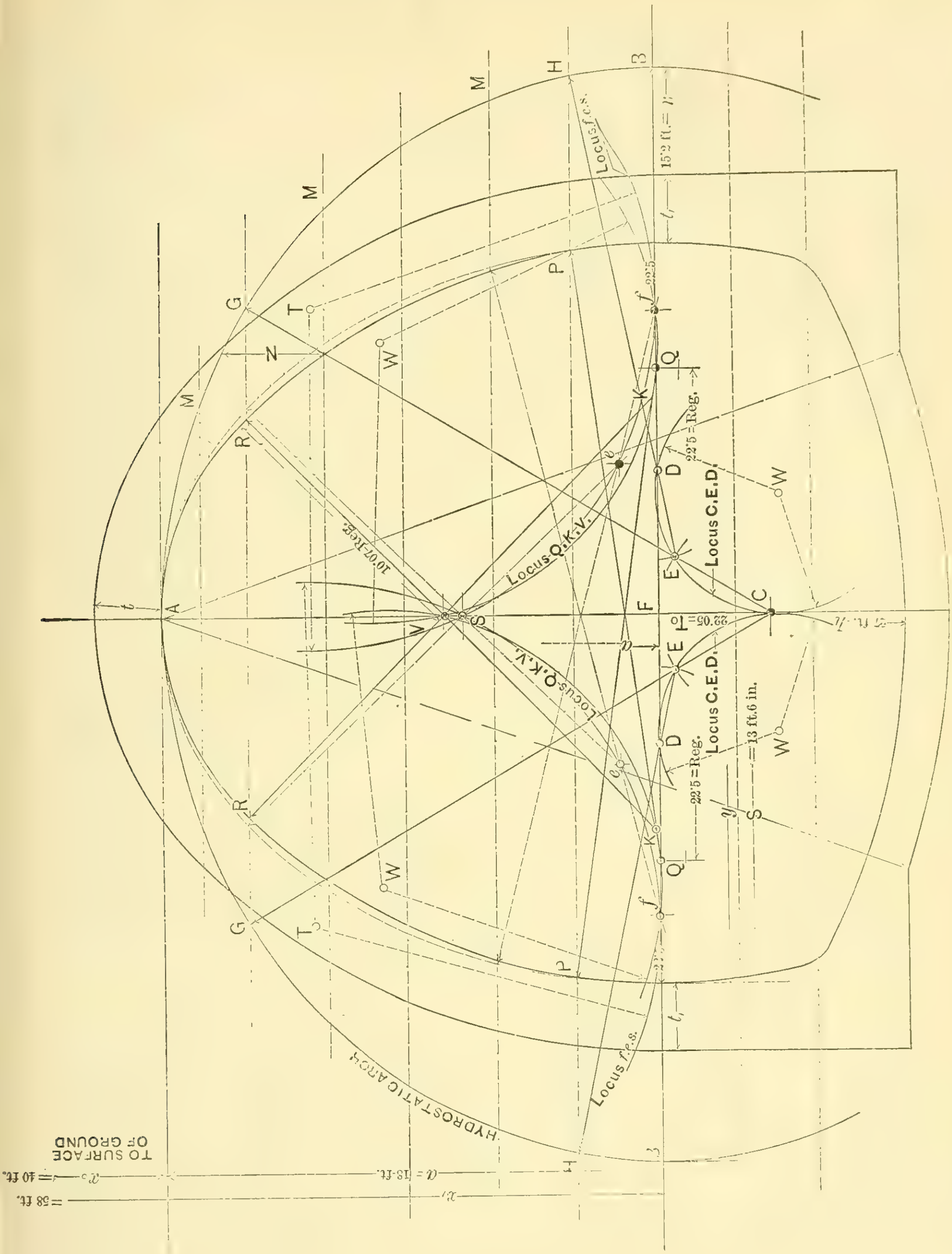


Fig. 2

Locus of Centers, Hydrostatic Arch, C.E.D. Center of Locus W.
" Geostatic by reduced ordinates f.e.s. Curve dotted. Center of Locus T.
" " calculated radii ($x_0^3\,x_1$) Q.K.V. Curve in full line. Center of Locus W.

Span 27 ft.
Height 27 ft.

curve it is evident that if the inner arch was calculated to give a sufficiency of headway at the line N, then with the wider arch the floor might be raised by the amount N, and the clearance would still be sufficient, and thus the greater amount of excavation due to width would be in part compensated for by the decreased amount at the floor. It is important that a tunnel section should not at the sides be flatter than the geostatic form would allow, though there appears, as a rule, to be no objection, especially in firm ground, to the curva-

ture being increased, for the outward thrust in this case is taken by the earth behind, whilst in the former case the sides crush or are in danger of crushing inwardly. In fact the outward thrust is in stone arches, as Rankine remarks, specially counted upon the arch tending to spread, laterally compresses the material of the spandrils at the haunches, until the spandril resistance is equal to the thrust of the arch. On this account, therefore, the arch centering of elliptical and similar approximately hydrostatic arches should not be struck until time has been allowed for the settlement and hardening of the material of the spandril filling. Rankine says the backing should be solid up to the level of the crown.

It now remains for us to draw in the figure of the hydrostatic arch, and by transformation the tunnel section, to do which we must find the radius of the hydrostatic arch at the crown and at the greatest horizontal diameter, that is at a depth x_0 and x_0+a, or x_1.

Rankine's formula for this is as follows: Calling r_0 the radius at the crown and r_1 that at the depth x_1, we have

$$r_0=a+\frac{a^2}{2x_0} \text{ and } r_1=a-\frac{a^2}{2(x_0+a)}$$

Substituting known values of a and x_0 we get

$$r_0=18+\frac{18^2}{80}=22.05 \text{ ft.}$$

and $$r_1=18-\frac{18^2}{2\times58}=15.2 \text{ ft.}$$

to draw in the hydrostatic figure (Fig. 2) the crown radius $r_0=22$ ft. 0.6 inches must be set out from the crown upon the center vertical. The springing radius r_1 $=15$ ft. 2.4 inches must then be set off from the end B of the greatest horizontal diameter. Then the points C and D thus found are two centers. Then from C, with the radius CF, draw an arc intersecting at E, another arc drawn from D as a center, with a radius equal to FA—BD, *i.e.*, to the difference of the least and greatest radii of the arch. Then CG drawn through E, and EH drawn through D, mark the limits of the arch curve struck from the three centers C, E and D. Having drawn in this curve, the true profile of the tunnel may be found by reducing the breadth of the tunnel at several horizontal lines, as MM in the ratio C to 1.

The radius at the crown and at the greatest diameter may be calculated thus:

The reduced arch crown radius is to the radius of the hydrostatic crown, as $C^2:1$, and at the greatest horizontal diameter the same ratio is $\frac{1}{c}$, the crown radius becoming smaller and the springing radius larger.

Now the value of c was determined from the frictional stability of the earth to be .662, and having fixed our half span as 13 ft. 6, we found our hydrostatic span to be 20 ft. 4.8 inches. Having, however, chosen a somewhat too small we found the width of the arch which resulted come out as 19.97 instead of as 20.4, and as in our drawing of the hydrostatic arch we have made the half span FB=19.97 we must alter the value of c proportionately. The new value is

$$C_c=\frac{20.4\times C}{19.97}=\frac{204\times662}{1997}=.676$$

and the horizontal ordinates of the tunnel will be less than the corresponding hydrostatic ordinates in the ratio .676 to 1, to which ratio a scale of reduction can be made. Practically the ratio is $\frac{2}{3}$. For the alteration of the radii with .676 $=c_c$ the new value of c, we get $c^2=.457$, and $\frac{1}{c}=1.48$ nearly. Then the geostatic radii at crown and springing r_0g, and r_1g respectively are found to be

$$r_0g=22.05\times.457=10.07,$$

$$r_1g=15.2\times1.48=22\ 5.$$

With these radii the crown and springing curves may be drawn to such limits as are indicated by the intersections of the radii at R and P with the horizontal projection from the limiting points G and H of the hydrostatic radii, as already indicated, the intermediate portion of the arch RP being struck with a radius found by producing the limiting radii at R and P until they intersect at K, which is a point in the locus of centers. The coincidence, or approximate coincidence of the tunnel profiles found by this method and the method of reduced horizontal co-ordinates will show the extent o error or difference in the method of re-

duced ordinates, though both are for all practical purposes sufficiently accurate.

The method of intersection of radii for the intermediate portions of the geostatic arch does not give exactly accurate meeting of the three arcs with the three centers alone, but the crown and springing arcs being struck with the radii as calculated. The remaining portion is to be drawn in by a few trial approximations. The centers of curvature all lying upon the curves marked locus QKV, which curves are drawn from the centers W to pass through the three determined points Q, K and V.

The method given by Rankine of finding the hydrostatic arch by an approximation of the centers CED is stated by him to give a curve too full at G and too contracted at H. This shows itself, in the present case, the method of transformation by reduction of the horizontal ordinates, producing a tunnel section (shown dotted) which falls beyond the section, found by calculated radii, at the level of c and the locus of centers f e s for striking it approximately, having the centers T in place of W.

It is unnecessary to go into calculations of greater refinement for the purpose of tunnelling, and indeed it may be said that the sections of tunnels were fixed before the subject ever received the attention of mathematicians, these forms being merely empirical alone, and found by experiment, that is, by actual practice, for in the early days of railway engineering the brick linings of tunnels would frequently give way, and when this took place the section would be made of sharper curvature at the point of weakness thus indicated, and in time the tunnel form was correctly developed to be afterwards explained mathematically. There is, however, good reason that engineers should understand the principles underlying buried archways. They are thus enabled to design their structures to approximate to correctness. In studying Rankine, a young engineer is often led to attach undue importance to theoretical forms, especially in regard to stone or brick archways, which are stated to be insecure when the middle third of the arch ring does not contain the curve of equilibrium. Doubtless, for a simple arch ring, this may be very true, but in practice with the solid spandril backing carried up almost to crown level, and with a considerable depth of ballasting and roadbed, it is almost impossible for an arch, as usually constructed, to give way in the manner indicated by Rankine, the thrust being by no means entirely borne by the so-called arch ring, but by the mass of backing or spandril walls also.

The thickness of tunnel walls is usually determined by practical experience, and is rarely less than 18 inches. It should be sufficient to withstand the superincumbent weight of earth which, for each foot of length of tunnel will be

$$w[x_1(S+t)-('8a\times S)],$$

the minus quantity in the equation being the approximate area of the half tunnel above the line of greatest breadth, and w being the weight per cubic foot of the superincumbent earth.

It is only in archways near the surface and in loose earth that it can be assumed that the side walls of a tunnel bear the whole weight of material between the tunnel and the ground surface, and when no other data are as hand it is best to assume a thickness at calculated from the empirical formula generally used for the purpose, namely,

$$t \text{ in feet} = \sqrt{\left(.12 \text{ to } .5\right)\times r}$$

according to the firmness or slipperyness of the material, r being taken as $\frac{a^2}{s}$, which in the present case will give 23 feet.

With the coefficient .2 this will give

$$t=\sqrt{.2\times 23}=\sqrt{4.6} \text{ or say 2ft. 3 in.}$$

It is, of course, specially important that as the lining of a tunnel is built in the void between the brickwork or masonry, and the excavated sides of the tunnel be firmly rammed up solid and close, so that when the earth pressure comes upon the tunnel, possibly with greater force at first at some point than at another, the archway may not be able to crush in at that particular point, owing

to the remainder of the arch being unable to yield outwardly by reason of the support of the well rammed earth behind.

It is this precaution in practice which has no doubt contributed to the stability of many rule-of-thumb structures which do not coincide at all closely with the theoretical form as deduced from the principle of the hydrostatic arch.

ELECTRIC LAUNCHES.

By A. RECKENZAUN.

From the "Journal of the Society of Arts."

It is not my intention to treat this subject from a shipwright's point of view. The title of this paper is supposed to indicate a mode of propelling boats by means of electrical energy, and it is to this motive power that I shall have the honor of drawing your attention.

The primary object of a launch, in the modern sense of the word, lies in the conveyance of passengers on rivers and lakes, less than for the transport of heavy goods; therefore, it may not be out of place to consider the conveniences arising from the employment of a motive power which promises to become valuable as time and experience advance. In a recent paper before the British Association at Southport, I referred to numerous experiments made with electric launches; now it is proposed to treat the subject in a wider sense, touching upon the points of convenience in the first place; secondly, upon the cost and method of producing the current of electricity; and thirdly, upon the construction and efficiency of the propelling power and its accessories.

Whether it is for business, pleasure, or war purposes, a launch should be in readiness at all times, without requiring much preparation or attention. The distances to be traversed are seldom very great, fifty to sixty miles being the average.

Nearly the whole space of a launch should be available for the accommodation of passengers, and this is the case with an electrically-propelled launch. We have it on good authority that an electric launch will accommodate nearly double the number of passengers that a steam launch of the same dimensions would; therefore, for any given accommodation we should require a much smaller vessel, demanding less power to propel it at a given rate of speed, costing less, and affording easier management.

A further convenience arising from electromotive power is the absence of combustibles, and the absence of the products of combustion — matters of great importance; and for the milder seasons, when inland navigation is principally enjoyed, the absence of heat, smell, and noise, and, finally, the dispensing with one attendant on board, whose wages, in most cases, amount to as much or more than the cost of fuel, besides the inconvenience of carrying an additional individual.

I do not know whether the cost of motive power is a serious consideration with proprietors of launches, but it is evident that if there be a choice between two methods of equal qualities, the most economical method will gain favor. The motive power on the electric launch is the electric current; we must decide upon the mode of procuring the current. The mode which first suggested itself to Professor Jacobi, in the year 1838, was the primary battery, or the purely chemical process of generating electricity.

Jacobi employed, in the first instance, a Daniell's battery, and in later experiments with his boat on the river Neva, a Grove's battery. The Daniell's battery consisted of 320 cells containing plates of copper and zinc; the speed attained by the boat with this battery did not reach one mile and a quarter per hour; when 64 Grove's cells were substituted, the speed came to two and a quarter miles per hour; the boat was 28ft. long, 7½ft. beam, and 3ft. deep. The electromotor was invented by Professor Jacobi; it virtually consisted of two discs, one of which was stationary, and carried a num-

ber of electro-magnets, while the other disc was provided with pieces of iron serving as armatures to the pole pieces of the electro-magnets, which were attracted whilst the electric current was alternately conveyed through the bobbins by means of a commutator, producing continuous rotation.

We are not informed as to the length of time the batteries were enabled to supply the motor with sufficient current, but we may infer, from the surface of the acting materials in the battery, that the run was rather short; the power of the motor was evidently very small, judging by the limited speed obtained, but the originality of Jacobi deserves comment, and for this, as well as for numerous other researches, his name will be remembered at all times.

It may not be generally known that an electric launch was tried for experimental purposes, on a lake at Penllegaer, near Swansea. Mr. Robert Hunt in the discussion of his paper on electro-magnetism before the Institution of Civil Engineers, in 1858, mentioned that he carried on an extended series of experiments at Falmouth, and at the instigation of Benkhausen, Russian Consul-General, he communicated with Jacobi upon the subject. In the year 1848, at a meeting of the British Association at Swansea, Mr. Hunt was applied to by some gentlemen connected with the copper trade of that part, to make some experiments on the electrical propulsion of vessels; they stated that although electricity might cost thirty times as much as the power obtained from coal, it would, nevertheless, be sufficiently economical to induce its employment for the auxiliary screw ships employed in the copper trade with South America.

The boat at Swansea was partly made under Mr. (now Sir William) Grove's directions, and the engine was worked on the principle of the old toys of Ritchie, which consisted of six radiating poles projecting from a spindle, and rotating between a large electro-magnet. Three persons traveled in Hunt's boat, at the rate of three miles per hour. Eight large Grove's cells were employed, but the expense put it out of question as a practical application.

Had the Gramme or Siemens machine existed at that time no doubt the subject would have been further advanced, for it was not merely the cost of the battery which stood in the way, but the inefficient motor, which returned only a small portion of the power furnished by the zinc.

Professor Silvanus Thompson informs us that an electric boat was constructed by Mr. G. E. Dering, in the year 1856, at Messrs. Searle's yard, on the River Thames; it was worked by a motor in which rotation was effected by magnets arranged within coils, like galvanometer needles, and acted on successively by currents from a battery.

From a recent number of the *Annales de L'Électricité* we learn that Count de Moulins experimented on the lake in the Bois de Boulogne, in the year 1866, with an iron flat-bottomed boat, carrying twelve persons. Twenty Bunsen cells furnished the current to a motor on Froment's principle turning a pair of paddle-wheels.

In all these reports there is a lack of data. We are interested to know what power the motors developed, the time and speed, as well as dimensions and weights.

Until Trouvé's trip on the Seine in 1881, and the launch of the Electricity on the Thames in 1882, very little was known concerning the history of electric navigation.

Mr. Trouvé originally employed Planté's secondary battery, but afterwards reverted to a bichromate battery of his own invention. In all the primary batteries hitherto applied with advantage, zinc has been used as the acting material. Where much power is required, the consumption of zinc amounts to a formidable item; it costs, in quantity, about 3d per pound, and in a well-arranged battery a definite quantity of zinc is transformed. The final effect of this transformation manifests itself in electrical energy, amounting to about 746 watts, or one electrical horse-power for every two pounds of this metal consumed per hour. The cost of the exciting fluid varies, however, considerably; it may be a solution of salts or it may be dilute acid. Considering the zinc by itself, the expense for five electrical or four mechanical horse-power through an efficient motor, in a small launch, would be 2s. 6d. per hour. Many persons would willingly

sacrifice 2s. 6d. per hour for the convenience, but a great item connected with the employment of zinc batteries is in the exciting fluid, and the trouble of preparing the zinc plates frequently. The process of cleaning, amalgamating, and re-filling is so tedious that the use of primary batteries for locomotive purposes is extremely limited. To recharge a Bunsen, Grove, or bichromate battery, capable of giving six or seven hours work at the rate of five electrical horse-power, would involve a good day's work for one man; no doubt he would consider himself entitled to a full day's wages, with the best appliances to assist him in the operation.

Several improved primary batteries have recently been brought out, which promise economical results. If the residual compound of zinc can be utilized and sold at a good price, then the cost of such motive power may be reduced in proportion to the value of those by-products.

For the purpose of comparison let us now employ the man who would otherwise clean and prepare the primary cells at engine-driving. We let him attend to a six horse-power steam-engine, boiler, and dynamo machine for charging 50 accumulators, each of a capacity of 370 ampere hours, or one horse-power hour. The consumption of fuel will probably amount to 40 lbs. per hour, which, at the rate of 18s. a ton, will give an expenditure of nearly 4d. per hour. The energy derived from coal in the accumulator costs, in the case of a supply of 5 electrical horse-power for 7 hours, 2s. 9d.; the energy derived from the zinc in a primary battery, supplying 5 electrical horse-power for seven hours, would cost 17s. 3d.

It is hardly probable that any one would lay down a complete plant, consisting of a steam or gas engine and dynamo, for the sole purpose of charging the boat cells, unless such a boat were in almost daily use, or unless several boats were to be supplied with electrical power from one station. In order that electric launches may prove useful, it will be desirable that charging stations should be established, and on many of the British and Irish rivers and lakes there is abundance of motive power, in the shape of steam or gas-engines, or even water wheels.

A system of hiring accumulators ready for use may, perhaps, best satisfy the conditions imposed in the case of pleasure launches.

It is difficult to compile comparative tables showing the relative expenses for running steam launches, electric launches with secondary batteries, and electric launches with primary zinc batteries; but I have roughly calculated that, for a launch having accommodation for a definite number of passengers, the total costs are as 1, 2.5, and 12, respectively, steam being lowest and zinc batteries highest.

The accumulators are, in this case, charged by a small high-pressure steam-engine, and a very large margin for depreciation and interest on plant is added. The launch taken for this comparison must run during 2,000 hours in the year, and be principally employed in a regular passenger service, police and harbor duties, postal service on the lakes and rivers of foreign countries, and the like.

The subject of secondary batteries has been so ably treated by Professor Silvanus Thompson, and Dr. Oliver Lodge, in this room, that I should vainly attempt to give you a more complete idea of their nature. The improvements which are being made from time to time mostly concern mechanical details, and although important, a description will scarcely prove interesting.

A complete Faure-Sellon-Volkmar cell, such as is used in the existing electric launches is here on the table; this box weighs, when ready for use, 56 lbs.; and it stores energy equal to 1 horse-power for 1 hour=1,980,000 foot-pounds, or about 1 horse-power per minute for each pound weight of material. It is not advantageous to withdraw the whole amount of energy put in; although its charging capacity is as much as 370 ampere hours, we do not use more than 80 per cent., or 300 ampere hours; hence, if we discharge these accumulators at the rate of 40 amperes, we obtain an almost constant current for $7\frac{1}{2}$ hours; one cell gives an E. M. F. of 2 volts. In order to have a constant power of 1 horse for $7\frac{1}{2}$ hours, at the rate of 40 amperes discharge, we must have more than 9 cells per electrical horse-power; and 47 such cells will supply 5 electrical horse-power for the

time stated, and these 47 cells will weigh 2,632 lbs.

We could employ half the number of cells by using them at the rate of 80 amperes, but then they will supply the power for less than half the time. The fact, however, that the cells will give so high a rate of discharge for a few hours, is, in itself, important, since we are enabled to apply great power if desirable; the 47 cells above referred to can be made to give 10 or 12 electrical horse power for over two hours, and thus propel the boat at a very high speed, provided that the motor is adapted to utilize such powerful currents.

The above-mentioned weight of battery power—viz., 2,632 lbs., to which has to be added the weight of the motor and the various fittings—represents, in the case of a steam launch, the weight of coals, steam-boiler, engine and fittings. The electro motor capable of giving four horse-power on the screw shaft need not weigh 400 lbs., if economically designed; this, added to the weight of the accumulators, and allowing a margin for switches and leads, brings the whole apparatus up to about 28 cwt.

An equally powerful launch engine and boiler, together with a maximum stowage of fuel, will weigh about the same. There is, however, this disadvantage about the steam power, that it occupies the most valuable part of the vessel, taking away some eight or nine feet of the widest and most convenient part, and in a launch of 24 feet length, requiring such a power as we have been discussing, this is actually one-third of the total length of the vessel, and one-half of the passenger accommodation; therefore I may safely assert that an electric launch will carry about twice as many people as a steam launch of similar dimensions.

The diagram on the wall represents sections of an electric launch built by Messrs. Yarrow & Co., and fitted up by the Electrical Power Storage Company, for the recent Electrical Exhibition in Vienna. She has made a great number of successful voyages on the River Danube during the autumn. Her hull is of steel, 40 ft. long and 6 ft. beam, and there are seats to accommodate forty adults comfortably. Her accumulators are stowed away under the floor, so is the motor, but owing to the lines of the boat the floor just above the motor is raised a few inches. This motor is a Siemens D_2 machine, capable of working up to 7 horse-power with 80 accumulators.

In speaking of the horse-power of an electro-motor, I always mean the actual power developed on the shaft, and not the electrical horse-power; this, therefore, should not be compared to the indicated horse-power of a steam engine.

I am indebted to Messrs. Yarrow for the principal dimensions and other particulars of a high-pressure launch engine and boiler, such as would be suitable for this boat. From these dimensions I prepared a second diagram representing the steam-power, and when placed in a position it will show at a glance how much space this apparatus will occupy. The total length lost in this way amounts to 12 feet, leaving for seating capacity only 15 feet, while that of the electric launch is 27 feet on each side of the boat; thus, the accommodation is as fifteen to twenty-seven, or as twenty-two passengers to forty, in favor of the electric launch.

Comparing the relative weights of the steam power and the electric power for this launch, we find that they are nearly equal, each approaches 50 cwt.; but in the case of the steam launch we include 10 cwt. of coals, which can be stowed into the bunkers, and which allow 15 hours' continuous steaming, whereas the electric energy stored up will only give us seven and a-half hours with perfect safety.

I have here allowed 8 lbs. of coal per indicated horse-power per hour, and 10 horse-power giving off 7 mechanical horse-power on the screw shaft; this is an example of an average launch engine. There are launch engines in existence which do not consume one-half that amount of fuel, but these are so few, so rare, and so expensive, that I have neglected them in this account.

Not many years ago, a steam launch carrying a seven hours' supply of fuel was considered marvelous.

Our present accumulator supplies 33,000 foot-pounds of work per pound of lead, but theoretically one pound of lead manifests an energy equal to 360,000 foot-pounds in the separation from its oxide; and in the case of iron, Prof. Osborne Reynolds told us in this place, the energy evolved by its oxidation is equiva-

lent to 1,900,000 foot-pounds per pound of metal. How nearly these limits may be approached will be the problem of the chemist; to prophesy is dangerous, whilst science and its application are advancing at this rapid rate.

Theoretically then, with our weight of fully oxidized lead, we should be able to travel for 82 hours; with the same weight of iron for 430 hours, or 18 days and nights continually, at the rate of 8 miles per hour, with one charge. Of course, these feats are quite impossible. We might as well dream of getting 5 horse-power out of a steam-engine for one pound of coal per hour.

Whilst the chemist is busy with his researches for substances and combinations which will yield great power with small quantities of material, the engineer assiduously endeavors to reconvert the chemical or electrical energy into mechanical work suitable to the various needs.

To get the maximum amount of work with a minimum amount of weight, and least dimensions combined with the necessary strength, is the province of the mechanical engineer; it is a grand and interesting study; it involves many factors; it is not, as in the steam engine and the hydraulic machine, a matter of pressures, tension and compression, centrifugal and static forces, but it comprises a still larger number of factors, all bearing a definite relation to each other.

With dynamo machines the aim has been to obtain as nearly as possible as much electrical energy out of the machine as has been put in by the prime mover, irrespective of the quantity of material employed in its construction. Dr. J. Hopkinson has not only improved upon the Edison dynamo, and obtained 94 per cent. of the powers applied in the form of electrical energy, but he got 50 horse-power out of the same quantity of iron and copper where Edison could only get 20 horse-power—and, though the efficiency of this generator is perfect, it could not be called an efficient motor, suitable for locomotion by land or water, because it is still too heavy. An efficient motor for locomotion purposes must not only give out in mechanical work as nearly as possible as much as the electrical energy put in, but it must be of small weight, because it has to propel itself along with the vehicle, and every pound weight of the motor represents so many foot-pounds of energy used in its own propulsion; thus, if a motor weighed 660 pounds, and were traveling at the rate of 50 feet per minute, against gravitation, it would expend 33,000 foot-pounds per minute in moving itself, and although this machine may give 2 horse-power, with an efficiency of 90 per cent., it would, in the case of a boat or a tram-car, be termed a wasteful machine. Here we have an all important factor which can be neglected, to a certain extent, in the dynamo as a generator, although from an economical point of view, excessive weight in the dynamo must also be carefully avoided.

The proper test for an electro-motor, therefore, is not merely its efficiency, or the quotient of the mechanical power given out, divided by the electrical energy put in, but also the number of feet it could raise its own weight in a given space of time, with a given current, or, in other words, the number of foot-pounds of work each pound weight of the motor would give out.

The Siemens' D_2 machine, as used in the launch shown in the diagram on the wall is one of the lightest and best motors; it gives 7 horse-power on the shaft, with an expenditure of 9 electrical horse-power, and it weighs 658 lbs.; its efficiency, therefore, is $\frac{7}{9}$ths, or nearly 78 per cent.; but its "co-efficient" as an engine of locomotion is 351—that is to say, each pound weight of the motor will yield 351 foot-pounds on the shaft. We could get even more than 7 horse-power out of this machine, by either running it at an excessive speed, or by using excessive currents; in both cases, however, we should shorten the life of the apparatus.

An electro-motor consists, generally, of two or more electro-magnets so arranged that they continually attract each other, and thereby convey power. As already stated, there are numerous factors, all bearing a certain relationship to each other, and particular rules which hold good in one type of machine will not always answer in another, but the general laws of electricity and magnetism must be observed in all cases. With a given energy expressed in watts, we can arrange a quantity of wire and iron to

produce a certain quantity of work; the smaller the quantity of material employed, and the larger the return for the energy put in, the greater is the total efficiency of the machine.

Powerful electro-magnets, judiciously arranged, must make powerful motors. The ease with which powerful electro-magnets can be constructed, has led many to believe that the power of an electro-motor can be increased almost infinitely, without a corresponding increase of energy spent. The strongest magnet can be produced with an exceedingly small current, if we only wind sufficient wire upon an iron core. An electro-magnet excited by a tiny battery of 10 volts, and, say, one ampere of current may be able to hold a tremendous weight in suspension, although the energy consumed amounts to only 10 watts, or less than $\frac{1}{75}$th of a horse-power; but the suspended weight produces no mechanical work. Mechanical work would only be done if we discontinued the flow of the current, in which case the said weight would drop; if the distance is sufficiently small, the magnet could, by the application of the current from the battery, raise the weight again, and if that operation is repeated many times in a minute, then we could determine the mechanical work performed. Assuming that the weight raised is 1,000 lbs., and that we could make and break the current two hundred times a minute, then the work done by the falling mass could, under no circumstances, equal $\frac{1}{75}$th of a horse-power, or 440 foot-pounds; that is, 1,000 lbs. lifted 2.27 feet high in a minute, or about one-eighth of an inch for each operation; hence the mere statical pull, or power of the magnet, does in no way tend to increase the energy furnished by the battery or generator, for the instant we wish to do work we must have motion —work being the product of mass and distance.

Large sums of money have virtually been thrown away in the endeavor to produce energy, and there are intelligent persons who to this day imagine that, by indefinitely increasing the strength of a magnet, more power may be got out of it than is put in.

Large field-magnets are advantageous, and the tendency in the manufacture of dynamo machines has been to increase the mass of iron, because with long and heavy cores and pole pieces there is a steady magnetism ensured, and therefore a steady current, since large masses of iron take a long time to magnetize and demagnetize; thus very slight irregularities in the speed of an armature are not so easily perceived. In the case of electro-motors these conditions are changed. In the first place, we assume that the current put through the coils of the magnets is continuous; and secondly, we can count upon the momentum of the armature, as well as the momentum of the driven object, to assist us over slight irregularities. With electric launches we are bound to employ a battery current, and battery currents are perfectly continuous—there are no sudden changes; it is consequently a question as to how small a mass of iron we may employ in our dynamo as a motor without sacrificing efficiency. The intensity of the magnetic field must be got by saturating the iron, and the energy being fixed, this saturation determines the limit of the weight of iron. Soft wrought iron, divided into the largest possible number of pieces, will serve our purpose best. The question of strength of materials plays also an important part. We cannot reduce the quantity and division to such a point that the rigidity and equilibrium of the whole structure is in any way endangered.

The armature, for instance, must not give way to the centrifugal forces imposed upon it, nor should the field magnets be so flexible as to yield to the statical pull of the magnetic poles. The compass of this paper does not permit of a detailed discussion of the essential points to be observed in the construction of electro-motors; a reference to the main points may, however, be useful. The designer has, first of all, to determine the most effective positions of the purely electrical and magnetic parts; secondly, compactness and simplicity in details; thirdly, easy access to such parts as are subject to wear and adjustment; and, fourthly, the cost of materials and labor. The internal resistance of the motor should be proportioned to the resistances of the generator, and the conductors leading from the generator to the receiver.

The insulation resistances must be as high as possible; the insulation can

never be too good. The motor should be made to run at that speed at which it gives the greatest power with a high efficiency, without heating to a degree which would damage the insulating material.

Before fixing a motor in its final position, it should also be tested for power with a dynamometer, and for this purpose a Prony brake answers very well.

An ammeter inserted in the circuit will show at a glance what current is passing at any particular speed, and voltmeter readings are taken at the terminals of the machine, when the same is standing still as well as when the armature is running, because the E M.F. indicated when the armature is at rest alone determines the commercial efficiency of the motor, whereas the E.M.F. developed during motion varies with the speed until it nearly reaches the E.M.F in the leads; at that point the theoretical efficiency will be highest.

Calculations are greatly facilitated, and the value of tests can be ascertained quickly, if the constant of the brake is ascertained; then it will be simply necessary to multiply the number of revolutions and the weight at the end of the lever by such a constant, and the product gives the horse-power, because, with a given Prony brake, the only variable quantities are the weight and the speed. All the observations, electrical and mechanical, are made simultaneously. The electrical horse-power put into the motor is found by the well known formulæ $C \times E \div 746$; this simple multiplication and division becomes very tedious and even laborious if many tests have to be made in quick succession, and to obviate this trouble, and prevent errors, I have constructed a horse-power diagram.

Graphic representations are of the greatest value in all comparative tests. Mr. Gisbert Kapp has recently published a useful curve in the *Electrician*, by means of which one can easily compare the power and efficiency at a glance.

The speeds are plotted as abscissæ, and the electrical work absorbed in watts divided by 746 as ordinates; then with a series-wound motor we obtain a curve. The shape of this curve depends on the type of the motor. Variation of speed is obtained by loading the brake with different weights. We begin with an excess of weight which holds the motor fast, and then a maximum current will flow through it without producing any external work. When we remove the brake altogether, the motor will run with a maximum speed, and again produce no external work, but in this case very little current will pass. Between these two extremes external work will be done, and there is a speed at which this is a maximum.

I have now to draw your attention to a new motor of my own invention, of the weight of 124 lbs., which, at 1,550 revolutions, gives 31 amperes and 61.5 volts at terminals. The mechanical horse-power is 1.37, and the co-efficient 373.

	Ohms.
Armature resistance....	.4w.
Field-magnet resistance..	.17w.
Insulation resistance....	1,500,000w.

This motor was only completed on the morning before reading the paper; it could not, therefore, be tested as to its various capacities.

We have next to consider the principle of applying the motive power to the propulsion of a launch. The propellers hitherto practically applied in steam navigation are the paddle wheel and the screw. The experience of modern steam navigation points to the exclusive use and advantage of the screw propeller where great speed of shaft is obtainable, and the electric engine is pre-eminently a high-speed engine, consequently the screw appears to be most suitable to the requirements of electric boats. By simply fixing the propeller to the prolonged motor shaft, we complete the whole system, which, when correctly made, will do its duty in perfect order, with an efficiency approaching theory to a high degree.

Whatever force may be imparted to the water by a propeller, such force can be resolved into two elements, one of which is parallel, and the other in a plane at right angles to the keel. The parallel force alone has the propelling effect; the screw, therefore, should always be so constructed that its surfaces shall be chiefly employed in driving the water in a direction parallel to the keel from stem to stern.

It is evident that a finely-pitched screw, running at a high velocity, will supply

these conditions best. With that beautiful screw lying on this table, and made by Messrs. Yarrow, 95 per cent. of efficiency has been obtained when running at a speed of over 800 revolutions per minute, that is to say, only 5 per cent. was lost in slip.

Reviewing the various points of advantage, it appears that electricity will, in times to come, be largely used for propelling launches, and, perhaps, something more than launches.

In conclusion, quoting Dr. Lardner's remarks on the subject of steam navigation of nearly fifty years ago, he said:

"Some, who, being conversant with the actual conditions of steam engineering as applied to navigation, and aware of various commercial conditions which must affect the problem, were enabled to estimate calmly and dispassionately the difficulties and drawbacks, as well as the disadvantages, of the undertaking, entertained doubts which clouded the brightness of their hopes, and warned the commercial world against the indulgence of too sanguine anticipations, of the immediate and unqualified realization of the project. They counseled caution and reserve against an improvident investment of extensive capital, in schemes which can still be only regarded as experimental, and which might prove its grave. But the voice of remonstrance was drowned amid the enthusiasm excited by the promise of an immediate practical realization of a scheme so grand.

"It cannot," he continues, "be seriously imagined that any one who had been conversant with the past history of steam navigation could entertain the least doubt of the abstract practicability of a steam vessel making the voyage between Bristol and New York. A steam vessel, having as cargo a couple of hundred tons of coals, would, *cæteris paribus*, be as capable of crossing the Atlantic as a vessel transporting the same weight of any other cargo."

Dr. Lardner is generally credited with having asserted that a steam voyage across the Atlantic was "a physical impossibility," but in the work from which I took the liberty of copying his words he denies the charge, and says that what he did affirm was, that long sea voyages could not at that time be maintained with that regularity and certainty which are indispensable to commercial success, by any revenue which could be expected from traffic alone.

The practical results are well-known to us. History repeats itself, and the next generation may put on record our weak attempts, our doubts and fears of this day. Whether electricity will ever rival steam, remains yet to be proved; We may be on the threshold of great things. The premature enthusiasm has subsided, and we enter upon the road of steady progress.

ELECTRICAL UNITS OF MEASUREMENT.

By Sir WILLIAM THOMSON, F.R.S., M. Inst. C. E.

From the Proceedings of the Institution of Civil Engineers.

In physical science a first essential step in the direction of learning any subject, is to find principles of numerical reckoning, and methods for practicably measuring, some quality connected with it. I often say that when you can measure what you are speaking about, and express it in numbers, you know something about it; but when you cannot measure it, when you can not express it in numbers, your knowledge is of a meagre and unsatisfactory kind: it may be the beginning of knowledge, but you have scarcely, in your thoughts, advanced to the stage of *science*, whatever the matter may be. I may illustrate by a case in which this first step has not been taken. The hardness of different solids, as precious stones and metals, is reckoned by a merely comparative test. Diamond cuts ruby, ruby cuts quartz, quartz, I believe, cuts glass-hard steel, and glass-hard steel cuts glass; hence, diamond is reckoned harder than ruby; ruby, than quartz; quartz, than glass-hard steel; and glass-hard steel than glass; but we have no nu-

merical measure of the hardness of these, or of any other solids. We have, indeed, no knowledge of the moduluses of rigidity, or of the tensile strength, of almost any of the gems or minerals, of which the hardness is reckoned by mineralogists in their comparative scale, beginning with diamond, the hardest of known solids. We have even no reason to believe that the modulus of rigidity of diamond is greater than that of other solids; and we have no exact understanding of what this property of hardness is, nor of how it is related to moduluses of elasticity, or to tensile or shearing strength, or to the quality of the substance in respect to its bearing stresses exceeding the limit of its elasticity. It must, therefore, be admitted that the science of strength of materials, so all-important in engineering, is but little advanced; and the part of it relating to the so-called hardness of different solids least of all; there being in it no step toward quantitative measurement or reckoning in terms of a definite unit.

A similar confession might have been made regarding electric science, as studied even in the chief physical laboratories of the world ten years ago. True, Cavendish and Coulomb, last century, and Ampere and Poisson, and Green, and Gauss, and Weber, and Ohm, and Lentz, and Faraday, and Joule, this century, had given us the mathematical and experimental foundation for a complete system of numerical reckoning in electricity and magnetism, in electro-chemistry, and in electro-thermodynamics; and as early as 1858 a practical beginning of definite electric measurement had been made, in the testing of copper resistances, insulation resistances, and electro-static inductive capacities, of submarine cables. But fifteen years passed after this beginning was made, and resistance coils and ohms, and standard condensers and micro-farads, had been for ten years familiar to the electricians of the submarine-cable factories and testing stations, before anything that could be called electric measurement had come to be regularly practiced in most of the scientific laboratories of the world. I doubt whether ten years ago a single scientific instrument maker or seller could have told his customers whether the specific conductivity of his galvanometer coils was anything within 60 per cent. of that of pure copper; and I doubt whether the resistances of one in a hundred of the coils of electro-magnets, galvanometers, and other electro-magnetic apparatus, in the universities, and laboratories, and lecture establishments of the world, were known to the learned professors whose duty it was to explain their properties, and to teach their use to students and pupils. But we have changed all that; and now we know the resistances of our electro-magnetic coils, generally speaking, better than we know their lengths, and our least advanced students in physical laboratories are quite able to measure resistances through a somewhat wide range with considerable accuracy. I should think, indeed, that with the appliances in ordinary use they are more likely to measure resistances of from 100 to 10,000 ohms to an accuracy of $\frac{1}{10}$ per cent. than they are to be right to 1 millimeter in a meter in their measurements of length. It certainly is a very surprising result, that in such a recondite phenomenon—such a subtle quality to deal with—as electric resistance, which is so very difficult to define, and which we are going to learn is a velocity, every clerk in a telegraph station, the junior students and assistants in laboratories, and even workmen in electric lighting establishments, are perfectly ready to measure, more accurately than you would measure the length of 10 feet of wire, the resistance of electric conductors in definite absolute units.

I suppose, too, that nearly every apparatus room and physical laboratory possesses a micro-farad, but I am afraid its pedigree is not often known; and if its accuracy within 10 per cent. were challenged, I doubt whether, in many cases, any one, whether maker or possessor, or other electrical expert, could be found to defend it. As for our electrostatic apparatus, I confess that I do not know the capacity of a single one of the two or three dozen Leyden jars which, in 1846, I inherited in the Natural Philosophy apparatus room of the University of Glasgow, or which I have made from time to time during the thirty-seven years passed since that date. I would fain hope that I am singular in such a confession, and that no other professor of natural phil-

osophy in the world would let a Leyden jar be put on his lecture-room table without being able to tell his students its capacity in absolute measure. The reckoning of Leyden jar capacity in square inches of coated glass—thickness and specific inductive capacity not stated—ought to be as much a thing of the past as is the reckoning of resistances in terms of a mile of wire—weighing 14 grains to the foot, of ordinary commercial copper, specific resistance not stated (perhaps 45 per cent.? or 70 per cent.? or 98 per cent? of the conductivity of pure copper). And as to practical measurement of electro-motive force, we have scarcely emerged one year from those middle ages when a volt and a Daniell's cell were considered practically identical—to the higher aspiration of measurement within 1 per cent. It seems, indeed, as if the commercial requirements of the application of electricity to lighting, and other uses of every-day life, were destined to cause an advance of the practical science of electric measurement, not less important and valuable in the higher region of scientific investigation, than that which, from twenty to thirty years ago, was brought about by the practical requirements of submarine telegraphy.

There cannot be a greater mistake than that of looking superciliously upon practical applications of science. The life and soul of science is its practical application, and just as the great advances in mathematics have been made through the desire of discovering the solution of problems which were of a highly practical kind in mathematical science, so in physical science many of the greatest advances that have been made, from the beginning of the world to the present time, have been made in the earnest desire to turn the knowledge of the properties of matter to some purpose useful to mankind.

The first step toward numerical reckoning of properties of matter more advanced than the mere reference to a set of numbered standards as in the mineralogist's scale of hardness, or to an arbitrary trade standard, as in the Birmingham wire gauge, is the discovery of a continuously-varying action of some kind, and the means of observing it definitely, and measuring it in terms of some arbitrary unit or scale division. But more is necessary to complete the science of measurement in any department; and that is the fixing on something absolutely definite as the unit of reckoning, which, with reference to electric and magnetic science, is the subject of my lecture of this evening.

In electricity the mathematical theory and the measurements of Cavendish, and in magnetism, the measurements of Coulomb gave, one hundred years ago, the requisite foundation for a complete system of measurement: and fifty years ago the same thing was done for electro-magnetism by Ampere.

I speak of electricity, of magnetism, and of electro-magnetism. Now, I must premise, as a matter of importance in respect of some of the technical details which we shall have to consider a little later, that magnetism must be held to include electro-magnetism. Electro-magnetism and magnetism are one and the same thing. Electro-magnetic and electro-static force, which are very distinct just now, are two things which deeper science may lead us to unite, in a manner that we can scarcely see at present. We have the foundation in the last century of Cavendish for electricity, and of Coulomb for magnetism, which falls in perfectly with what I shall have to say a little later on in respect of Gauss and Weber's work, of magnetism and electro-magetism. I say this, because there has been some little discussion in respect to the magnetic unit and the electro-magnetic unit, as if the magnetic unit might be something different from the electro-magnetic unit, or the electro-kinetic unit. It will simplify matters if we think merely of a magnetic force, whether it be due to a steel magnet, or to a wire conveying a current, and make no distinction so far as measurement is concerned, through the range of the science of magnetism, including electro-magnetism. We shall find that we have the two capital subjects: electricity and electro-static force, one of them; magnetism and electricity and motion through conductors, and magnetic and electro-magnetic force, the other. The first complete method of scientific measurement for any of these subjects was that of Gauss, in his system of absolute measurement for terrestrial magnetism so splendidly realized by Gauss and Weber in their Magnetic So-

ciety of Göttingen, which gave the starting impulse for the whole system of absolute measurement as we now have it, throughout the range of electric science. In fact, Weber himself, after realizing absolute measure in terrestrial magnetism in conjunction with Gauss, carried it on through the field of electro-magnetism in his "Elektrodynamische Maasbestimmungen," and thence into electro-statics in his joint work with Kohlrausch, under the same title, "Elektrodynamische Maasbestimmungen." The now celebrated "v" (velocity) which Maxwell, in his electro-magnetic theory of light, pointed out to be not merely by chance approximately equal to the velocity of light, but to be probably connected physically, in virtue of the forces concerned, with the actual action or motion of matter which constitutes light, was found to be approximately 300,000 kilometers per second.

As early as 1851 I commenced using the absolute system in the reckoning of electromotive forces of voltaic cells, and the electric resistances of conductors, in absolute electro-magnetic units; and after advocating the general use of the absolute system, both for scientific investigation and for telegraph work, for ten years, I obtained in 1861 the appointment of a Committee of the British Association on Electrical Standards.

This committee worked for nearly another ten years through the whole field of electro-magnetic and electro-static measurement, but chiefly on standards of electric resistance, until in its final report, presented to the Exeter meeting in August, 1869, it fairly launched the absolute system for general use; with arrangements for the supply of standards for resistance coils in terms of a unit, first called the British Association unit, and afterwards the Ohm; of which the resistance reckoned in electro-magnetic measure was to be, as nearly as possible, 10,000 kilometers per second.

In regard to the name of "ohm," I may mention that a paper was communicated to the British Association in 1861 by Sir Charles Bright and Mr. Latimer Clark, in which the names that we now have, with some slight differences, were suggested; and a complete continuous system of measurement was proposed, which did not fulfill certainly all the conditions of the absolute system, but which fulfilled some of them in an exceedingly useful manner for practical purposes. To Sir Charles Bright and Mr. Latimer Clark, therefore, is due the whole system of names as we have it now, ohms, volts, farads, and micro-farads. From 1870 or 1871 forward, the absolute system, with the approach to accurate realization of it given by the British Association unit, has been in general use in England and America; but another decade has passed, a rather long one, before the definitive practical adoption of the absolute system by France, Germany and other European countries, as decreed by the International Conference, for the determination of Electric Units, held at Paris in October 1882. The decision adopted was, not to take the British Association unit. Doubt was thrown upon its accuracy, which we shall see was well founded. The question of a strict foundation for a metrical system was before the conference, and it was inclined to adopt the absolute system, but the question occurred, "What is the ohm?" Who can see an ohm? Who can show what an ohm is? Who can measure the resistance of any conductor for us, in this absolute measure of Weber's? Weber's own measurement differed greatly from that of the British Association. Several experimenters, in endeavoring to verify or test the British Association measurement, arrived at results which were discordant among themselves, and therefore could not be confirmatory of the British Association measurement. Things were in this doubtful state, and the conference had a very important practical question to decide. A proposal had been before the world for ten years at least, to found accurate measurement of electrical resistance upon a material obtainable in uniform quality and by easy precautions in a state of perfect purity, or sufficiently nearly perfect to fit it practically for the purpose in question, which is—the giving of a standard for the measurement of resistance. The Siemens unit, founded upon the specific resistance of mercury, had been proposed. The great house of Siemens (Berlin and London) our distinguished *confrere*, Sir William Siemens, and his distinguished brother, Dr. Werner Siemens, worked upon this subject in the most thorough and powerful way—the measurement of resistances in terms of the

specific resistances of mercury—in such a manner as to give us a standard which shall be reproducible at any time and place, with no other instrument of measurement at hand than the meter measure. I say, the system of measurement of resistance on a mercury standard had been worked out, and its practicability demonstrated. Werner and William Siemens themselves were both present at the conference, and they joined heartily in the proposal to adopt an absolute system, but the question was how to make a beginning; and the answer adopted by the conference was to ask for a definition of an absolute system in terms of a column of mercury. The column of mercury was the one standard in existence, that could be reproduced otherwise than by merely copying from one wire to another; and it was naturally adopted as the foundation upon which a standard, if not a practical unit to be used, should be founded. In short, then, the finding of the conference was to this effect: that as soon as good evidence is given of a sufficiently near measurement for practical purposes, of the resistance of any conductor—be it a piece of wire or a column of mercury—as soon as such measurement should be made, with evidence that it is accurate enough for practical purposes, then the unit which the British Association had aimed at should be adopted; but it was to be left to the judgment and the convenience of the users of standards when to make the change, should a change be necessary, from the British Association unit as the ohm, or from the Siemens unit, to bring measurement into more close agreement with the absolute reckoning. What had been done by Lord Rayleigh and Mrs. Sidgwick, had left very little room for doubt but that the British Association unit was in error to the extent of 1.3 per cent. The Siemens unit had the advantage of being somewhat approximately equal to the desired absolute unit, though not professing to be an absolute unit at all. It was simply the resistance of a column of mercury at zero temperature, a meter in length and a square millimeter in section. There were great difficulties in the reproduction of the Siemens unit, in the earlier times of the investigation; but Dr. Werner Siemens, and Lord Rayleigh, and Mrs. Sidgwick, and many other workers besides, all working to compare with the British Association unit, obtained results which finally left no doubt whatever as to the true relation. Dr. Werner Siemens' result found the mercury unit to be 0.9536 of the British Association unit; Lord Rayleigh and Mrs. Sidgwick found it 0.9542, which is an exceedingly close agreement, being within $\frac{1}{10}$ per cent. of the result of Dr. Werner Siemens. A result differing by nearly 1 per cent. had been obtained by Mathiessen and Hockin a good many years before, when the precautions necessary to reproduce the mercury standard with absolute accuracy, were not so well known as, in the course of a few years after their work, they came to be known. The final conclusion of Lord Rayleigh's work was, that the Siemens mercury unit is 0.9413, of what the conference at Paris agreed to define as the ohm; and that is the resistance measured by 1,000,000,000 centimeters per second. I am afraid that conveys a strange idea, but it is perfectly true as to the absolutely definite meaning of resistance. I shall have occasion to refer to the subject later, when I hope to explain this mysterious velocity of 10_9 centimeters per second. In the course of the thirty years from the time when telegraphy began to demand definite measurement, a great deal of accurate measurement in terms of variously defined units of resistance had been made. Many sets of resistance coils had been produced by the Varley brothers, and other instrument makers, and many scientific investigators in laboratories had produced standards, and sets of resistance coils were made according to those standards; but within the last twelve years all have merged into, either the Siemens, or the British Association unit. The British Association unit, as I have said, was an attempt at absolute measurement, which succeeded in coming within 1.3 per cent. of the 10_9 aimed at. *Copies* of the British Association unit were *accurate to $\frac{1}{10}$ per cent.* The Siemens unit was founded on another idea, but it gave results no less definite and no less convenient for a great multitude of practical applications, than did the somewhat nearer approach to a convenient absolute unit realized by the British Association Committee.

Gauss' principle of absolute measure-

ment for magnetism and electricity, is merely an extension of the astronomer's method of reckoning mass in terms of what we may call the universal-gravitation unit of matter; and of the reckoning of force adopted by astronomers, in common with all workers in mathematical dynamics, according to which the unit of force is that force, which, acting on unit of mass for unit of time, generates a velocity equal to unit of velocity. The universal-gravitation unit of mass is such a quantity of matter, that if two quantities, each equal to it, be placed at unit distance apart, the force between them is unity.

The universal-gravitation method I refer to for this reason. There is a terrestrial-gravitation reckoning of force, according to the weight of the unit of mass; and after all, when we terrestrial creatures take a mass in our hand and feel the weight of it, it is a kind of measurement that we cannot do away with. The kilogramme, or the pound, or the ounce, is a thing we have to deal with; we have it in our hand, and we cannot help using it to give us by its *heaviness* a reckoning of force. A local gravitation unit of force means the weight of a gramme in London, in Glasgow, at the Equator, or anywhere else; and it is a convenient unit; but the common mode of measuring force by reference to weight without reference to locality is not definite, because the weight of a gramme is different here from what it is at the Equator. The heaviness of a pound or a gramme is greater by a two-hundredth at either pole than at the Equator; or to give the exact figures, 0.00512. That is a difference of $\frac{1}{2}$ per cent., and if your accuracy is to be within a $\frac{1}{2}$ per cent., you cannot ignore the difference of the force of gravity in different places. But a vast number of measurements in engineering, and in teh most ultra scientificwork of scientific laboratories, does not aspire to so high a degree of accuracy; and for all such work the local or terrestrial-gravitation unit suffices, without specifying what the particular place is—only that it is somewhere or other on the face of the earth. For instance, moduluses of rigidity, moduluses of rupture, breaking strains of material, are stated accurately enough for engineering purposes, in terms of a ton weight per square centimeter, or pounds weight per square centimeter, or any other such mode of reckoning; or if I had not vowed never to mention inches, I would say tons per square inch, which is common (perhaps too common) in engineering. All such measurements ignore the difference of gravity in different localities, except some more precise measurements, in which an allowance for the force of gravity to reduce it to a standard of lat. 45° is made, or it is left to the person using the measurement to make the reduction. For all purposes, however, in whieh it would be desirable to apply a correction for the varying force of gravity in different places, it is convenient to use Gauss' absolute unit, and not the terrestrial-gravitation unit of force. I may say in passing, that the mere idea, which lurked or was visibly manifested, according to the degree of understanding, in the old formula of elementary dynamics $F=m\frac{dv}{dt}$, was an immense step; and the realization of that idea, the bringing of it into practical use, has contributed more than anything else I know, to the intelligent treatment of the dynamic problems and their applications to both scientific and engineering matters. The system of absolute reckoning of force by Gauss cannot be too much commended, as a great and important practical improvement in the fundamental science of engineering and physics, the science of dynamics. It consists simply in defining the unit of force as that force which, acting on a unit of mass for a unit of time, generates a velocity equal to the unit of velocity. It leaves the units of mass, space, and time to be assumed arbitrarily; the gramme, the centimeter, and the mean solar second, for example, as in the now generally adopted "C. G. S." system.

But the universal-gravitation system of the dynamical astronomer defines the unit of mass in terms of the unit of space and the unit of force. I need not repeat the definition. Thus we have the interlocking of two definitions: the unit of force defined in terms of the units of mass, space, and time; the unit of mass defined in terms of the unit of force and the unit of space. It might seem as if we were proceeding in a vicious circle; but the circle is not vicious—the two definitions are logically and clearly inter-

dependent. We have, as it were, two unknown quantities and two equations; and the elimination of one of the unknown quantities from the two equations, gives us the other explicitly. The two are mixed up in a somewhat embarrassing way in the primitive definitions, but when we disentagle them, we arrive at the simple result, which I shall state presently, of independent definitions of the unit of mass and the unit of force, each in terms of units of space and time chosen arbitrarily.

Though the units of force and mass thus defined are essentially implied in all the regular formulas of physical astronomy from those most elementary ones, which appear in the treatment of the undisturbed elliptic motion, according to Newton's inferences from Kepler's laws, up to the most elaborate working out of the lunar, planetary, and cometary theories, and the precession and nutation of the earth's axis; it has not been usual for physical astronomers to found any systematic numerical reckoning upon them nor even to choose arbitrarily and definitely any particular units of length and time on which to found the units of force and mass. It is, nevertheless, interesting, not only in respect to the ultimate philosophy of metrical systems, but also as full of suggestions regarding the properties of matter to work out in detail the idea of founding the measurements of mass and force on no other foundation than the measurement of space and time. In doing so we immediately find that the square of an angular velocity, is the proper measure of density or mass per unit volume; and that the fourth power of a linear velocity is the proper measure of a force. The first of these statements is readily understood by referring to Clerk Maxwell's suggestion, of taking the period of revolution of a satellite revolving in a circle close to the surface of a fixed globe of density equal to the maximum density of water, as a fundamental unit for the reckoning of time. Modify this by the independent adoption of a unit of time, and we have in it the foundation of a measurement of density, with the detail that the density of the globe is equal to $3/(4\pi)$ of the square of the satellite's angular velocity in radius* per second; that is, the square of the satellite's velocity, multiplied by 3 and divided by 4π, measures the density of the globe. It may be a hard idea to accept, but the harder it is the more it is worth thinking of, and the more instructive in regard to the properties of matter. There it is, explain it how you will, that the density of water, the density of brass, the mean density of the earth, is measured absolutely in terms of the square of an angular velocity. I do not know whether it is generally known that to Fourier are due those dimensional equations that appear in the British Association's volume of reports, and in Clerk Maxwell's book, and in Everett's useful book, "Units and Physical Constants." The dimension for the reckoning of density is the square of an angular velocity on the universal gravitation absolute system, and is therefore T^{-2}. Equally puzzling and curious is a velocity to the fourth power for the reckoning of force, which we have next to consider.

The universal gravitation reckoning of force, which we shall see is by the fourth power of a linear velocity, may be explained as follows: Find the velocity with which a particle of matter must be projected to revolve in a circle around an equal particle fixed at such a distance from as to attract it with a force equal to the given force. The fourth power of this velocity is the number which measures the force. Sixteen times the force will give double the velocity; eighty-one times the force will give three times the velocity, and so on.

Now, if I were to say that the weight of that piece of chalk is the fourth power of twenty miles an hour, I should be considered fit, not for this place, but for a place where people who have lost their senses are taken care of. I suppose almost every one present would think it simple idiocy if I were to say that the weight of that piece of chalk is the fourth power of seven or eight yards per hour, yet it would be perfectly good sense.

* The radian is the unit in which angular velocity is expressed. It is an angle of $\left(\frac{180^\circ}{\pi}\right)$ about 57°.3 (or more correctly 56°.2958). Thus an arm, or radius vector turning through an angle of about 57°.3 per second, is moving with unit angular velocity: or if the arm makes a complete circle in one second its angular velocity is 2π.

Think now of an infinitesimal satellite revolving around the earth—you ask, what is an infinitesimal satellite? To be "infinitesimal" for our present purpose it must be very small in comparison with the earth, so as not to cause sensible motion by its reaction on the earth. Well, a 500-lb. shot is an infinitesimal satellite, though it is not, perhaps, infinitesimal in some of its aspects. There must be no resistance of the air, of course. Now fire it off with such a velocity that it will have a very flat trajectory, neither more nor less flat than the earth, and it will continue going round and round the earth. Find the velocity at which you must fire off the shot to make it go round the earth, and, if there is no resistance of the air, there is our infinitesimal satellite. These somewhat pedantic words are justified, because "infinitesimal satellite" is nine syllables to express three or four sentences; that is our justification.

The semi-period of an infinitesimal satellite revolving round the earth, close to its surface, is equal to the semi-period of an ideal simple pendulum of length equal to the earth's radius, and having its weighted end infinitely near to its surface; and, therefore, when reckoned in seconds, is approximately equal to the square root of the number of meters (6,370,000) in the earth's radius; because the length of a seconds pendulum (or the pendulum whose semi-period is a second) is very approximately 1 meter. Thus we find 2,524 mean solar seconds for the semi-period of the satellite, and its angular velocity in radians per second is, therefore $(\pi/2524=)0.001244$; hence the earth's mean density, reckoned on the universal-gravitation system, with the mean solar second for the unit of time, is $[(0.001244)^2\times 3/(4\pi)=]3.70\times10^{-7}$; and, if we take (from Bailey's repetition of Cavendish's experiment)[2] the earth's mean density as 5.67 times the maximum density of water, we find 6.53×10^{-8} for the maximum density of water according to the universal gravitation reckoning. To measure mass we must now introduce a unit of length, and if we take this as 1 centimeter, we find that as the mass of a cubic centimeter of water at maximum density is very approximately equal to what is called a gramme, the universal gravitation unit of matter is $[1/(6.53\times10^{-8})=]15.3\times10^{6}$ grammes, or 15.3 French tons; hence, the unit force on the universal-gravitation system is 15.3×10^{6} dynes, or 15.6 times the terrestrial weight of a kilogramme.

15.3 French tons, then (a French ton is 1.4 per cent. less than the British ton), is the universal-gravitation unit of matter. The time may come when the universal-gravitation system will be the system of reckoning; when 15.3 tons will be the unit of matter, and when the decimal subdivision of 15.3 French tons may be our metrical system, and grammes may be as much a thing of the past as grains are now.

There is something exceedingly interesting in seeing that we can practically found a metrical system on a unit of length and a unit of time. There is nothing new in it, since it has been known from the time of Newton, but it is still a subject full of fresh interest. The very thought of such a thing is full of many lessons in science that have scarcely yet been realized, especially as to the ultimate properties of matter. The gramme, it will be remembered, is founded on the properties of a certain body, namely, water; but here, without invoking any particular kind of matter, simply choosing a certain definite length marked on a measuring rod, and a unit of time (how obtained we shall consider presently), we can take up a piece of matter and tell, in any part of the universe, how to measure its mass in definite absolute units.

Think now of the two units on which this universal-gravitation metrical system depends, the unit of length and the unit of time. The unit of length is merely the length of a certain definite piece of brass, or other solid substance used for a measuring rod, or the length between two marks upon it; it may be an inch, or a foot, or a yard, or a meter, or a centimeter, the principle is the same. The meter, it is true, was made originally as nearly as possible equal to the ten-millionth of the length of a certain quadrant of the earth, estimated as accurately as possible from the geodetic operations of MM. Méchain and Delambre, in 1792, performed for the foundation of the metrical system. But this merely gave the original meter measure, and what is meant by the meter now is a length equal to it, or to some authentic

copy of it, which has been made from it as accurately as possible; and the one-hundredth part of the meter thus defined is the centimeter which we definitively adopt as the unit of length.

Thus our unit of length is independent of the earth, and is perfectly portable, so that the scientific traveler, roaming over the universe, carries his measuring rod with him, and need think no more of the earth so far as his measurement of space is concerned. But how about the mean solar second, in terms of which he measures his time? What of it if he has left the earth for good; or if, even without leaving the earth, he carries on his scientific work on the earth through a few million years, in the course of which the period of the earth's rotation around its axis, and its revolution around the sun, will both be very different from what they are now? If he takes a good watch or chronometer with him, well rated before he leaves the earth, it will serve his purpose as long as it lasts. What it does is merely to count the vibrations of a certain mass under the influence of a certain spring (the balance wheel under the influence of the hair spring). If, for any secular experiment he has in hand, he wishes to keep up a continuous reckoning of time, he must keep his watch always going, and not a vibration will be lost in the counting performed by the hands. But if he merely wishes to keep his unit of time, and to make quite sure that any number of million years hence, this shall be within one-tenth per cent. of its present value, he should take a vibrator better arranged for permanence and for absolute accuracy than the balance wheel with its hair spring of a watch or a chronometer. A steel tuning-fork, which has had its period of vibration determined for him before he leaves the earth, by Prof. Macleod or by Lord Rayleigh, will serve his purpose. By measuring the period in terms of mean solar seconds, with the prongs up, and horizontal, and vertically down, he will be able to eliminate the slight effect of terrestrial gravity; and he will have with him a time standard that will give him the mean solar second, as accurately as his measuring rod gives him the centimeter, in whatever part of the universe, and at whatever time, now or millions of years later, he has occasion to use his instruments.

I hope that you will not feel that I am abusing your good nature with an elaborate frivolity when I ask you to think a little more of the unital equipment of our ideal traveler on a scientific tour through the universe. For myself, what seems the shortest and surest way to reach the philosophy of measurement—an understanding of what we mean by measurement, and which is essential to the intelligent practice of the mere art of measuring—is to cut off all connection with the earth, and think what we must then do to make measurements which shall be definitely comparable with those which we now actually make, in our terrestrial workshops and laboratories. Suppose, then, the traveler to have lost his watch and his tuning-fork and his measuring rod, but to have kept his scientific books, or at all events to have in his mind a full recollection and understanding of their contents: how is he to recover his centimeter, and his mean solar second?

Let us consider the recovery of the centimeter first. Wherever he is let him make a piece of glass, like this which I hold in my hand, out of materials which he is sure to find, in whatever habitable region of the universe he may chance to be; and let him with a diamond, or with a piece of hard steel, or with a piece of flint, engrave on it one thousand equidistant parallel lines, upon a space which may be about the breadth of his thumb, and which he may take as a temporary or provisional unit of length. He may help himself to engrave the glass by means of a screw cut in brass or steel, which he will easily make, though he has no tools, not even flint implements, to begin with. With a little time and perseverance he will make the requisite tools. Let him also make a temporary measuring rod, and mark off equal divisions upon it, which may be of any convenient length, and need not have any relation to the definite provisional unit. Let him now make two candles, and light them and place them as you now see those on the table, at any convenient distance apart, measured on his measuring rod. He holds the piece of ruled glass in his hand, close to his eyes, as I hold this, and sees two rows of colored spectrums, each with one of the candles in its center. He turns the glass around till the two rows of spectrums are in the same line, and

adjusts the parallelism of its plane, so as to make the distance from spectrum to spectrum a minimum. He moves backwards and forwards as I do now, keeping his eye at equal distances from the two candles, until he sees each candle shooting up out of the yellow middle of a spectrum of the other candle, with no spectrum between the two candles. With this condition fulfilled he measures the distance from the grating to the candles. Then, by the theory of diffraction he has the proportion: as the distance from the grating to the candles is to the distance between the candles, so is the distance from center to center of the divisions on the glass, to the wave length of yellow light. This, he remembers, is 5.892×10^{-5} of a centimeter, and thus he finds the value in centimeters of his provisional unit.

[How easily this determination might be effected, supposing the grating once made, was illustrated by a rapid experiment performed in the course of the lecture; without other apparatus than a little piece of glass with two hundred and fifty fine parallel lines engraved on it, two candles, and a measuring tape of unknown divisions of length (used only to measure the ratio between two distances). The result showed the distance from center to center of consecutive bars of the grating, to be 32 times the wave-length of yellow light. The breadth of the span on which the two hundred and fifty lines of grating were ruled, was thus measured as ($250 \times 32 \times 5.892 \times 10^{-5} =$) 0.47136 centimeter. According to the instrument maker this space was said to be 0.5 of a centimeter.]

Thus you see, by this hurried experiment with this rough-and-ready apparatus, we have been able to measure a length to within a small percentage of accuracy. A few minutes longer spent upon the experiment, and using sodium flames behind fine slits instead of open candles blowing about in the air, with more careful measurement of the ratio of the distances, might easily have given a result within one-half per cent. of accuracy. Thus the cosmic traveler can easily recover his centimeter and his meter measure.

But how is our scientific traveler to recover his mean solar second, supposing he has lost his tuning-fork? He may think of the velocity of light, and go through Foucault's experiment. That is a thing that can be done from the beginning, with nothing but cutting tools and pieces of metal to begin with. Let him get a piece of brass and make a wheel, and cut it to two thousand teeth. I do not know how many teeth Foucault used, but our traveler can go through the whole process, and set the wheel revolving at some uniform rate (not a known rate, because he has no reckoning of time): and he will tell what the velocity of the wheel is in terms of the velocity of light, which is known to be about 300,000 kilometers per second. If he is electrically minded, as this evening we are bound to suppose our scientific traveler to be, he will think of "v" or of an ohm. He may make a Siemens unit; that he can do, because he has his centimeter, and he finds mercury and glass everywhere. Then he goes through all that Lord Rayleigh and Mrs Sidgwick have done. He will, with a temporary chronometer or vibrator, obtain a provisional reckoning of time, and he will go through the whole process of measuring the resistance of a Siemens unit in absolute measure, according to his provisional unit of time. His measurement gives him a velocity in, let us say, kilometers per this provisional unit of time, as the value of the Siemens unit in absolute measure. Then he knows from Lord Rayleigh and Mrs. Sidgwick, that the Siemens unit in absolute measure is 9,413 kilometers per mean solar second; and thus he finds the precise ratio of his provisional unit of time to the mean solar second.

Still, even though this method might be chosen as the readiest and most accurate, according to present knowledge of the fundamental data, for recovering the mean solar second, the method by "v" is too interesting and too instructive, in respect to elimination of the properties of matter from our ultimate metrical foundations, to be unconsidered. One very simple way of experimentally determining "v," is derivable from an important suggestion of Clark and Bright's paper, referred to above. Take a Leyden jar, or other condenser of moderate capacity (for example, in electro-static measure, about 1,000 centimeters), which must be accurately measured. Arrange a mechanism to charge it to an accurately measured potential of

moderate amount (for example, in electro-static measure, about 10 c.g.s., which is about 3,000 volts), and discharge it through a galvanometer coil at frequent regular intervals (for example, ten times per any convenient unit of time). This will give an intermittent current of known average strength (in the example, 10^6 electro-static c.g.s., or about 1/300,000 c.g.s. electro-magnetic, or 1/30,000 of an ampere), which is to be measured in electro-magnetic measure by an ordinary galvanometer. The number found by dividing the electro-static reckoning of the current, by the experimentally found electro-magnetic reckoning of the same, is "*v*," in centimeters per the arbitrary unit of time, which the experimenter in search of the mean solar second has used in his electro-static and electro-magnetic details. The unit of mass which he has chosen, also arbitrarily, disappears from the resulting ratio.

But there is another exceedingly interesting way—a way which, although I do not say it is the most practical, has very great interest attached to it, as being a way of doing the thing in one process—that is, by the method of electrical oscillations. I should certainly like to see how a person who has lost his standards, after having recovered his centimeter (which he certainly would do by the wave-length of light), would succeed in recovering his unit of time by the following method. Take a condenser—a very large Leyden jar; electrify it, and connect the two poles through a conductor, arranged to have as large an electro-magnetic *quasi* inertia, electro-magnetic self-induction—as possible. The method is given in Clerk Maxwell's "Electricity and Magnetism" (vol. ii. chap. xix.): it is too long to explain the details. Read the mathematical parts of Clerk Maxwell, read the British Association volume of Reports on Electrical Standards, and read Everett's "Units and Physical Constants"; get these off by heart from the first word to the last, and you will learn with far less labor than by listening to me. Take a resistance coil of proper form for maximum electro-magnetic inertia, and discharge the condenser through it; or rather start the condenser to discharge through such a coil, and you will have a set of oscillations, following exactly the same law as the oscillations of the water-level in two cisterns, which, having initially had the free water-level in one higher than in the other, are suddenly connected by a U-tube. Imagine two cisterns of water, connected by a U-tube with a stop-cock, and having the water higher in one cistern than in the other; now suddenly open the stop-cock, and the water-level will begin to fall in one cistern, and rise in the other. The inertia of the water, thus made to flow through the connecting U-tube, will cause it to flow on after it has come to its mean level in the two cisterns, and rise to a higher level in the one in which it was previously higher, and sink to a correspondingly low level in the other. Thus the water-level in each cistern would alternately be above and below the mean free level: the range of motion being gradually diminished, in virtue of the viscosity of the water, until after a dozen or two of oscillations, the amplitude of each becomes so small that you cannot notice it. Precisely the same thing happens in the case of the discharge of a condenser through a resistance coil of large electro-magnetic inertia: the resistance of the copper wire being like the viscous influence which causes the oscillations of water to subside. If, in his investigations throughout the universe, our traveler could meet with a metal which is about a million times as conductive as copper, he would make this experiment with much greater ease; but it is practicable with copper. It is certain from the observations made by Feddersen, Schiller, and others, that a great number of oscillations can be observed, and that the period, or semi-period of oscillation, can be determined with considerable accuracy.

If our scientific traveler wishes, by this beautiful experiment, once for all to determine his time reckoning, let him proceed thus. Let him take a coil, of which he knows the dimensions perfectly, having already gone through the preliminary process of measuring its electrical dimensions; or if he cannot measure these with sufficient accuracy (and there is enormous difficulty in finding the electric dimensional qualities of a coil by measurement), let him do it partly by direct measurement of its length and of linear dimensions of the figure into which it is wound, and partly by comparing it electro-magnetically with other coils. By

an elaborate investigation he can find the electro-magnetic inertia of the coil in terms of his centimeter. And here, again, there is a curious kind of puzzle and apparent incongruity, when I say that the electro-magnetic inertia equivalent of a coil, is a length, and is measured as a numeric of centimeters. Let him make a condenser, and by building it up from small to large, let him learn the capacity of it in electro-static measure. Let him begin with two plates or cylinders, or a sphere enclosed within concentric sphere, and go on multiplying till he gets a capacious enough condenser of which he knows, in electro-static measure, the electro-static capacity. This, again, is a line. Now let him take the rectangle of those two lines, and construct the equivalent square—let him, geometrically or arithmetically, take the square root of the product of the two lines—and let him observe the period of electric oscillation that I have spoken of. Let him imagine the hand of a watch, going once round in the observed period. He has good magnetic eyes, and he sees the electro-magnetic oscillation, or he has appliances by which he can test it: the thing has been done. He sets in motion a little piece of wheel-work, with a hand going once round in the period of the oscillation. Now, for a moment, let him imagine that hand to be equal in length to the square root of the product of those two lines—several million centimeters, or several thousand kilometers, if the coil and condenser are of dimensions convenient for the actual experiment, as we terrestrials might do it. The velocity of the end of that hand is "v." There he has this wonderful quantity "v." He has a hand going round in a certain time, and he knows that if that hand be of the calculated length, the velocity of the end of it is "v." This is interesting and instructive, and though I do not for certain know that it is very practicable, it is still, I believe, sufficiently so to be worth thinking of. I think it will be one of the ways of determining this marvelous quantity "v."

It is to be hoped that before long "v" will be known, in centimeters per mean solar second, within 1/10 per cent. At present it is only known that it does not *probably* differ 3 per cent. from 2.9×10^{10} centimeters per mean solar second. When it is known with satisfactory accuracy, an experimenter provided with a centimeter measure may, anywhere in the universe, rate his experimental chronometer to mean solar seconds, by the mere electro-static and electro-magnetic operations described above, without any reference to the sun or other natural chronometer.

I have tried your patience, I fear, too long, but I have now only reached the threshold of my subject. We now must commence the consideration of electrical units of measurement. I need not go round defining quantities electro-statically and electro-magnetically; you will find it all in Everett, and in the British Association volume of collected Reports by the first Committee on Electric Measurement. It is not for me to tell you of an ohm, a volt, a micro-farad, and so on; but there are two or three points that I should like to notice, and one is, the limitation of the so-called practical system. The absolute system goes from beginning to end in a perfectly consistent manner, with the initial conditions carried out all through; one of which, in the electro-magnetic system, is that the electromotive force produced by the motion at unit speed, across the lines of force of a field of unit intensity, of a unit length of conductor, is unity. That you must carry out if the system is to be complete and consistent, and the dimensions of all your instruments and apparatus must all be all reckoned uniformly in terms of the unit of length adopted in the absolute definition. The ohm is 1,000,000,000 centimeters, or 10,000 kilometers, per second. If we are to make the ohm an absolute electro-magnetic unit with the second as the unit of time, we must take the earth's quadrant as the unit of length. If we take that consistently throughout, we need never leave this particular system, and we need have nothing to do with C. G. S. We should have the Q. G. S. system, pure and simple! But it would be obviously inconvenient to measure the dimensions of instruments, the diameters of wheels, and the gauges of wire in submultiples of the earth's quadrant. Imagine the horror of a practical workman, on hearing a scientific person say to him, "Give me a wire 1/1,000,000 of an earth-quadrant long, and 1/10,000,000,000 in diameter." Now wherein does the so-called practical sys-

tem differ from the absolute system, and why is it not to be as logical and complete as the absolute system? We would never leave the absolute system, if it gave us in all cases convenient numbers; and it does give us convenient numbers for the measurement of a current, its unit being ten times the "ampere" of the practical system. The unit of resistance in C. G. S., however, is too small, so is the unit of electromotive force. To get convenient numbers, we give names to certain multiples of units, that is all; and we use these multiples just as long as it is convenient, and not any longer. That is my idea of the practical system—to use it for convenience and as long as it is convenient; the moment it ceases to be convenient, to throw it overboard and take C. G. S. pure and simple. The conference at Paris decided upon the practical system, by adopting the units which are now so familiar, the ohm, the volt (taken from the British Association recommendation), and the ampere. The coulomb was also added, and it was most satisfactory to get old Coulomb's name in—one of the fathers of electrical science. Then the watt was added by Sir W. Siemens, and it has been generally accepted, and has proved exceedingly convenient. But when you go farther with the practical system, and take anything that involves a magnetic pole or a magnetic field, you get lost in the trouble of adopting the earth's quadrant as unit of length, and the deviation from C. G. S. ceases to be convenient. Return then to C. G. S. pure and simple.

I spoke of the resistance of an ohm being measured in terms of a velocity. I should like to explain this in a few words. Imagine a mouse-mill set with its axis vertical. Put a pair of brushes at the tops and bottoms of the bars; put the brushes in the magnetic north and south plane through the axis, and set the mouse-mill to spin at any rate you please. Take a galvanometer like a tangent galvanometer, but with only an arc equal to the radius—an arc subtending an angle equal to about 57.3°—and electrodes perpendicular to the plane of the arc connected with the brushes. The mouse-mill must be placed so far from the galvanometer, as not sensibly to influence it. Now take the galvanometer, and turn the mouse-mill; let the length of each bar of the mouse-mill be a centimeter; but that would be a flea-mill rather than a mouse-mill—say, let each bar be 100 centimeters; turn the mouse-mill round fast enough to cause your galvanometer to be deflected 45°. Then one hundred times the velocity of the bars is equal to the resistance in the circuit. Double resistance requires double velocity; half resistance requires half velocity to give the prescribed 45° deflection. There, then, is the rationale of 10,000 kilometers per second, or 1,000,000,000 centimeters per second being the measure of resistance. While we thus measure resistance in electro-magnetic measure by a velocity, we measure a conductivity in electro-statics by a velocity. I have given a very simple explanation of this also in a statement quoted by Sir William Siemens in his presidential address to the British Association at Southampton in 1882. The velocity at which the surface of a globe must shrink towards the center, to keep its potential constant, when it is connected to the earth by a wet thread, measures the conducting power of that wet thread. Double conducting power will require double velocity of shrinkage, that is, the globe must shrink twice as fast not to lose its potential. With a very long semi-dry thread the globe may shrink slowly. Suppose we have a globe insulated in the air of this room for electrical experiment, and connected with the ground by a silk thread. If you have an electrometer to show the potential, you will see it gradually sink. You might imagine that dust in the air would carry off electricity, but in truth practically the sole loss is by this semi dry silk thread. When you see the potential sinking, imagine you see the globe shrinking slowly, so as to keep its potential constant, while it is gradually losing its electric charge little by little: the velocity with which the surface must shrink towards the center to keep the potential constant, measures the conducting power of the thread in electro-static measure. Thus we learn how it is a velocity which measures in electro-static measure the conducting power of a certain thread or wire. But, as we have seen in electro-magnetic measure, the resistance of the same thread or wire is measured by another velocity. The mysterious quantity "v"

is the square root of the product of the two velocities. Or it is the one velocity which measures in electro-magnetic measure the resistance, and in electro-static measure the conductivity, of one and the same conductor: which must be of about 29 ohms resistance, because experiment has proved "v" to be not very different from 290,000 kilometers per second.

I have spoken to you of how much we owe to Sir Charles Bright and Mr. Latimer-Clark for the suggestion of names. How much we owe for the possession of names, is best illustrated by how much we lose—how great a disadvantage we are put to—in cases in which we have not names. We want a name for the reciprocal of resistance. We have the name "conductivity," but we want a name for the unit of conductivity, I made a box of resistance coils thirty years ago, and another fifteen years ago, for the measurement of conductivity, and they both languished for the want of a name. My own pupils will go on using the resistance box in ohms, rather than the conductivity box, because it is so puzzling to say, "The conductivity is the reciprocal of the sum of the reciprocals of these resistances." It is the conductivity that you want to measure but the idea is too puzzling; and yet for some cases the conductivity system is immensely superior in accuracy and convenience to that by adding resistances in series. For the reciprocal of an ohm in the measurement of resisting power—for the unit reckoning of conductivity which will agree with the ohm—it is suggested to take a phonograph and turn it backwards, and see what it will make of the word "ohm." I admire the suggestion, and I wish some one would take the responsibility of adopting it; we should then have *mho* boxes of coils at once in general use. With respect to electric light, what is it we want to measure by the current galvanometer? We have a potential galvanometer, and we have a current galvanometer. Everybody knows what we want to measure with the potential galvanometer. The servant in every house that is lighted electrically knows about potentials; and if in reading the galvanometer he sees it is down to 80 volts he knows that something is wrong, and will at once go to the engine-room and cause 84 volts to be supplied; supposing, for example (as in the case of my own house, temporarily, until I can get two-hundred-volt lamps), that the proper potential is 84 volts. But in the current galvanometer there are so many divisions indicating, it may be, the number of amperes in the current. But after all, what do we want besides a knowledge of the potential? It is the sum of the reciprocals of the resistances in the circuit. In the multiple-arc system each fresh lamp lighted adds a conductivity. In a circuit of Edison or Swan hundred-volt lamps, in each of which you have a current of 0.7 of an ampere, and therefore a resistance of 143 ohms, how convenient it would be, in putting on a lamp—adding a certain conductivity—if we could say we add a *mho*, or a fraction of a *mho*, as the case may be. I do not say that *mho* is the word to be used, but I wish it could be accepted, so that we might have it at once in general use. We shall have a word for it when we have the thing, or rather I should say, we shall have the thing when we have the word. The Appendix to the 1862 Report of the first British Association Committee on Electric Measurements contains a description of a "Resistance Measurer" invented by Sir William Siemens, and a "Modification of Siemens' Resistance Measurer," by Professor Jenkin. This instrument gives directly the resistance of a conductor, by means of an instrumental adjustment, bringing a magnetic needle to a zero position for each observation. In the original Siemens instrument the adjustment is a shifting of two coils by translational motion, and the conductivity is read on a scale of equal divisions adapted, by means of a curve determined by experiment, to give a reading of the required resistance. In Jenkin's modification the mechanical arrangement is much simplified by the adoption of a different electro-magnetic combination; and the required resistance is given by the tangent of the angle through which the coils must be turned to bring the needle to zero. A similar instrument to give conductivity by a simple reading, without any adjusting or "setting" for each observation, is easily made. I made such an instrument in 1858, being simply a galvanometer with controlling resistance coils instead of controlling magnets. Such an instru-

ment at once gives conductivity, and you want a name (suppose you adopt *mho*) for the unit of conductivity, and call the instrument a mhometer. The rule would be, the reciprocal of the sum of the reciprocals of mhos, is equal to the number of ohms; and the reciprocal of the sum of the reciprocals of ohms, is equal to the number of mhos. The number of mhos, or of millimhos, will then measure the number of lamps in the circuit. The domestic incandescent lamp of the early future ought to be, and we hope will be, a one-millimho lamp, to give a 10 or 12-candle light with the Board of Trade regulated 200 volts of potential. Thus the lamp-galvanometer, or lamp-meter, may have its scale divided to one millimho to the division, and the number read on its scale at any time will be simply the number of lamps lighted at the time. The instrument will also have the great advantage of being steady, notwithstanding the variations of the engine. A potential instrument on an electro-light circuit at best is always somewhat variable, because the potential varies a good deal—within 1 or 2 per cent. perhaps—but the resistance in the lamps varies exceedingly little. The mho-meter will in these circumstances be an absolutely steady instrument; you will not see it quiver, even though the engine is irregular. The potential galvanometer will show you how much unsteadiness there is to be complained of or to be corrected.

Lastly, as to the objects to be aimed at in respect to the use of this great system of units. Nothing can be much more satisfactory than is the measurement of somewhat large resistances, as we have it habitually at present; but if we want a little more method for low resistances, it will be helped very much by the use of the mho boxes of conductivities which I have indicated. The great thing we want now in the way of practical electric measurement, is a good standard of electromotive force. That was the chief object of a recent British Association Committee, but it has not yet been satisfactorily attained for practical purposes. Standard cells serve for the purpose to some extent, but we want something better, something of the nature of an electro-dynamometer, to give a good steady idiostatic potential gauge, by which the constant of any electrometer or ordinary galvanometer may be easily and accurately tested. That is an object to be sought; there are plenty of ways of attaining it, and I hope, before another year has passed, to see it realized in many ways, certainly in one way.

As to the science of electricity, the great want in the way of measurement just now is the accurate measurement of (" v ") the ratio between the electro-static and the electro-magnetic units; and I hope that scientific investigators will take the matter up, and give to it an accuracy like that which Lord Rayleigh has given to the measurement of the ohm.

A most interesting point remains. It is Joule's work, reported on by the British Association Committee: see volume of Reports on Electrical Standards, p. 138. It was only in my preparation for this lecture that I came upon it, and put the figures definitely together. Joule, with a modesty characteristic of the man, and with a magical accuracy characteristic of his work, made, at the request of the British Association, an investigation of the heating effect of a measured current in a definite way, according to the measure of resistance of the British Association ohm, supposed then to be 10_9 C. G. S. units of resistance; and he himself considered that the electrical measurement which he then made, was more accurate than his old frictional measurement of the mechanical equivalent of the thermal unit could be. The result obtained, assuming the British Association ohm to be absolutely correct, gave the mechanical equivalent as 782.2 foot-pounds, instead of 772 which he had made it before, and he expressed himself willing to make a new determination of it by the frictional method. But now let us put ourselves in the position of 1866, the date of this report, with these competing determinations of the ohm: that obtained by the British Association method of spinning coils; and by Joule's electro-thermal method with the dynamical value of the thermal unit, as was given by his frictional method. Supposing that his electro-thermal method was right, then what we are to infer is not that the result is the mechanical equivalent, but that the British Association unit was not 10^9, as it was supposed to

be, but $10^9 \times 0.98699$. Thus this experiment was virtually Joule's determination of the resistance of the British Association ohm in absolute measure. Lord Rayleigh's determination is $10^9 \times 0.98677$, a difference of 2 in the 4th place, within about 1/50 per cent. There is perfect magic in the accuracy of Joule's work; it is not a matter of chance. I think between Joule, Lord Rayleigh, Mrs. Sidgwick, and others, we cannot have much doubt now, what is the absolute value of the Siemens unit, or of the British Association unit. I advise everybody to take the Rayleigh ohm unit, instead of the British Association ohm. I have begun to do so, and I mark everything R. O. You may have everything in the British Association unit, but reduce, if you please, to Rayleigh ohms by the reducing factor 0.98677. Volts must be reduced in the same ratio. The old estimate which I made in 1851 from Joule's experiment, for the absolute electromotive force of a standard Daniell cell, was 1.07 volts; and after thinking it was 1.078 for ten years, because of the British Association unit, we come back to correct it, and find it is 1.07. So much for the volt. But we want far more accurate instruments and methods connected with other parts of electric measurement, especially electromotive force and capacity, electro-static, or electro-magnetic, with the comparing number "v."

These are the things we want to advance and perfect, to give a satisfactorily scientific character, to this great system of absolute measurement, of which I have endeavored to trace and explain the origin.

TESTING MACHINES, THEIR HISTORY, CONSTRUCTION AND USE.

By ARTHUR V. ABBOTT.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

III.

SPECIAL MACHINES.

Passing now to our second general subdivision of testing machines, we come to the consideration of those which have been designed for special purposes, and perhaps the first and most notable among these is the autographic torsion machine of Prof. Thurston. Fig. 21 is a perspective view of this machine, and in Fig. 22 a front elevation, a longitudinal section on line aa, and a transverse section on line aa' may be seen. The machine consists of two A-shaped frames AA, AA, and shown in all the figures. These are firmly mounted on a heavy bed-plate. Near the top of each of these frames are spindles C and D, each of which has a rod E and H, with a jaw to receive and hold the heads of the specimens. The two spindles are not connected in any way to each other, except by the specimen itself when placed and adjusted to be tested. To the spindle a long arm HB is attached. It carries a heavy weight B attached to its outward end. This spindle has a norm wheel driven by a worm F on the shaft LL, which is turned by a hand-crank K. When the specimen is placed in the two jaws, and the spindle is turned by the worm-gear, the effect is to twist the specimen. The position of the arm and the weight will at all times be a measure of the torsional stress which is exerted on the specimen; but as this torsional stress is increased the specimen commences to give way or be twisted by the stress, according to the quality of the material. In making such torsional tests it is essential that the amount of twist should be known. If a record of this could be procured, it would be an indication of the capacity of the material to resist such stresses, or, in other words, of its quality. This testing machine was designed for this purpose. The record is made in the following way: To the spindle C a cylindrical drum is attached, which is covered with a sheet of cross-section paper. To the arm N a

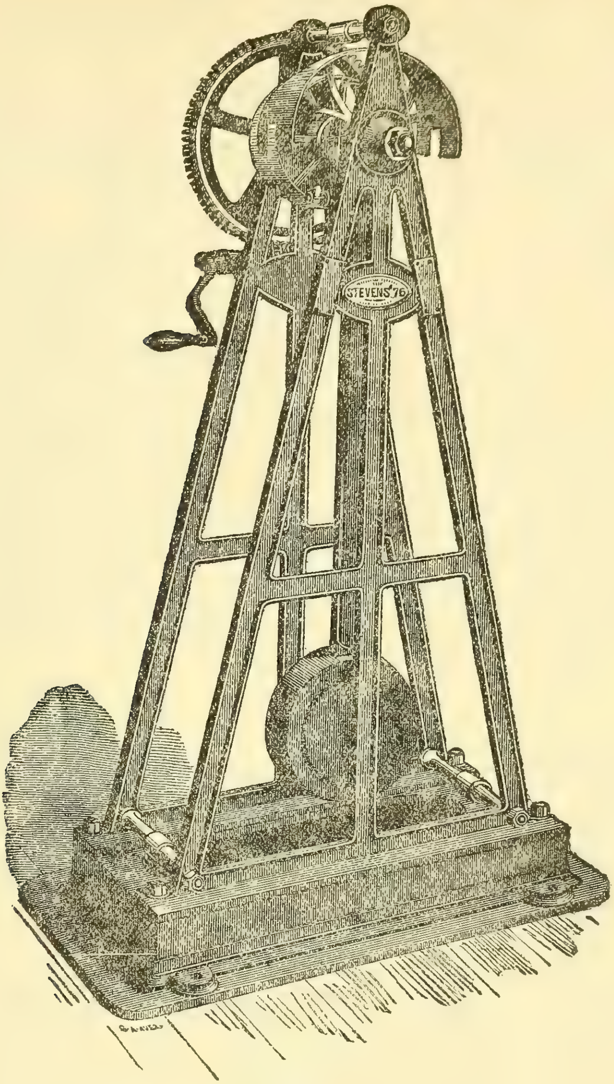


FIG. 21.

pencil holder is attached at F, and carries a pencil D, the point of which bears upon the paper of the drum. Supposing that the specimen in the machine should offer no resistance, and should merely twist, the pencil would then remain stationary, and as the drum is revolved the pencil would trace a straight line on the paper, the length of which would measure the amount which the specimen was twisted. If, on the other hand, the specimen be supposed to resist and to twist simultaneously, as is always the case, it will be seen that the spindle B would be turned, and the arm N would be moved from a vertical position to a distance proportionate to the stress resisted by the specimen. The pencil holder attached to the arm would move with it. To make a record of this distance a guide curve FF′ is attached to the frame of the machine, so that when the arm N and the pencil holder are moved out of their vertical position the pencil is moved towards the left by the guide curve, which is of such a form that the lateral movement which it gives to the pencil is proportional to the moment of the weight on the end

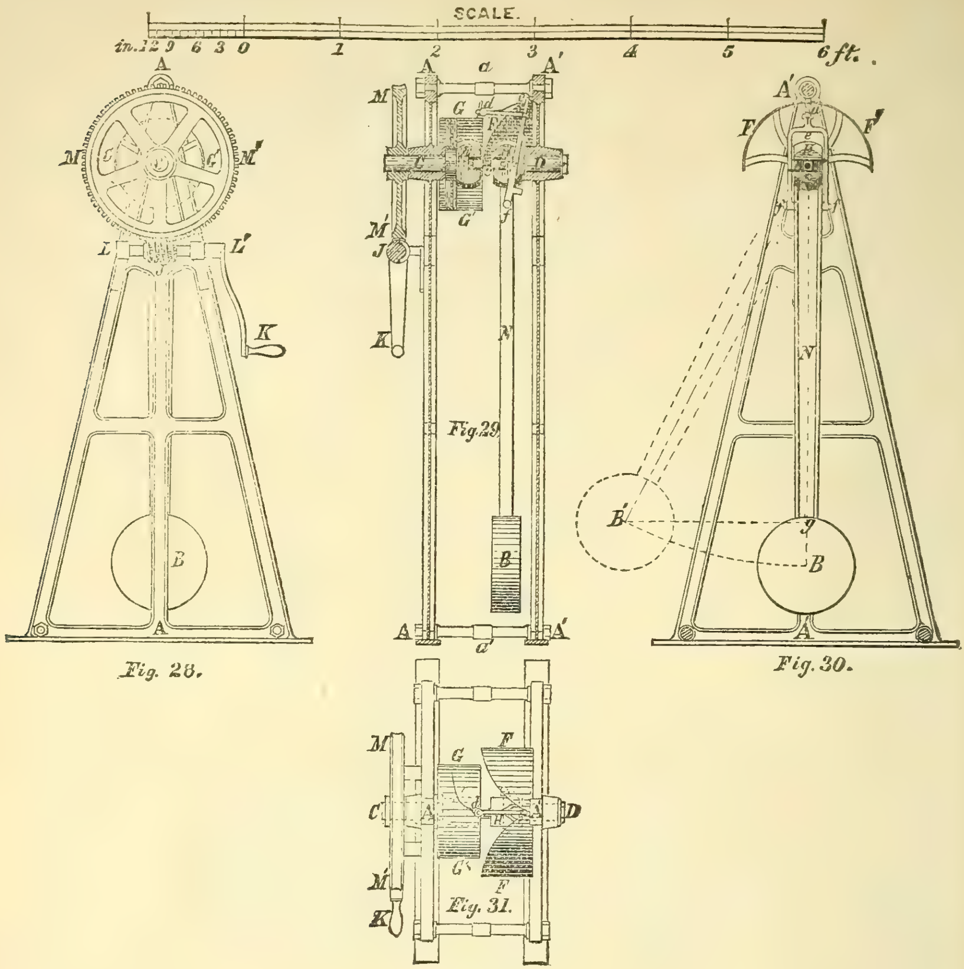


Fig. 22.

of the arm. In this machine each inch of ordinate denotes 100 foot-pounds of moment transmitted through the test piece, and each inch of abscissa ten degrees fo torsion. By use of this machine the metal tested is compelled to tell its own story, and to give a permanent record of its strength, elasticity, and every other quality which is brought into play during the test.

The transverse testing machine of the Stevens Institute, Fig. 23, built by Messrs. Fairbanks & Co., consists of a heavy platform scale, on the platform of which stands a cast-iron frame, to which are fastened the supports *dd* at the required distance apart. The pressure on the specimen is applied by means of a hand wheel on the upper end of the screw *k*, which screw passes from the nut *e*, and terminates in the sliding cross-head *i*. This cross-head serves both as a guide and as a pressure block. The test piece *l*, rests upon mandrils mounted upon the supports *dd*, at the required distance apart. All the loads are weighed in the usual manner at *m*.

A second example of transverse testing machine given in the illustration Fig. 24, is one which is especially designed to meet the requirements of makers and users of cast iron. The construction of the machine is exceedingly simple, and may be almost seen at a glance. A wooden framework supports a hand-wheel for applying the stress to the

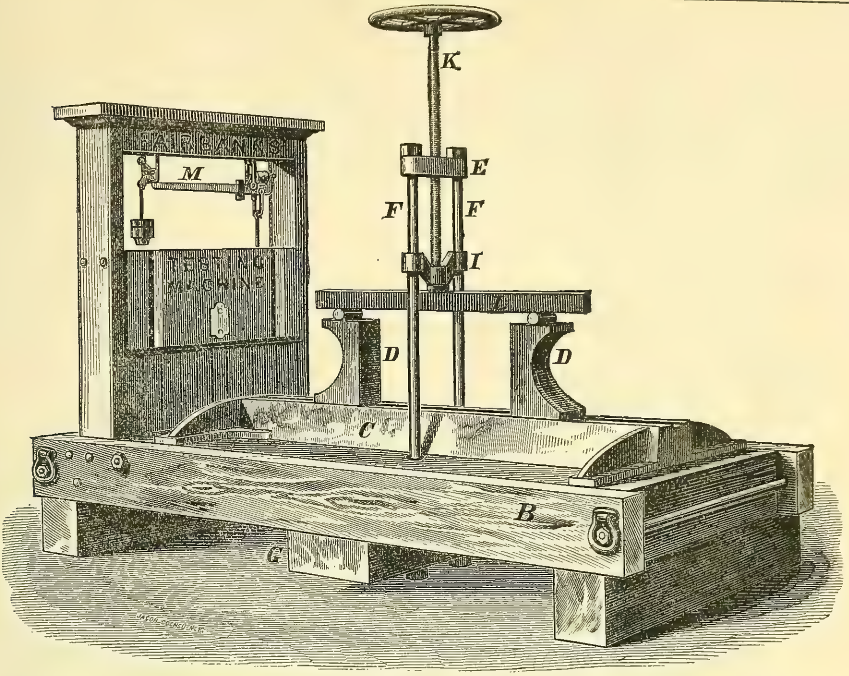

FAIRBANKS
M
TESTING
MACHINE
K
E
F
F
I
L
D
D
C
B
G

Fig. 23.

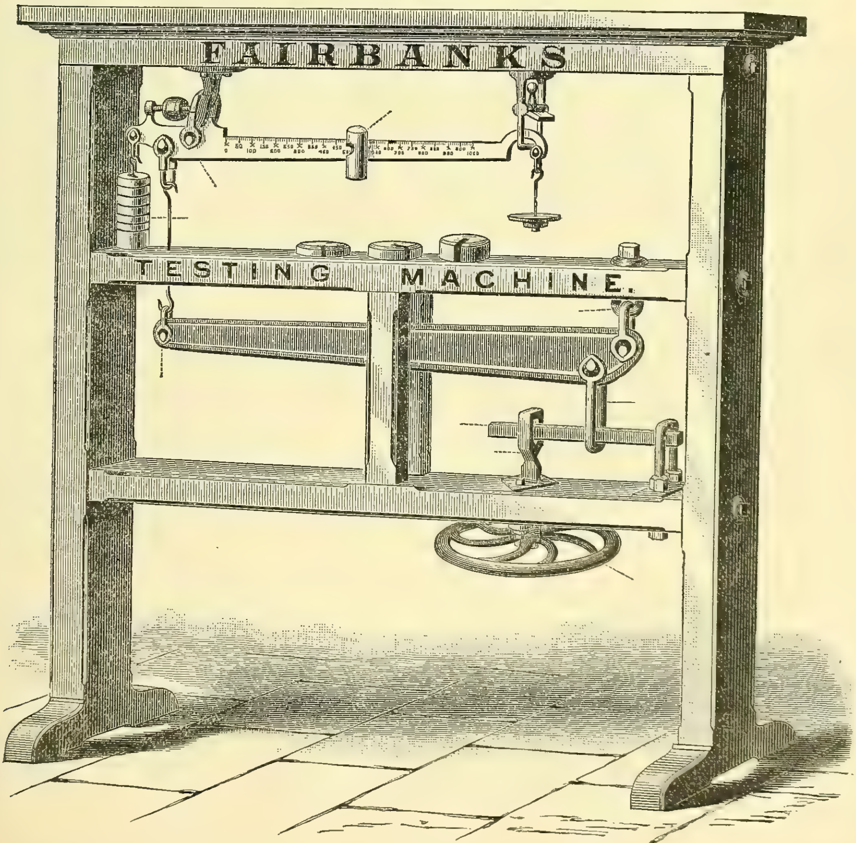

FAIRBANKS
TESTING MACHINE.

Fig. 24.

specimen, which is hung from the end of a lever supported close to the wheel. The end of this lever carries the stress to the weighing beam, where it may be estimated by sliding the poise to and fro, and adding the weights to the weight counterpoise.

the scale work of the machine. Perhaps its operation may be clearly understood by describing the method of making the experiment. The cement being mixed with the necessary quantity of water, is pressed into the moulds M, and having stood a sufficient time to set, the mould is removed by opening the clamps and sliding the two side pieces away. After the briquette has attained the requisite age it is placed in the clamps NN, the hand wheel P is turned, drawing down the lower clamp and taking up the slack so as to raise the scale beam nearly or quite

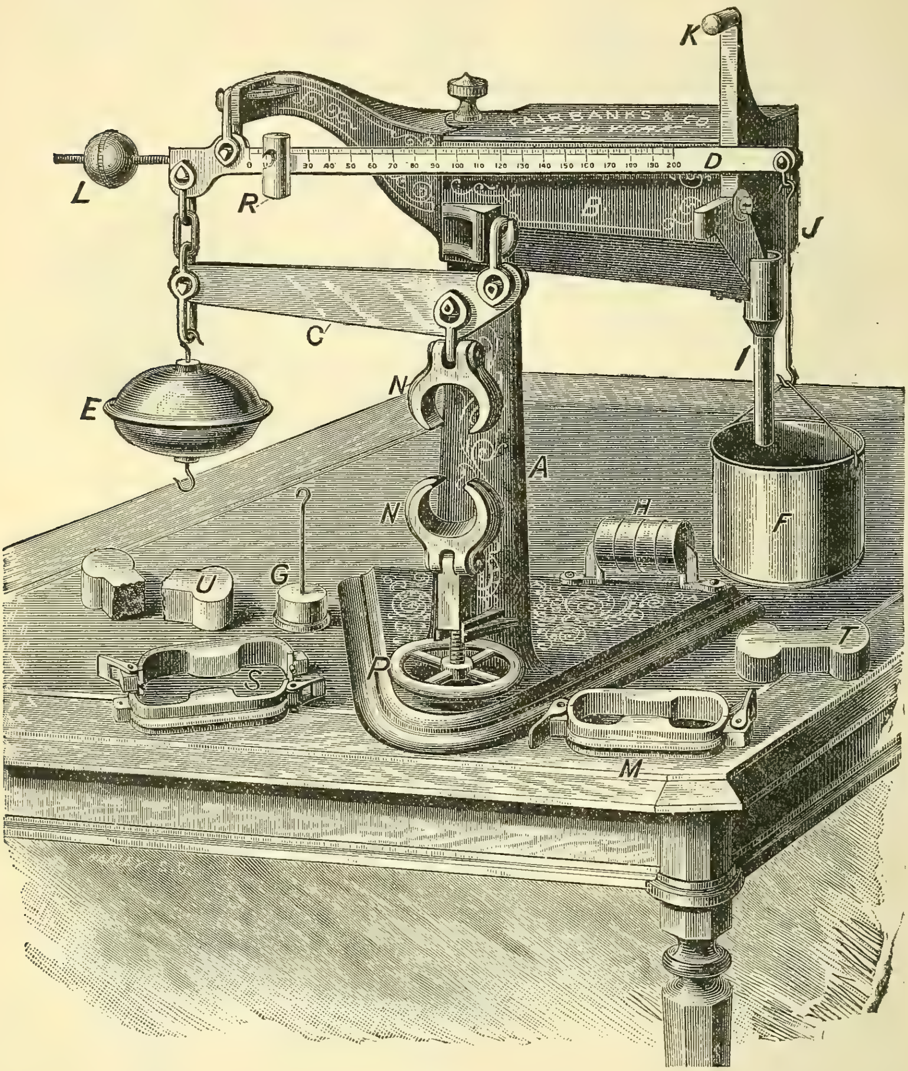


Fig. 25.

Perhaps next to iron and steel the material most frequently tested is cement, and in the illustration, Fig. 25, may be seen an automatic cement-testing machine. On the pedestal of the machine there stands a column A, supporting on the top a receiver B, to which is attached

to the top of the guard K. The reservoir B has previously been filled with shot, and by opening the valve J this shot is allowed to run from the tube I into the pail F. The weight of the shot flowing into the pail constantly increases the stress on the briquette between the clamps NN. As soon as this stress reaches the tenacity of the specimen in question, the briquette breaks, the beam D drops, and striking the valve J, shuts it up, cutting off the supply of shot. The pail with its shot is now detached from the end of the beam, and hung on the hook on the under side of the balance ball E. In the place of the pail is hung the counterpoise G, which exactly balances the weight of the pail, thus leaving only the amount of the shot contained therein to affect the scale. By sliding the poise R to and fro on the beam, or by adding on the counterpoise the weights H the amount of the shot required to break the cement may be readily and directly read from the beam The beam is graduated to 200 lbs., and with the addition of four small weights a capacity of 1,000 lbs. is obtained.

The elasticity of car springs, and the amount which they will deflect under a given load has lately become a matter of much investigation, for the railway companies constantly increasing the loads in their cars and running at higher speeds, require springs which shall be correspondingly safer and more durable to support the heavy use to which they are subjected. Fig. 26 is an illustration of a machine designed to meet the wants of car spring manufacturers, so that every spring that is made may be, as fast as it is completed, tested, to ascertain whether or not it fulfills the specifications that it is intended to meet. A heavy cast iron framework supports a cross-head carrying an adjustable plunger. This plunger may be raised or lowered by means of a screw thread cut on it, so as to accommodate any length of spring from two inches to three feet. Directly beneath this plunger there is a

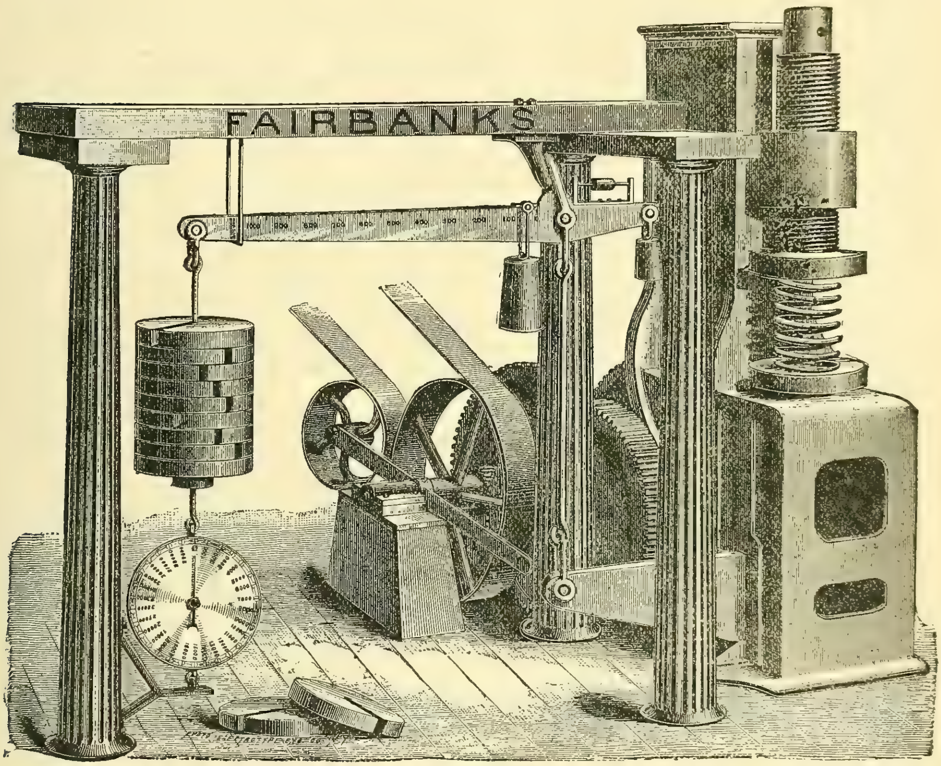


Fig. 26.

bed plate for supporting the spring, which forms a platform of scale. The other end of this lever stands on a lever directly inside of the casting that conveys the stress received by the spring to the scale beam, where, by means of a sliding poise and the large weights, its quantity may be correctly estimated. The gearing and the belt at the back of the machine form a means by which the plunger may be moved up and down. For example, supposing a railway company has issued specifications for 10,000 springs 6 in. in height, which shall carry 10,000 lbs. and deflect 7-8 of an inch under a load. The spring maker coils a spring which he supposes will answer the requirements, and placing it in a testing machine, so adjusts the cross-head that it shall depress the spring 7-8 of an inch. The machine is then started and the weights added to the scale beam until a balance is obtained at the given deflection of the specification. If the weight thus indicated corresponds with that of the specifications, the spring maker knows that the spring in question will answer the requirements, and he goes on to fill his contract. If, however, the spring be either too stiff or too weak, a corresponding change must be made in the article. Having completed the entire contract the machine is adjusted to give a deflection of 7-8 of an inch, and as fast as the springs are ready, each one is placed in the machine. A glance at the spring balance, attached at the underside of the weight counterpoise, will tell the exact number of pounds that the springs sustained with a deflection corresponding to that inserted in the specification. All the springs which fulfill the requisite of weight may be confidently shipped by the manufacturer, while those that for any reason fail, may be returned to the factory. With a machine of this kind the manufacturer has a guarantee that all of the entire product must be accepted by the railway company as fulfilling their specification.

THE USE OF THE TESTING MACHINE.

The investigation of the physical properties of materials used in construction may be analytically divided into four parts:

1st. The investigation of the qualities of a special material. For example, the engineer may wish to ascertain the tensile strength, ductility, and resilience of a special steel.

2d. The manufacturer may wish to ascertain variation in quality produced by a change in the chemical composition of the metal forming his output. For instance, in the processes of the open hearth or the Bessemer converter, changes in the relative proportion of carbon, manganese or phosphorus may produce the greatest changes in the physical properties of the steels produced.

3d. The manufacturer or the consumer may wish to ascertain the changes produced in material by means of the different mechanical processes to which it is subjected while being shaped into the proper forms required by different structures. A noteworthy example is the various methods by which eye-bar heads are formed.

4th. The actual strength, elasticity, or resistance of a member forming one of the parts of a bridge or other structure may be desired, either for information as to the best method of design, or from which the actual strength of the structure in question may be ascertained.

These various subdivisions will now be treated in their appropriate order.

CARE ON THE PART OF THE OPERATOR.

The manipulation of the testing machine in order to obtain therefrom the most accurate and reliable information, embracing all the facts and data to be obtained from an experiment, is an operation requiring on the part of the manipulator the greatest exercise of care and skill and watchfulness. In making experiments on ordinary test pieces of a few inches in length, the operator is constantly dealing with two sets of quantities: one the measurement of the size of the test piece, and the amount of deformation produced by the stress previous to the elastic limit—involves the use of the most minute quantities. An error in the measurement of the piece of a thousandth of an inch, a quantity far too small to be appreciated, except by the most delicate means of measurement, may induce errors in the result sufficiently large either to condemn an otherwise good material, or to accept one which is inferior. In the measurements of the

deformation of the piece under the action of the stress, the quantities are still smaller, reaching to a ten thousandth, or even to a millionth of an inch, so that it will be obvious that during this part of the operation the greatest care is necessary to observe most carefully and accurately all of the circumstances which take place. The measurement of the stresses exerted by the machine running to thousands or hundreds of thousands of pounds, requires an exercise of skill in quite a contrary direction. It is not only necessary to measure these stresses accurately, but at the same time the mind of the operator must have sufficient flexibility so as to turn rapidly and easily from the consideration of a very small quantity to one which is exceedingly large. The entire behavior of the piece under the operation of the stress is one which requires the minutest attention. The equal or the unequal yielding of one side or the other, the appearance of the piece as under the operation of the stress, the fibers are torn apart, the surface becoming blistered, seamy and uneven, appeal in the most eloquent manner to the eye of the skillful investigator. The amount of deformation produced by the stress, and the way in which it takes place, whether the piece uniformly yields under its stress, or whether the yielding takes place at one particular point, may convey most important information as to the character, qualities and suitability of the material under investigation.

Placing the test piece in the testing machine is an operation requiring most careful attention. It is necessary that the piece should be so arranged that its axis may coincide with the axis of stress produced by the testing machine. Even in the machines so designed as to furnish automatic means of centering it is still necessary for the operator to exert the most unceasing vigilance to see that the piece is correctly surrounded by the jaws of the machine, to insure that these jaws exert an equal bearing all over the surface, that it is properly set in line, and that the entire adjustment with reference to the various parts of the machine is done in the most careful and accurate manner. This adjustment of the test piece is especially essential in experiments upon metals which are crystalline, stiff, and unyielding, such as cast iron. Mr. Hodgenson states that the strength of a rectangular piece of cast iron so placed in the machine as to cause the stress to pass along the side of the piece is only one-third of the strength to resist a central stress. It is evident that this ratio would perhaps be given only by a few forms, subjected to tension in a peculiar manner, and that it probably would vary with every change in shape and position of the test piece. In machines not furnished with self-adjusting jaws the importance of this care in setting the test piece can hardly be too strongly insisted upon. It has frequently happened in the experience of the author that in making experiments upon plate iron the piece would be so placed in the testing machine as to be torn apart rather than fairly broken. In many experiments the bend of the test piece after rupture was markedly perceptible to the eye. The errors in such experiments may rise as high as twenty or thirty per cent. of the actual strength of the piece in question.

THE APPEARANCE OF THE FRACTURE.

After the completion of an experiment a careful examination of the test piece should be made, and the character and appearance of the fracture should be carefully noted as an index of the quality of the material. Good wrought iron, when broken by tension, should present a fracture resembling a bundle of fibers, having a dark gray color. There should be little or no appearance of lamination due to the piling and the rolling of the bar, and no appearance of any bright or crystalline spots. The fracture should be irregular, inclining towards cup shape. The general appearance of the fracture of good wrought iron is indicated in Fig. 27. In the poorer qualities of wrought iron it is very customary to find the bar interspersed with a greater or less number of bright, shining crystalline spots, similar in appearance to cast iron, excepting that the crystals have very much higher metallic luster. The relative proportion that the crystalline spots bear to the whole area of the piece may be taken as a very fair index of the quality of the material. Fig. 28 gives a representation of the fracture of the medium quality of cast iron, showing about one-third of its area to be com-

posed of these hard crystals. Such iron may stand a very high tensile stress, but it will show very little ductility. This crystalline structure of the bar may be due to one of two causes. Either the iron contains too large a proportion of phosphorus, and so is termed cold-short, being exceedingly brittle and unworkable when cold, or else during some processes of the manufacture the bar was overheated (burnt, as it is said in the blacksmith's shop) and allowed to cool with insufficient mechanical work. The cold-shortness attributable to too large a quantity of phosphorus is not indicated by any exterior appearance of the bar, whereas, if the iron has been burnt, the exterior is blistered and rough, showing to the practiced eye that there has been overheating, by which the more fusible parts of the bar were actually melted.

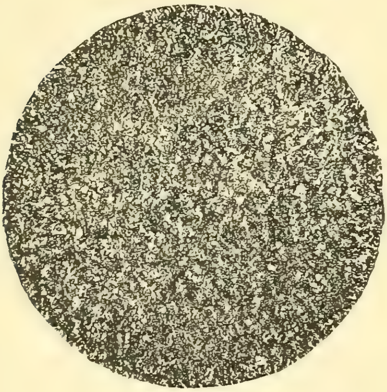


Fig. 27.

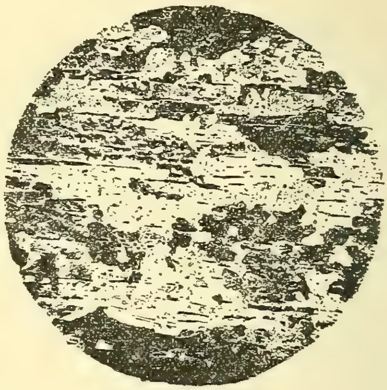


Fig. 28.

The length or size of the fibers in the fracture is an exceedingly good index of the value of the material. Fractures which occur with short and apparently brittle fiber, rather fine in texture, generally indicate an iron which will when cold possess considerable ductility, yet probably has such a proportion of sulphur as to make it red-short and exceedingly troublesome to manipulate under the hammer. The results of irons of this kind may be fairly good in the testing machine, yet as far as is possible they should be avoided in construction on account of the difficulty of obtaining good results from shop manipulation.

During an experiment on wrought iron, as soon as the elastic limit is passed the surface of the specimen, if it has previously been machined, gradually loses its bright metallic luster and becomes rough and mottled under the distorting effect of the stress. In the finer and most superior irons this mottling of the surface is exceedingly delicate. The fibers of the piece seem to be rumpled and distorted by the stress so that the whole of the surface is thrown into an innnumerable number of minute ridges. In the coarser and less valuable irons this mottling of the surface is comparatively heavy and uneven and the want of homogeneity in the structure of the metal is clearly indicated by the uneven appearance of the test piece.

The fractured surfaces of steel bars present a far greater variety than those of iron specimens, in fact there are at least a dozen or fifteen different appearances in steel indicating corresponding changes in the value of the metal. The appearance of the fracture of good constructural steel, having an elastic limit of from forty to fifty thousand pounds, and an ultimate of from seventy to eighty thousand is a light silky gray. One end of the test piece, broken by tension, being a slightly truncated cone that fits into a corresponding concavity in the other end. Steel fractures, excepting those of the poorer varieties, should rarely be termed fibers, for the texture of the metal is so exceedingly delicate as to be only comparable to the finest silk. Steels made by the Bessemer process generally pre-

sent a fracture which is darker and a little more corky in appearance than those made by the crucible or the open hearth methods. As the amount of carbon increases, the tensile strength and the elastic limit are increased, and at the same time the silky and conical appearance of the fracture gradually decrease, until, when the tensile strength rises to a hundred thousand pounds per square inch, the silky appearance is almost entirely lost, the fracture becomes square, showing little reduction in the bar and presenting a granular appearance made up of small brilliant crystals. The same appearance of the fracture may be produced by an excess of manganese. In such steels the tensile strength may not rise over sixty to eighty thousand pounds, yet the character of the fracture is by the presence of the manganese entirely changed from that of the fine delicate texture of the carbon steel to a coarse and granular appearance almost resembling that of cast iron.

The effects of phosphorus and sulphur are similar to those produced in iron. An excess of phosphorus renders the piece cold-short, reducing the amount of reduction and giving the fracture a more granular character.

The following classification of the characteristics of fractures is presented, hoping that it may in some measure aid towards uniformity in description.

STEEL FRACTURES.

First, square.—Those in which the plane of the fracture is nearly at right angles to the axis of the test piece.

Second, irregular.—Those in which the plane of the fracture is inclined more or less to the axis of the test piece and in which the fracture presents one or more jagged points.

Third, concoidal.—In which one end of the test piece presents a cone matching in a corresponding conical socket in the other half.

AS TO MOLECULAR STRUCTURE.

First, fine crystalline.—Fractures such as are presented by the rupture of the best quality of tool steels, in which the bar seem to be made up of an infinite number of exceedingly fine and iridescent crystals

Second, crystalline.—Such fractures as are presented by the coarser qualities of tool steels, or by the higher grades of structural steel containing large amounts of carbon or phosphorus. Here the fracture is composed of larger crystals, losing to a great extent their iridescence and seeming to be grouped in radial lines around the center.

Third, silky.—Fractures which are manifested by the best varieties of structural steels in which the crystalline appearance is entirely lost, inasmuch as the crystals seem to be merged in long bundles of the finest and silkiest fibers.

Fourth, granular.—Fractures of this kind are manifested by the inferior varieties of structural steels and seem to consist of a large number of granular crystals of considerable size, grouped in radial lines around a central axis.

Fifth, coarse granular.—Fractures of this class are only manifested by the inferior qualities of structural steels and are generally indicative of too high a per centage of manganese. In such cases the crystals are large and coarse, presenting many glimmering facets and having almost the appearance of the poorer qualities of wrought iron.

Wrought iron fractures may be classified into square and irregular, as follows :

Square fractures are presented only by the inferior crystalline varieties that break sharply and shortly with but little or no reduction or elongation. As the quality of the iron becomes better, and the crystalline texture disappears, the fracture becomes more and more irregular, until, when a pure fibrous iron is attained, the fracture may occur at a plane inclined to the axis of the test piece at any angle, and presenting many jagged irregular points. The square fractures are always exceedingly crystalline, presenting a great number of large crystalline facets, having an exceedingly high metallic luster. The irregular fractures may be divided into long fibrous and short fibrous, depending simply upon the relative length of the fibrous structure.

Fractures of cast iron may be divided into, simply, fine crystalline and coarse crystalline, depending upon a relative size of the crystals. The coarser and less refined varieties of cast iron present, when broken, a fracture which is made up of a large number of dark grayish crystals, interspersed here and there with

traces of graphite. Upon remelting and refining the texture of the fracture, by each succeeding operation it becomes finer and finer, until, in the best refined iron, we have a fracture which is made up of exceedingly fine gray crystals, having but little metallic lustre and giving no evidence of graphite.

EFFECT OF THE SIZE OF THE SPECIMEN.

In making experiments upon the *qualities* of material the first point to be considered is the size and shape of the test piece and effect which this produces upon the results to be obtained from the testing machine. The variation of the form of the test piece considerably modifies the apparent tenacity of ductile material; consequently it is necessary to note the size and shape of the specimen to be tested, in order that the results obtained from the experiment may be correctly compared with those of previous or future results. When a piece of metal is subjected to tensile stress, and slowly drawn in two, it will yield at the weakest section first, and if that section is of considerably less area than the adjacent parts, or if the metal is crystalline and unyielding, it will break sharply and without appreciable stretch, the fractured surface having a granular appearance.

On the contrary, if the test piece has a uniform section of considerable extent, it will gradually stretch with a uniform reduction of section from end to end. Toward the ends where the piece is grasped by the machine this reduction of area is least perceptible. When the stress has obtained so great an intensity that the weakest section is strained beyond its elastic limit, the molecules of the metal commence to flow, and the piece gradually reduces in cross section, the particles immediately adjacent to the over-strained portion flowing towards the section of greatest stress. If, however, the shape of the specimen is so arranged as to prevent or to retard this flow of metallic molecules, the result is to largely increase the apparent tenacity of the material. This increase of tenacity due to the shape of the specimen has been suspected for many years past; but has only received a complete demonstration within a comparatively short space of time. The United States Board for making tests of iron and steel, made a series of experiments to ascertain the relative value of specimens having long or short uniform sections. The results of these tests are presented in Table I.

TABLE No. 1.

Prog. No.	Specimen L″	E.Lim lbs. □″	Ult St. lbs. □″	Elong. %	Reduc. %
1	5 00	26795	51989	30.	49.3
2	3.938	28194	52389	32.	50.0
3	4.500	28062	52495	30.	46.3
4	3.500	27268	53052	31.6	48.0
5	3.000		52984	33.0	48.0
6	2.472		52852	32.1	45.6
7	1.989		53169	32.9	45.0
8	1 500		52666	35.0	45.2
9	1.000		53169	35.4	43.5
10	.500		57318	41.6	36.6

Some experiments have given the following results from Bessemer steel specimens:

TABLE No. 2.

Grooved Specimen—	
Highest	162974
Lowest	136490
Average	153677
Long Cylinder—	
Highest	123166
Lowest	103255
Average	114460

In Fig. 29 may be seen the curves that have been obtained from a series of specimens of open hearth steel, made under the direction of the author in the following manner. From a bar of steel sixteen feet long there were cut twelve pieces. Each of these pieces was placed in a lathe, and one cut taken over it so as to render the piece perfectly straight and true. The series of steel pieces thus obtained, all cut from the same bar, were turned to lengths of uniform diameter, as seen in the column headed "Lengths" in the accompanying Table 3.

From these experiments it will be seen that the uniform length of the piece under examination exercises an exceedingly important part in the results obtained from the experiment.

In explanation of the cause of the difference in the flow, and the increased tenacity of the grooved specimen over one having for considerable length a uniform section, let it be supposed that a

TABLE No. 3.

Dimensions of Test Piece.			Stress Tension in lbs. per Square Inch.				Elongation in inches.	Reduction per cent.
Length in inches.	Diameter in inches.	Area in square inches.	Elastic Limit.	Maxi-mum.	Ulti-mate.	On reduced Area.	Ultimate.	
Groove	1.000	.785	Elastic limit taken by fall of beam. Not perceptible before this point.	79286	78930	99614	Infinite.	20.77
.25	"	"		71138	70070	88530	100.00	20.85
.500	"	"		66446	64306	84983	60.00	24.33
.75	"	"		63036	61224	87114	46.66	29.72
1.00	"	"		62637	59707	87771	40.00	31.97
2.00	"	"		61671	57398	89463	30.00	35.84
3.00	"	"		61161	56122	94017	29.30	40.31
4.00	"	"	39642	61253	56226	94602	27.50	40.54
5.00	"	"	39667	61125	56226	94421	25.00	40.41
6.00	"	"	39515	60778	56250	94839	23.66	40.69
8.00	"	"	39235	60778	56226	94218	22.50	40.43
10.00	"	"	39158	60735	56250	94839	22.00	40.69

single chain of molecules of a ductile metal be subjected to tensile test, and let the length of this chain be ten inches. Under tension, each molecule would become separated from its neighbor, as far as their cohesive attraction would allow, on releasing the stress the molecules would resume their original places, no permanent extension having taken place. The greatest distance which one molecule could be separated from its neighbor would be an infinitely small quantity, and the total of the numerous extensions would probably not exceed one or two hundredths of an inch in the length of a ten-inch chain. This separation would represent the real limit of elasticity, which, if exceeded, would be followed by rupture. The stress causing this temporary extension would be the elastic limit, the tenacity and the tensile strength of the material. If the extension were carried beyond this, the chain of molecules would break, but each of the broken parts of the chain would show no indication of permanent set.

A test bar may be conceived to be made up of a number of these chains of molecules, each link having the peculiar property under high tension of leaving its own chain and taking up a position between the two nearest links of the next chain of molecules. These chains may thus become permanently elongated by the addition of new links, but they are at the same time reduced in number. The permanent elongation of the specimen is an indication that such a flow of molecules has occurred. The direction of this flow is from the exterior to the interior, as is proved by the reduction (always observed at the point of fracture) in a ductile material. Each chain in the aggregate may be supposed to be capable of bearing the same stress as if tested alone, or as if it had not become elongated by the addition of the neighboring molecules; but as the chains are fewer in number, the total stress carried by the whole, prior to rupture, is less than if the flow of molecules could have been prevented. For example, it is obvious that one hundred feet of ordinary iron chain, having a strength of one hundred pounds may, be divided into ten equal lengths of ten feet each, which by their combined tension might be made to support a load of one thousand pounds; but if the chain be divided into five parts, each twenty feet long, the united strength would only be five hundred pounds. In the case of a test piece, each molecule is not only a link of the chain in longitudinal direction, but it may also be regarded as a constituent link in all directions. In an ordinary ten-inch cylindrical specimen the end links of the transverse train of molecules come to the surface and are free. They therefore offer no resistance to flow in the transverse direction. In a grooved specimen the transverse molecular chains are not

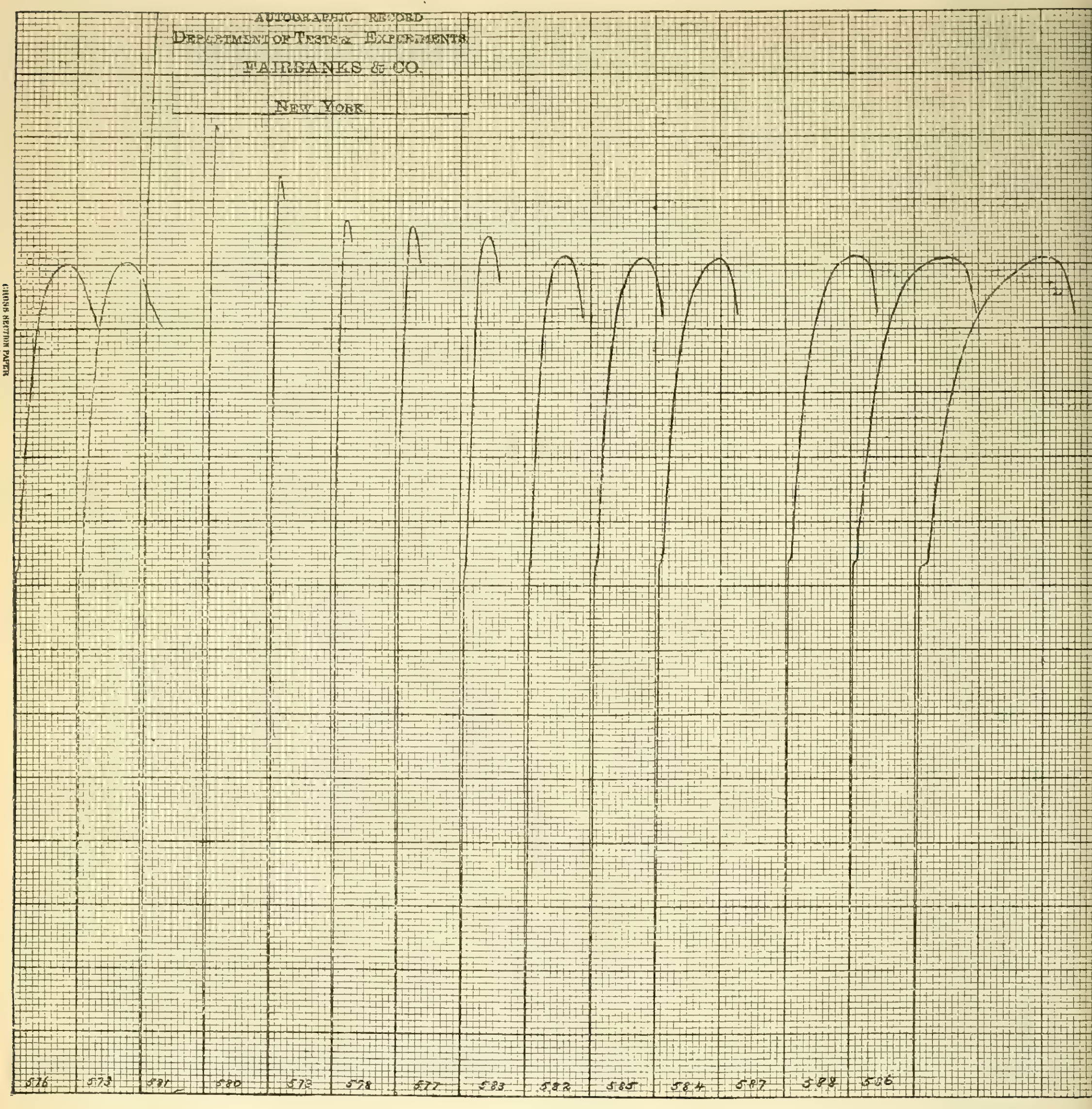


FIG. 29.

free, but are united to the ring of external metal. If this ring be of sufficient strength to resist the transverse stress, the reduction of the area of the piece would be almost, if not entirely, prevented, and consequently the tensile strength per square inch will approximate to the tenacity of this specimen—using the word tenacity to mean the strength calculated on the reduced section of the piece. Consequently, in a grooved specimen the additional strength thereby obtained may be regarded to be the sum of the tensile strength of the piece, plus the amount of force requisite to prevent the flow of metal and the ordinary reduction of area. In proof of this an examination of column of "tension per square inch of reduced area," in the above series of steel tests, will show that the ultimate tenacity

of the piece obtained by calculating the ultimate breaking strength as the reduced area of the piece, is nearly a constant quantity. To measure accurately the exact size of a broken test piece is a matter of considerable difficulty, and the variations in the foregoing column may probably be regarded to be due more to the inaccuracies in the measurements of the reduced area rather than to deviations from the above expressed law. Experiments of various grades of iron and steel have certainly indicated the probability that each particular grade of metal has its own ultimate of tenacity, so that if the tenacity of the piece be calculated upon the reduced area after fracture, a constant quantity would be obtained for each of the different grades of metal for every different length of test piece.

The importance of specifying in contracts for iron work, the exact size and shape of the pieces to be tested, has thus been rendered very obvious.

It is to be regretted that the size of test pieces has not been even more rigorously proscribed, and it is now suggested that engineers in framing iron and steel specifications, would do well to carefully indicate the exact size, shape and other circumstances governing the specimens to be tested.

THE EFFECT OF TIME OCCUPIED IN MAKING THE TEST.

It has previously been indicated that one of the first effects of stress on a ductile material is to cause the molecules to flow from the exterior towards the center. The question of the time occupied in making an experiment would seem to be an important one, and doubtless if the rapidity of the test is so great as to cause the stress to approximate towards a blow or a shock, the material has considerably less resisting power than it would have to a slowly and gradually applied stress. Yet the time occupied by most machines in making an experiment is so considerable that no approximation towards shock is ever reached.

In making a large number of steel tests for the East River Bridge, the question of the time to be occupied in making each experiment was gravely considered. The machine upon which most of the tests were made was worked by a hydraulic press actuated by hand power. In order to determine the effect of the time nine pieces of steel, one inch square and two feet long, were cut from the same rolled bar. These pieces were carefully measured and were placed in the testing machine without any preparation whatever. Three of the pieces were broken in ten minutes each, three were broken in six minutes each, and three were broken in twenty minutes each. The results obtained from these nine experiments only indicated a difference of some three or four hundred pounds, and no greater difference was observed between the test made in six minutes each, than those that were made in twenty minutes. It was inferred that on this particular grade of steel little or no difference could be attributed to the time occupied, provided it did not exceed the above maximum and minimum limits.

As a mean of a number of tests on wrought iron, Kircaldy found that with a suddenly applied stress an ultimate resistance of 46,500 pounds per square inch was obtained, while with a stress gradually applied the resistance rose to fifty-seven thousand two hundred pounds. Unfortunately the report from which the above is quoted does not give any information as to size or kind of specimens, or to the manner in which the suddenly applied stress was exercised. In making many experiments upon wrought iron and steel the author observed the following phenomena: If the testing machine be run comparatively slowly—so slowly that the motion of the cross head was a little slower than the natural flow of the metal—the test piece yielding gradually, giving a good elongation and reduction. By increasing the speed of the machine so as to quicken the motion of the cross head to such a speed as to a little more than keep up with the flow of the metal, a higher breaking stress was reached, accompanied with less elongation and reduction of area. By still further increasing the speed of the cross head a point would be reached where there was still less elongation and reduction and where the breaking strain was higher than in the first instance, and lower than that observed in the second instance. It would therefore seem that for each metal there is a particular rate of flow to which, if

the testing machine be carefully adjusted, the maximum resistance may be obtained. Yet, for the ordinary experiments upon a commercial product, or even for the purpose of comparing material with specifications, the time occupied in making the experiment may be considered as comparatively of slight moment, provided it be kept within the limits of from five to twenty minutes for each experiment.

Tests in tension require the observation of five points:

First, *the elastic limit.*
Second, *the maximum strength.*
Third, *the ultimate strength.*
Fourth, *the elongation.*
Fifth, *the reduction of area.*

In order of importance, as well as in order of occurrence, the elastic limit possesses by far the greatest interest to the engineer and the investigator. The elastic limit, as usually understood, is that point at which the metal, under the influence of the stress to which it is subjected, takes the first appreciable set. In the previous autographic diagrams it will be observed that the famous principle, known as "Hook's law," that the strain produced in the piece was proportional to the stress causing it is certainly true within the elastic limit. Here it will be observed that as fast as the stress on the test piece increases the pencil of the registering cylinder draws a straight line slightly inclined to the axis of Y at a constant tangent. This straight line continues until the elastic limit is reached, when, by a sudden breaking down of the molecular structure, a sharp point of inflection occurs and the curve turns rapidly towards the axis of X. Several methods have been used for the determination of the elastic limit. Formerly it was customary to place upon the test piece two center punch marks, occupying a distance from eight to ten inches. A pair of divides were carefully set so as to exactly coincide with the center punch marks. The piece was then subjected to stresses increasing by preconcerted increments; after the application of each succeeding increment or stress was removed, the dividers applied to the center punch mark to ascertain whether the piece had indicated any permanent set. It will at once be seen that this was a comparatively rough method of making so delicate a determination. In fact, strains smaller than one hundredth of an inch are scarcely to be observed by any such method. It will be readily seen that unless the test piece is very long, so as to present a very marked increase of length at the first permanent set, the elastic limit, as determined in this way, is almost certain to be several thousand pounds too high. An improvement on this method is obtained by the employment of micronometer screws, one or more of which are attached to the specimen, and the strain produced by the stress, together with the resulting set, if any, are recorded by measurements made with these screws. The objection to micronometer screws is two-fold. In the first place, the time occupied in making thirty or forty readings on a single test piece is so great as to make the operation of testing exceedingly tedious and wearisome; second, considerable personal equation is almost invariably introduced in ascertaining if the screw makes a fair contact.

To overcome this, electrical and frictional contacts have been devised, so as to insure that the pressure applied to the screw head, by the operator was a constant and uniform quantity. These methods have succeeded to a great degree. At the same time the micronometer screw, except in the hands of a very skilled operator, is liable to a considerable percentage of error. Again, the micronometer screw has to be placed at such a distance from the axis of the specimen that the slightest bending or twisting on the part of the piece introduces very grave errors into the results obtained.

Several forms of vernier gauges have been contrived, which may be applied directly to the specimen, and readings of extension and set obtained with them. One of the most compact and easily managed of these may be seen in Fig. 18.

Determinations of elastic limit, either by the micronometer or vernier gauge, generally give results which are five to ten per cent. lower than those obtained by the use of dividers and center punch marks. Prof. Thurston has shown that intermittent applications of stress to a test piece, especially at or near the elastic limit, tends to raise that quantity higher and higher. The author has therefore become accustomed to make determinations

of the elastic limit, simply by applying gradually increasing increments of stress to the bar, and reading the vernier gauge at the moment when the rising of the beam indicated that the requisite stress had been reached. So long as the increments of strain are proportional to the increments of stress, it is assumed that the strain on the piece is within the elastic limit. The moment, however, that the strain becomes disproportionate to the increment of stress, it is assumed that the limit has been passed.

Another method of very rapidly determining the limit in a testing machine worked by screws, is obtained by carefully regulating the speed of the machine with reference to the motion of the poises along the beam, and watching the motion of the beam itself. As an example in the machine used in the Department of Tests and Experiments, the multiplication between the end of the beam and the platform of the machine is 24,000. That is, in order for the platform to move downwards one inch, the end of the beam would have to move 24,000 inches. If the cross head of the testing machine be carefully driven by the screws, so as to apply the stress to the piece, no faster than the poise travels out along the beam, the moment that the elastic limit is reached and the piece under the application of the stress suddenly elongates, the beam will quickly drop to the bottom of its surrounding guard, and remain there stationary for a few moments, until the motion of the cross head catches up to the flow of the metal in the test-piece. Experiments on test pieces of various kinds have shown that this method of obtaining the elastic limit is fully as accurate as that by means of either the micronometer screw or the vernier gauge. It, however, does not give the amount of elongation that the test piece has undergone, and so is comparatively useless in those investigations requiring that elongation, for the purpose of calculating the modulus of elasticity. The observations on the elastic limit are by far the most important as furnishing data for the guidance of the engineer and the architect in construction, for it is now a well-established fact that if material is never strained to a point approaching the elastic limit, it is safe for an indefinite life; consequently, the chief point to be observed in making experiments on materials is the careful determination of the elastic limit and the modulus of elasticity.

THE MAXIMUM STRENGTH.

After passing the elastic limit, the curve on the cross-section paper proceeds forwards gradually, inclining more and more towards the axis of X, and assuming a general parabolic form. After proceeding in this way for some time, the maximum strength of the material is reached. At this point the rapidity of the application of the stress is equaled by the flow of the molecules in the test piece. For a few moments the curve becomes approximately parallel to the axis of X, and the entire work done by the testing machine is consumed in moving the molecules from the outside of the test bar towards its interior. In a few moments, however, this rapid movement of the molecules is attended by contraction in the size of the bar. As the result of this contraction there are a less number of molecules in the cross section to resist the force applied to the test piece, consequently the stress begins to decrease, the flow of the molecules becomes more and more rapid, until at last the rupture of the piece is obtained. In previous experiments very little account has been made of the difference between the maximum and the ultimate stresses to which the test piece in tension is subjected. In fact, excepting in those machines in which the weighing is done automatically, this difference is hardly perceptible, and even when it is perceptible, it is difficult by hand to remove the weights from the testing machine with sufficient precision to catch accurately the ultimate stress at which the piece is ruptured. By means, however, of the autographic testing machine the entire behavior of the test piece may be readily observed. Generally the ultimate and the maximum are regarded as almost one and the same quantity. In steel and wrought iron it will be seen from the foregoing diagrams that they are exceedingly different. Whereas, in experiments made on brittle and hard materials, having little or no ductility, these quantities are so nearly the same as not to require any special mention.

LONG COLUMNS—A NEW DEMONSTRATION OF PROFESSOR ROBINSON'S FORMULÆ.*

By W. S. WESTON.

Contributed to Van Nostrand's Engineering Magazine.

Having lately published tables computed according to Professor Robinson's formulæ, the author feels justified in presenting in this article a new demonstration of formulæ which he believes to be perfectly rational.

Professor Robinson's column formulæ differ very essentially from other rational formulæ that have been presented hitherto in the introduction of a new relation which limits and gives a maximum value for the deflection of the column. A little thought will show that such a limit does exist. If a column is loaded it deflects; increasing the load increases the deflection. Is there not a limit to this deflection at which, if the load be further increased the column will fail? Professor Robinson called attention to this fact; determined the value of the limit, and then with obvious propriety used it in producing new formulæ for columns.

It has been objected by Professor Wm. H. Burr, in the November number, 1883, of this Magazine, that Robinson's and all other rational formulæ are vitiated by the assumptions of the common theory of flexure; and also that Robinson's are only Euler's formulæ with a mixture of errors. Hence, before proceeding to demonstrate and apply the limiting value of the maximum ordinate y_1 (the nomenclature in this article being the same as that used by Professor Robinson, see *Van Nostrand's Magazine*, vol. 26, page 490), it will be well to consider the effect of the assumptions of the common theory on the reliability of the formulæ, and to justify the use of Euler's formulæ.

The assumptions of the common theory of flexure are four in number: *First*, the co-efficient of elasticity is assumed to be constant; *second*, a plane section of a beam or column before flexure is assumed a plane after flexure; *third*, the resistance to compression is assumed equal to the resistance to tension; *fourth*, the reciprocal of the radius of curvature is assumed equal to the second differential coefficient. Other assumptions, differing in name, will be found on analysis to be but modified forms of these four. That for incipient flexure these assumptions represent actual conditions must be admitted by all. The question is, at what limit of flexure will they cease to be justifiable? It is contended that this limit is never reached in a safely loaded column. The following are the reasons:

By experiment we know the coefficient of elasticity is practically constant within a limit always recognized in loading columns. The error of the third assumption can not affect the formulæ, because the neutral axis is generally without the column, and, as will be shown, may never approach nearer the center than the radius of gyration. The second assumption is justified by our knowledge of the infinitesimal nature of the change in a section of a metallic column when safely loaded. The error of the fourth assumption can be computed and is known to be very small for small changes of curvature. This will be true for stable columns. For, a short column fails by crushing, mainly, while a long column fails by bending when, as will be shown, its deflection exceeds the radius of gyration; and hence the change of curvature in either case will be small.

One other assumption is made in the development of the new formulæ. A load on a column produces compression by virtue of its bending effect and also by virtue of its being transmitted through the column. It is assumed that the total resistance to compression is equal to the sum of the resistance to compression due to bending and the resistance to direct compression, these actions being conceived to take place independent of each other. There is no reason for questioning this assumption for strains within the elastic limit.

* This article was sent in for publication before the appearance of Professor Robinson's answer to Professor Burr in the April number of this Magazine.

With this brief discussion of the assumptions we are now ready to establish the old as well as the new relations employed in developing the new formulæ. For the sake of simplicity only columns with round ends will be considered.

Question has been raised as to the propriety of using Euler's formulæ

$$\frac{T}{\varepsilon I_1}=\frac{\pi^2}{l^2} \quad . \quad . \quad (1)$$

If this does not give the strength of a column, how can it be used to produce an expression which will?

Equation (1) is deduced from the general equation of moments,

$$\frac{\varepsilon I_1}{\rho}=Ty_1 \quad . \quad . \quad . \quad (2)$$

and results from the fact that the elastic curve of a stable column is a sinusoid. The elastic curve is that curve which the axis of the column takes when the material is nowhere strained beyond the elastic limit. These equations (1) and (2) are thoroughly theoretical and do not recognize the fact that the material has what may be called perfect elasticity only within very narrow limits. It is in the use of the quantity represented by the symbol ε that our trouble is to be found. In these equations, ε enters as an absolute quantity, whereas, it can not be and never was intended to be used otherwise than as a coefficient to denote the elastic nature of material within a very close limit. The coefficient of elasticity does appear in the new formulæ, but, as will be shown, in its true office of a coefficient. The quantity T in (1) and (2) may be looked upon as a load which, if the material has unlimited perfect elasticity, would give the column its elastic curve. It may also be concluded that so long as the curve of a column is the elastic curve, so long will (1) and (2) be true equations. But there is little in equation (1) to show how much a column will bear before its curve will cease to be an elastic curve. We know that not only the length of the column, but also the elastic limit, and a limit to the deflection, must be considered in determining the load.

It is plain then that neither (1) nor (2) can be used for a column formula. A legitimate and very proper use, however, is made of these expressions. It is desired that the curve of a loaded column shall always be the elastic curve. The above equations imply this condition. If then we can obtain from them a relation independent of ε, we shall be justified in using it in the solution of a new formula. Combining (1) and (2) gives

$$\frac{1}{\rho}=y_1\frac{\pi^2}{l^2} \quad . \quad . \quad . \quad . \quad (3)$$

Equation (3) is an equation of condition. When it is true, perfect elasticity exists, as shown above. When it is not true, perfect elasticity has been exceeded, and the column will be unsafe. Therefore, in view of these conclusions, and the fact that the elastic curve should be recognized, we are justified in the use of Euler's formula.

It will be admitted that the resistance to compression within the elastic limit should enter the formula. But, as noted in the fifth assumption, the compression consists of two parts. Let t equal the total resistance to compression; t_1, the resistance to compression due to bending, and t_2, the resistance to direct compression. Then,

$$t=t_1+t_2=Ty_1\frac{d_1}{I_1}+\frac{T}{K} \quad . \quad . \quad (4)$$

The value t_1 is found from the equation of moments,

$$Ty_1=\frac{t_1}{d_1}I_1 \quad . \quad . \quad . \quad (5)$$

It is now necessary to mark carefully the difference between equation (5) and equation (2). They are both true so long as the column preserves its elastic curve; that is, so long as the material is strained within its elastic limit. One equation recognizes this limit, while the other does not. Equation (2) implies, in the use of e, the existence of unlimited perfect elasticity; while (5), by the use of t_1, recognizes this quality only within limits well defined by experiment. We may say then with truth that T, as given by (5), is a much more reliable and a safer quantity than T, as given by equation (2).

To the relations expressed by equations (3) and (4), Professor Robinson has added a third. The limit and maximum value for the maximum ordinate y, may be proved in the following manner:

Take the moment equations,

$$\frac{\varepsilon I_1}{\rho}=Ty_1 \quad . \quad . \quad . \quad (6)$$

and

$$\frac{\varepsilon I}{\rho}=\frac{\varepsilon I_1}{\rho}+\frac{\varepsilon}{\rho}K\cdot\overline{BD}^2=T(y_1+BD). \quad (7)$$

in which BD is the distance from the center of gravity of the cross-section to the real neutral axis. Subtracting (6) from (7) gives

$$\frac{\varepsilon}{\rho}K\cdot\overline{BD}^2=T\cdot BD$$

Then dividing by BD and multiplying by y_1 we get

$$\frac{\varepsilon}{\rho}K\cdot BD\cdot y_1=Ty_1$$

But since in (6) $I_1=Kk^2$, k being the principal radius of gyration, we will have

$$\frac{\varepsilon}{\rho}K\cdot BD\cdot y_1=\frac{\varepsilon}{\rho}Kk^2$$

Therefore,

$$BD.y_1=k^2 \quad . \quad . \quad . \quad (8)$$

This is a true relation and is independent of the quantity T.

Equation (8) shows that the ordinate of deflection varies inversely as the distance of the neutral axis from the center of gravity of the section. For

$BD>k$, $y_1<k$; $BD=k$, $y_1=k$: and $BD<k$, $y_1>k$.

If the ordinate has a limit it must be sought by discussing the equation of moments.

In (7) the moment of internal resistance is placed equal to the moment of the external force. Note now the effect of varying the value of BD in this equation. First, taking $BD=k=y_1$, and substituting in (7), we find a true equality verified by (6). Second, taking $BD>k$, and hence $y_1<k$, we find that the first member of (7) becomes the greater, or that the internal moment is greater than the external. Third, taking $BD<k$, $y>k$, we find the first member the less, or the internal moment less than the external. But the moment of internal resistance should never be less than the moment of external force. Hence, the conclusion drawn from this discussion is this: BD may never be less and y_1 never more than k, and at the limit $BD=k=y_1$. In other words, the neutral axis of a safely loaded column may never approach nearer the center, nor the deflection be greater, than the principal radius of gyration of the cross section.

Equation (8) is an important relation, and may be deduced independent of the equations of moments. The resistance in the section of a column is undoubtedly analogous to the resistance of free bodies. When a free body is acted upon by an external force, it begins, by virtue of its resistance, to rotate about an axis which is called the axis of spontaneous rotation, or more properly the axis of *incipient* rotation. So also, when the resistance in the section of a column is brought into effect by an external force, the section will *begin* to rotate about an axis which passes through the neutral axis of the column. In both cases the axis of rotation is an axis of zero strain. Now, by a well-known principle of analytical mechanics, the distance from the center of gravity of the body to the axis of incipient rotation, multiplied by the distance from the center of gravity to the action line of the external force, is equal to the square of the radius of gyration. But BD is the distance from the center of gravity of the cross-section to the axis of incipient rotation, that is, to the neutral axis, and y_1 is the distance to the action line of the load. Therefore, $BD.y_1=k^2$.

Professor Robinson adopts at once the limit $BD=y_1$. It is proposed in this article, however, to wave for the present the choice of this limit, and to use in the further demonstration of the new formula the expression $BD = ny_1$, where n may be any value greater than unity. The effect of giving n its limit value *one* may be more thoroughty discussed when it appears in the column formula itself.

Let ad represent the shortening for a unit's length of the fibers which will fail first, and $(BD+d_1)$, the distance of these fibers from the neutral axis (see Fig. 21, *Van Nostrand's Magazine*, vol. 26, page 490). Then we shall have the proportion

$$ad : (BD+d_1) :: 1 : \rho.$$

From this, substituting for BD its value ny_1,

$$ad=\frac{ny_1+d_1}{\rho}$$

Now, remembering that the shortening ad is to be produced by stresses always within the elastic limit, we shall have for the resistance to this shortening.

$$t=\varepsilon . ad$$

Whence

$$ad=\frac{t}{\varepsilon}=\frac{ny_1+d_1}{\rho} \quad . \quad . \quad (9)$$

Since t has been determined by experience it is plain that the quantity ε enters this equation as a coefficient qualifying the value of the second member.

Combining (3) and (9) and solving for y_1 we get

$$y_1=\frac{d_1}{2n}\left(\sqrt{1+\frac{4ntl^2}{\varepsilon d_1^{\ 2}\pi^2}}-1\right) \quad . \quad (10)$$

Substituting this value of y_1 in equation (4), and transforming we obtain the formula for round end columns

$$\mathrm{T}=\frac{t\mathrm{K}}{1+\frac{\mathrm{K}d_1^{\ 2}}{2n\mathrm{I}_1}\left(\sqrt{1+\frac{4ntl^2}{\varepsilon d_1^{\ 2}\pi^2}}-1\right)} \quad (11)$$

Now it will be seen that for $n=1$, the formula gives a value less than it does for n equal to any quantity greater than one. Hence the choice of the limit $\mathrm{BD}=y_1$ will not affect the safety of the formula. This method introduces an approximation which, however, is quite small because of the position in the formula in which n is found. Omitting n, we get the formula as given by Professor Robinson.

In the same manner may be produced the formula for columns with ends fixed, and ends fixed and round.

It now becomes our duty to consider an objection which, so far as the writer knows, has not been made hitherto; but which is really the only well-founded objection to the new formulæ. It will be noticed that, although a maximum limit for y_1 has been determined by choosing $\mathrm{BD}=k=y_1$ the minimum limit of BD, and, therefore, $n=1$; yet y_1, as given in equation (10,) is still an unlimited variable dependent on the length of the column. There is then an objection to the unqualified use of (10), inasmuch as it does not recognize the fact that the deflection of the column may never exceed the radius of gyration.

It is obvious that the deflection before incipient failure is quite small for short columns, and also that this deflection will increase as the length until it reaches its maximum limit. If a column fails with its deflection less than the limit, we know that the failure was caused directly by a failure of the material; but in case the deflection is equal to or exceeds the limit before the material itself fails, we know the column was in unstable equilibrium as soon as its deflection passed the limit, and that therefore the failure was caused, primarily, by excessive deflection. Now the new formulæ are to be applied only to columns which will fail directly by a failure of the material. Hence, equation (10) may be used only so long as it will give a value of y_1 less than k. Substituting for y_1 and n their limit values k and 1, and solving for l, we shall obtain the length of column (in terms of k and d_1) which marks the limit of application of the formulæ. Fortunately, by giving to t the value of a safe working stress, the length as thus found is beyond the range of ordinary practice. The lowest limit of length belongs to solid round columns with round ends, and with $t=10{,}000$, and $\varepsilon=27{,}000{,}000$ is equal to $283k$. If we recognize, then, the limit of length for which y_1 will have a value less than k, there can be no objection to the use of equation (10). Although the formulæ are not completely universal as they were supposed to be, yet they are still sufficiently so for all kinds of columns having practical lengths.

Finally, from the foregoing demonstration and conditions there implied, we are prepared to draw the following conclusions:

First, the new formulæ are perfectly rational. The demonstration itself is the proof of this conclusion.

Second, the new formulæ can not, with justice, be compared with experimental results obtained in the usual method of breaking columns. It is a question whether practice would really desire the new formulæ to agree with the results of experiments for ultimate strength. It will be remembered that, in the demonstration, it was conditioned that the material should never be strained beyond the elastic limit. Under this condition the failure of a column is considered to begin at the moment when the elastic limit has been exceeded. This is exactly the condition which should be recognized in practice. With this definition of incipient failure, the new formulæ will give the strength of a column only when t is given a value within the elastic

limit. In the usual method of testing the strength of columns the material is strained to its ultimate resistance and considerably beyond its elastic limit. But the conditions near the point of crushing are quite different from those within the elastic limit, and from those belonging to an elastic column.

The irregular agreement of experimental and computed results, when t is given a value approximately equal to the ultimate resistance, does not prove or disprove the new formulæ, but, on the other hand, rather weakens the confidence in the usefulness of the experimental results for ultimate strength. It is believed that ultimate resistance is not so important a factor in practice as the elastic limit. The ultimate resistance should be looked upon only as a margin for safety in case of strain beyond the limit. If comparisons between the formulæ and the results for ultimate strength are made, t should be given a value greater than the experimental result, since the ultimate resistance of the material is necessarily greater than the ultimate strength of the column.

With the doubt as to whether the ultimate strength of experiment can be used with confidence, comes the doubt as to the reliability of empirical formulæ. This is but another proof of the superiority of rational to empirical formulæ.

Experiments may be devised to determine the greatest load which a column will bear and remain stable, and still retain its perfect elasticity. Such a method of testing is exemplified by that used in the testing of finished structures. A bridge is tested not by a breaking load but by a load which, according to the calculations, should not strain the material beyond the limit; and the return of the bridge to its former condition after the removal of the load is considered an evidence of its strength.

To sum up, it may be said of the new formulæ: they are perfectly rational; they apply only where the material is strained within the elastic limit, and therefore give the strength of an *elastic column*: and they are applicable to all kinds of columns and for all lengths within the range of ordinary practice, when t is given the value of a safe working stress—a value considerably within the elastic limit.

NOTES ON GEOMETRY, WITH ESPECIAL REFERENCE TO THE METHODS EMPLOYED.

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Written for VAN NOSTRAND'S MAGAZINE.

II.

GEOMETRY is justly considered one of the most exact of sciences, since the axioms and definitions that form its bases are regarded as such that every one is willing to admit, whilst the propositions are just deductions from the true assumptions.

It is evident, however, that if any of the hypotheses can be simplified, there will be a real gain in the perfecting of the science, which should be founded on the fewest number of things regarded as self-evident, especially if there should be the slightest suspicion that any of them may be contradictory. Again as to the methods of reasoning employed in deducing the various propositions from the given data, there is evidently choice, though modern authors have confined themselves so strictly to the deductive method, that the inductive or ancient analysis has scarcely received a passing notice from them. As the latter method is of such great value in scientific discovery, its complete exposition as a perfect method of reasoning, when rightly understood, is very important, and is well worthy of the most attentive consideration.

In developing the subjects under consideration, a brief exposition of general methods, applicable to all sciences of reasoning, is first given as naturally preceding the critical notes and applications to geometry.

Whatever of value is found in the following pages is due mainly to Duhamel, whose "Méthodes dans les Sciences de Raisonnement" has been freely used in the preparation; and it is hoped that none of the acute, searching thoroughness, so characteristic of that author, has been lost in the transfer.

Methods Common to all Sciences of Reasoning.

1. Necessary truths exist by themselves; reasoning and methods are only means that man employs to discover them or to recognize them, and their object is to produce in him the knowledge and *certainty* of the truths.

This state of certainty is caused in man by the clear conviction of the truth; in other words, as Descartes long ago observed, by the *evidence* or proof of it.

Truths whose obviousness occurs to all minds are used as the starting point, and the *methods* serve to discover other truths which can then be admitted with the same certainty as the first.

2. The ordinary form of the *syllogism* is a form of reasoning in which the characteristics belonging to a group are simply reaffirmed with respect to the individuals of that group, which is so simple that it scarcely needs giving a name to. It does not help to discover anything, for we must know or ascertain the qualities of the individual before we can affirm them for the group, and therefore it will not be included in the general exposition of methods.

3. The errors that we commit in reasoning proceed less often from a vicious deduction than from the inexactness of the propositions admitted, which are often, not those called *axioms*, which every one would admit from experience to be true, but rather plausible hypotheses, not so evident, which may be true for special cases, but are not so in all their generality, which, however, is often assumed as evident.

The only way to escape error in this direction is to examine carefully and perhaps tediously, every case that the statement involves, and verify it in each instance, or state the exceptions where the statement is not general, before proceeding with the investigation.

4. The operation of deducing a result from known results is called *deduction*; it corresponds to *synthesis*, and is the method generally used in communicating truths, that have been previously ascertained, to others.

5. The method of *induction* is the operation by which we refer the knowledge of a thing to that of other things of which it will be a consequence.

This corresponds to the *ancient analysis*, and is generally the march of discovery. It will therefore be fully developed as we proceed.

6. Propositions that cannot be true at the same time are said to be *incompatible*. Thus, to say that x is greater than y is incompatible with the statement that y is greater than x; but both statements can be false at the same time, which would happen if x were equal to y.

But if each proposition is the negative of the other, or *contradictory* to it, if one is true, the other, which denies it, will be false; and, if one is false, the other, which only denies it, will be true. We must be careful, though, not to omit any of the cases of the contradictory proposition, otherwise we should have a proposition that was simply incompatible with the first. Thus, if we assert that $x>y$, the contradictory proposition would assert that x was not greater than y, which includes two cases, viz., $x<y$, and $x=y$. If we simply said that $x<y$, the two propositions would be incompatible, but not necessarily contradictory, for both would be false if x equals y, so that we could not, if we recognized the falsity of one, thereby infer the truth of the other.

7. It must be carefully borne in mind, also, that truth may sometimes be deduced from falsehood, so that it does not follow because a deduction is true that the propositions from which it is deduced are true. They may be, one or more, false.

Thus, Aristotle gives the following odd illustrations, in the first of which the two premises are false, and the conclusion true:

Man is stone;
All stone is animal;
Then mankind is animal.

In the following, one premise only is false and the conclusion true:

All horses are animal;
Man is not animal;
Therefore, a man is not a horse.

Many illustrations could be given, but it suffices to remark that we may reach a true result from a false principle, either because the latter is a mixture of true and false, and we employ only the true, or that the errors made in the deductions compensate each other; thus it is proved that if we reach a true result, it does not follow that the relations admitted in deducing it are true.

8. It is equally important to observe, that if in going from certain relations we deduce, by just reasoning, a false conclusion, that *the first relations are not all true.*

For if they were we could only deduce true conclusions.

9. The science of a thing is the whole of the laws of that thing, so that the science of reasoning involves the laws of reasoning.

10. A *theorem* is a proposition to be demonstrated; a *problem*, something to be done, satisfying given conditions.

In a problem we indicate the result we wish to obtain and demand the means of attaining it. A theorem is true or false, a problem possible or impossible.

11. *Analysis.* The "*ancient analysis,*" which alone we shall consider in this paper, does not seem to be well known by modern logicians. We find the first traces of it in the elements of Euclid, though Pappus of Alexandria refers the invention to Plato.

The method can be applied both to theorems and problems, which applications will be considered separately.

12. *Analytical methods for the demonstration of theorems.*

When we have to find the demonstration of a given proposition we shall seek first if it can be deduced as a necessary consequence of admitted propositions, in which case it should be admitted; it will thus be demonstrated.

If we cannot perceive from what known propositions it can *be deduced*, we shall seek from what proposition not admitted it can be, and then the question will be to demonstrate the truth of the last. If this *can be deduced* from admitted propositions it will be true, and consequently the proposed will be; otherwise we shall seek from what proposition not admitted it *can be deduced*, and we then have to prove the truth of the last, if possible. We shall continue thus until we reach a proposition that may be recognized as true, and then the truth of the proposed will be demonstrated.

We see, therefore, that this method that we call analysis consists in establishing a chain of propositions, commencing with the one we wish to demonstrate and ending with a known proposition, so that in going from the first, each may be a necessary consequence of that which follows; whence it follows that the first is a consequence of the last, and therefore as true as it is.

13. *Definition.* When two propositions are so related that either one can be deduced from the other as a necessary consequence they are said to be *reciprocal.*

14. Now, if any two successive propositions, found after the general method above are reciprocal, we can consider the second as deduced from the first, and *if this reciprocity holds from the first to the last,* we can say that the analytical method consists in establishing a series of propositions of which the first is the one to be demonstrated, and such that the second is deduced from the first, the third from the second, and so on to the last proposition which is recognized as true.

15. It is by no means necessary that all the successive propositions be reciprocal; only if some are not so then the general method of art. 12 must be applied to such propositions.

If we employ the second manner of proceeding (art. 14), which is often easiest (as it is generally simpler to deduce a consequence from a proposition than to find another of which this will be a consequence), and do not ascertain that the successive propositions are reciprocal, if for only two, say; the second of the two can be true without the first being true, *since the truth can sometimes be deduced from error* (art. 7); so that we can not say that the first proposition is demonstrated.

Duhamel is the first author who has insisted on this reciprocity test, and it is well to call especial attention to it, for its neglect may lead to grave errors. Of course, where it does not obtain, the general method of art. 12 must be used.

16. *The analytical method for the solution of problems.*

The object of a problem is to determine one or more things of given kinds, so as to satisfy given conditions. If we

can refer the problem, then, to another whose conditions satisfy the first, so that the knowledge of the second comprehends at least some solutions of the first; if the new problem is easier to solve than the first, the question will be advanced.

If the second cannot be resolved, refer it to another as the first was referred to it, and so on until we reach a problem we can resolve. Its solution will furnish one or more solutions of the proposed.

17. Now, in this general method the successive problems have been so chosen that the conditions of any problem satisfy those of the preceding. Now, if the conditions of any one satisfy as well those of the succeeding one, the two conditions are said to be *reciprocal.*

If this does not obtain, the general method may not find all the solutions, so that there may be *lost* solutions in this case; for calling any problem (1), and the succeeding one, whose conditions satisfy those of the former (2), it is plain that a solution of (2) will give some of the solutions of (1) for the same conditions or requirements that satisfy (2) satisfy one; but some solutions of (1) may not be solutions of (2), for, by assumption, all the conditions of (1) do not necessarily satisfy (2), so that there will be more solutions of (1) than we should find by solving (2). But if, on the contrary, the conditions of the second problem and those of the first are reciprocal when the first is satisfied the second is also, therefore all the solutions of the first problem will be solutions of the second, and *vice-versa;* so that if we find all those of the second none will be lost.

Reasoning in the same way for the others, we can state generally the following proposition:

If in the problems that we substitute for the preceding, in going from the proposed, the conditions of any two consecutive ones whatsoever are reciprocal, the solutions of the first involve those of the last, and of any one whatsoever of the others, and reciprocally.

In the above reasoning we have not required that the things demanded be the object of research in each problem; but if they are different, we call the conditions of the two problems reciprocal, when knowing the things satisfying the imposed relations in either one of the problems, the corresponding things of the other satisfy the conditions imposed upon them in this other; in other words, when the complete solution of either problem entails the complete solution of the other.

18. It may conduce to clearness to mention the successive steps in the analytical solution of a problem given by Duhamel in his second book, which is too long to reproduce entire.

Problem—*To draw a circle tangent to three given circles.*

It is easily shown that the solution of this problem involves that of this problem, *to describe a circle which passes through a given point and is tangent to two given circles* and *vice versa.*

The two problems thus have reciprocal conditions, for the things satisfying the conditions of the one require as a necessary consequence that the corresponding things satisfy the conditions of the other.

We have thus referred the given problem to another whose solution embraces that of the first.

Similarly we refer the second problem to a third;

To describe a circle which passes through two given points and is tangent to a given circle,

whose solution involves that of the second, the conditions being reciprocal; when again, we refer the last to the problem,

To pass a circle through three points,

and with reciprocity.

It is to be understood that the constructions effected in referring the first to the succeeding problems have all been made, so that when the centre of the the circle corresponding to the last problem is found (which we know how to do), this is none other than the centre of the required circle, which is thus easily drawn tangent to the three circles, and the original problem is solved.

19. Let us next take the case, where the conditions of the new problem substituted for the first, are simply consequences of those of the first, but not reciprocal to them.

It follows that any solution of the first involves the solution of the second, since all that satisfies the conditions of the first, necessarily satisfies the conditions of the second; but *all* the solutions of the second are not necessarily solutions of the first, since the hypothesis is that the con-

ditions are not reciprocal, and it will therefore be possible to satisfy the conditions of the second without satisfying those of the first. The second problem in this case has, then, *all the solutions* of the first, but it may contain *strange* solutions in addition.

20. REMARK.—The writer is constrained to think that the reasoning of Duhamel in the preceding article is obscure and possibly requires elucidation. First, it seems to be tacitly assumed that the second problem substituted for the first is not simply *a particular case* of the first, for then the solution of the second might not include *all* the cases of the first, but that the conditions of the second are just as comprehensive as those of the first and include every case of the first, so that the solution of the second gives every solution of the first. But then it may be asked, why should it contain more, if it is *simply* a consequence of the first? How can a consequence, if it is simply a consequence and nothing more, include more than the original proposition?

The truth is, that in the examples illustrative of "strange solutions," it will be found that the so-called consequence, is a consequence only for certain values of the quantities entering it; whereas, in solving it, we tacitly assume it to be true for all values of its quantities, so that we find strange solutions to the original problem.

In fact the assumption, in art. 19, that the old and new conditions are not reciprocal, implies that the new conditions are more comprehensive than the first, and that only part of them are direct consequences of the first; for if all were, there would necessarily be reciprocity. For these reasons, I prefer to put the deductions of article 19 in the following form:

When the conditions of the old and new problems are such, that *all that satisfies the first satisfies the second, but not reciprocally*; then the complete solution of the second problem will certainly give all the solutions of the first, with perhaps *strange solutions*, if the conditions of the second problem are more general than those of the first and not all strictly consequences of the first. The solution of the following problem will elucidate what has been said:

21. PROBLEM.—*Knowing the hypothenuse of a right triangle, the sum of the other two sides and the altitude, to compute the last three quantities.* Call a this sum, b the hypothenuse regarded as the base; x and y the two sides, and z the altitude. The known properties of the triangle and the relation imposed in the enunciation give the three following equations:

$$x+y+z=a \quad . \quad . \quad . \quad . \quad (1)$$

$$xy=bz \quad . \quad . \quad . \quad (2)$$

$$x^2+y^2=b^2 \quad . \quad . \quad . \quad . \quad (3)$$

Thus we refer the geometrical problem to one of numbers. To solve the latter, transpose the z in eq. (1), and squaring, we have,

$$x^2+2xy+y^2=(a-z)^2 \quad . \quad . \quad . \quad (4)$$

subtracting eq. (3) from this, and double eq. (2) from this result we obtain, on reducing,

$$z^2-2\,(a+b)\,z+a^2-b^2=o \quad . \quad (5)$$

whence,

$$z=a+b\pm\sqrt{2ab+2b^2} \quad . \quad . \quad (6)$$

But it would evidently be wrong to take the solution that corresponds to the + sign of the radical, for z, the altitude of the triangle cannot be greater than $a+b$; whence substituting the other value in (1) and (2), we find the values of x and y by elimination between the first three equations.

But, we can ask, why have we found two values for z when one only applies, the other being "strange" to the solution. Duhamel says, "now we have truly demonstrated, that eqs. (1), (2) and (3) were *consequences* of the geometrical conditions, but not the reciprocity, whence the strange solutions." The truth is (1), (2) and (3) are not *simply* consequences of the geometrical conditions, but enclose in themselves other solutions, *for if x and y are both written with the minus sign*, we shall obtain the same value of z, eq. (6) just found. This is plain, for eq. (4) can equally be written $(x+y)^2=(a-z)^2$, or $(x+y)^2=(z-a)^2$ and the results of the calculation will be the same. Therefore, eqs. (1), (2) and (3) satisfy not only the geometrical conditions of the problem, but likewise another problem of numbers, in which x and y are both negative at the same time.

22. The conclusions arrived at concern-

ing the solution of problems may then be summed up as follows:

1° If the conditions of the successive problems to which we refer the proposed are reciprocal, in order, beginning with the proposed, the solutions involve each, the others.

If the successive conditions, in order, are not reciprocal, then:

2° If some of the conditions relative to any one whatsoever of these problems are simply consequences of those of the *following*, all the solutions of any one whatsoever are solutions of the proposed, but may not include them all.

3° If some of the conditions of any one are consequences of the *preceding*, so that what satisfies the latter will satisfy the former, the solutions of any one whatsoever comprehend all those of the proposed, and may include strange solutions.

If several of these cases hold successively in the solution of the problem we apply the proper principles in turn to each case.

23. The synthetical method.—By this method, for the demonstration of theorems, we start with known propositions and deduce the others successively as consequences until we arrive at the theorems proposed, which is thus recognized as true.

Similarly in the resolutions of problems we start with a known solution and deduce a new one from it, a new one from this and so on, until we reach the proposed, whose solution is thus obtained.

24. The synthetical method is generally used in communicating results to others, and often in discovery, whether deducing consequences from known results at random, or by intelligently combining known results with a view to a certain end. As it is often difficult to know from what known problem or theorem to start, we can often make a series of fruitless essays before hitting upon the proper solution.

The analytical method possesses the advantage of a starting point, as we know from what result we have to go to seek one from which it is to be deduced; and this is often a great advantage, even in communicating results; for by the synthetical method, the reader often sees no object in the various steps of the demonstration or solution, until the end is attained.

No rules can be given as to the best method to employ—sometimes both may be employed in the same investigation—for by *deducing* certain results having more or less analogy to the proposed, we stand some chance of applying them in the course of the analytical march.

25. The ancient authors did not perfect the analytical method. Thus Euclid and Pappus both regarded a proposition as demonstrated when they could deduce from it a proposition known to be true. The examples solved by them carry out this idea, which is evidently defective, since we have seen that false premises can sometimes lead to true conclusions. After reaching the true proposition, if they had then reversed the order and verified the preceding results synthetically, the results would be proved true; or still better, if they had shown the reciprocity in the successive theorems or in the conditions of successive problems, the results would be true without the necessity of a synthetical verification. It is little wonder that the analytical method should be treated with so little favor, when its limitations and requirements were so imperfectly perceived.

26. How to demonstrate that a proposition is false.—If we can deduce by just reasoning from a proposition, temporarily admitted, a false proposition, or one *incompatible* with any of the preceding, the assumed proposition is false; for if the first was true, all those deduced from it would necessarily be true, and could not consequently offer any incompatibility with it or any of the others.

This method is analytical, in that it takes for a point of departure the proposition in question and goes to some desired result, whose truth or falsehood thus establishes that of the first proposition.

27. Reductio ad absurdum.—Two propositions, of which each is the simple negative of the other, are contradictory (art. 6), and consequently, if one must be true, the truth of one involves the falsity of the other and reciprocally; so that if we can prove that the contradictory proposition to one is false, that one will be demonstrated true, provided that every contradictory case is stated. This method, called the "reductio ad absurdum," simply amounts to this:

Let A, B and C represent *all* the possible cases of a certain proposition, *one*

of which must be true. Now if we can prove that cases B and C are false (art. 26), then it must follow logically, that A is true, for one must be true, and case A is the only one not discarded as false. The latter method of putting the case is seized much more readily by students than the former, and amounts to the same thing as B and C are here contradictory to A, so that the first demonstration applies with the same results in both cases.

We have now given a rapid resumé of the laws of reasoning, and will now proceed to their application to geometry.

Application of General Methods to Geometry.

28. We are met at the threshold of the science of geometry with difficulties relative to proper definitions of the straight line and plane.

Euclid defines a straight line as one which rests evenly on its points, and adds, as an axiom, that two straight lines cannot enclose a space. The definition gives but little meaning, and should be rejected.

Modern authors define a straight line as the shortest distance between two points, and assume that any two straight lines can be made to coincide, besides often giving as an axiom a third property, that a straight line has the same direction throughout its whole extent, without inquiring whether some of these requirements may not be incompatible. Duhamel evidently takes a hint from Euclid's axiom, and defines *a straight line as such that through any two points we can only pass one straight line.* It follows that such a line will coincide with any other straight line when it has two points in common, and that the form of the line is everywhere the same.

This is a precise definition, but it does not immediately give us an idea of the form of the line meant by it.

If we define a straight line as one having everywhere the same direction, we form an idea at once of the kind of line meant, though the word direction must then be defined, as it is often associated with the path of a certain course on the earth's surface. It would be more precise to define *a straight line as one every point of which is in line with any two points of the line assumed at pleasure.* We sight along a straight edge to see if it is "straight," or satisfies the above condition, and this is really the first and most lasting impression we get of a straight line. Something has to be assumed as the result of observation or experience, no matter what definition is used. If we employ the last definition then it follows as a consequence that but one straight line can be drawn between any two points, which thus includes Euclid's axiom as a consequence, as well as Duhamel's definition, and the further fact that two indefinite straight lines coincide throughout their whole extent when they have two points in common. Accepting one of these definitions it should be next in order to *prove* that a straight line *is* the shortest distance between two points, and not assume it, as is done by the followers of Legendre. We shall enter into this demonstration directly, after defining a plane and showing some of its characteristics.

29. The usual definition given of a plane—such a surface that a straight line connecting any two of its points will lie wholly within or on the surface—is certainly the one derived from experience, for on looking over a smooth sheet of water we notice that it looks straight from point to point. Any one trying a straight edge on a blackboard will rest satisfied that there is such a surface, so it may be pertinently asked why not accept the definition, since we know that there is just such a surface as is described by it.

There can be no objection, that I see, from a practical point of view, but it is very desirable in pure theory to assume as little as possible, especially if the thing can be deduced from previous assumptions and axioms.

Therefore Duhamel conceives a plane to be generated by a straight line revolving about a point in another straight line and remaining always perpendicular to it. It is very evident that such a surface can exist, and we have only to show that a straight line passing through any two points of it lies wholly within the surface.

To prove this, conceive the surface completely turned over, so that the fixed straight line shall be in continuation of its first position, then if the point, the intersection of the perpendiculars to the

fixed line, is the same for the two surfaces they must necessarily coincide, since both are the loci of perpendiculars to the same fixed line at the same point. [For the same reason, if we revolve either surface about the fixed line it will coincide throughout with the other surface, though this fact is not necessary to the demonstration below.]

Now pass a straight line through *any* two points of the common surface, it will coincide with the surface throughout its whole extent. If you deny it, then it must leave the surface somewhere, either on the one or the other side; but any reason that may be given for its leaving the first surface on the side of the fixed line or the reverse, will apply equally well to the second surface, so that the line would have to be on both sides of the common surface at the same time, since the fixed line (or directrix) for one surface is on the opposite side of the common surface to that for the other surface.

Hence, as the straight line drawn through any two points of the plane must either lie in it entirely *or not*, and we have found the latter supposition absurd, the fact must hold (article 27).

It must follow, likewise, that any indefinite straight line which passes through any point outside of this locus (the plane surface), can have not more than one point in common with it, for if it had two, all its points would coincide with it, which is contrary to the hypothesis.

30. From the property just proved of the plane we can easily show that any two planes having any three common points, not in the same straight line, will coincide throughout. For, draw a straight line through two of the points, it will coincide with both surfaces throughout, and if we conceive an indefinite straight line to revolve about the third common point, always touching the line drawn, it will coincide with *both* surfaces at the same time in all its positions, since it has two points in common with them; therefore the two planes present a common surface, and coincide throughout.

31. If preferred, a plane may be defined as a surface generated by revolving a straight line about a fixed point, and always touching a fixed straight line not passing through the point. For, reversing the surface, and reasoning as before (29), we prove that a straight line will coincide with it from point to point, whence the conclusion of art. 30 holds.

32. We have given the reasons why the definition, that "the straight line is the shortest between two points," should not be accepted. It is a thing to be proved in due course, and we shall now give the few theorems, necessary to prove it and which should form some of the first theorems of any treatise. The demonstrations are taken mainly from Euclid, who did not fall into the error of modern geometers of assuming the principle in question when it was susceptible of proof. As the reader is supposed to be versed in geometric processes the demonstrations will be made very brief.

33. Using Duhamel's definition, that a straight line is such that but one straight line can be drawn through two points; or if preferred, the other that I suggest, that a straight line is such, that every point of it is in line with any two points of it whatsoever, which includes the above definition, we easily show in the usual manner, that,

If two straight lines cross each other, the vertical or opposite angles are equal.

Also, by superposition, it is shown in the usual manner that,

Two triangles are equal when they have an equal angle comprehended between two sides equal each to each.

34. Next we shall show, that,

The exterior angle of a triangle is greater than either of the opposite interior angles.

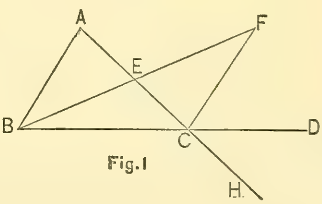


Fig.1

Let A B C (Fig.1) be a triangle, and ACD the exterior angle that we wish to prove greater than either BAC or ABC. Take E the middle of AC, draw BE and extend it to F, a distance EF = BE. Then the triangle AEB is equal to the triangle CEF (article 33) having the opposite angles at E and including sides, equal each to each. Therefore the angles A and ACF are equal; but as ACF

is less than ACD, from the construction, it follows that angle ACD is greater than the opposite interior A.

In a similar manner, by bisecting BC, &c., we can prove that the exterior angle BCH is greater than ABC; therefore, since BCH=ACD, (33), we conclude that any exterior angle is greater than either of the opposite interior, Q.E.D.

35. We shall next show that,

In an isosceles triangle, the angles opposite the equal sides are equal.

In the triangle ABC, let AB = AC (Fig. 2),

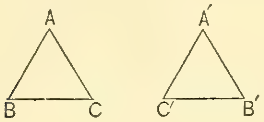


Fig.2

Turn the triangle ABC over, and suppose it to take the position A′B′C′, the angle B falling at B′, C at C′. Now slide the second triangle so as to coincide with ABC, which it can be made to do completely, since the angles A and A′ are equal, side AB=A′C′ and AC = A′B′. Therefore, the angles B and C′=C, are equal, which was to be proved.

36. *If two sides of a triangle are unequal the angle opposite the greater will be the greater.*

Let AC>AB (Fig. 3); take AD=AB, the triangle ABD will be isosceles, and

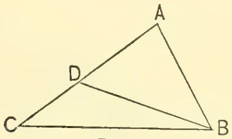


Fig.3

consequently, the angles ABD and ADB will be equal (35). But the angle ABC> ABD=ADB; and ADB>C (34): then for a stronger reason, ABC>C, which was to be proved.

37. *If two angles of a triangle are unequal the greater side will be opposite the greater angle.*

There are only three possible cases and one of them must be true; either the side opposite the greater angle must be less than, equal to or greater than the other side.

If it was smaller, the angle opposite would be the smaller of the two (36), which is contrary to the hypothesis.

If it was equal to the other side, the opposite angles would be equal (35), which is likewise contrary to the hypothesis. These two suppositions being false, the only remaining one, which agrees with the enunciation, must be true.

38. *In any triangle, any side whatever is smaller than the sum of the two other sides.*

In the triangle ABC (Fig. 4), prolong BA so that AD=AC; the triangle CAD will be isosceles and the angles D and

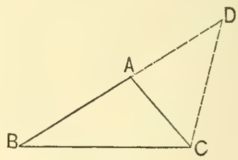


Fig.4

ACD will be equal; it follows that BCD> D, and consequently, in the triangle BCD the side BD will be greater than BC (37). But BD=BA+AC, hence the sum of the two sides BA, AC of the triangle ABC is greater than the third. Q.E.D.

39. Corollary.—It results from this, *that a straight line is shorter than any broken line having the same extremities.*

In Fig. 5, we wish to prove, that the straight line AB is shorter than the broken line AEDCB. Draw the diagonals AC, AD, then by (38)

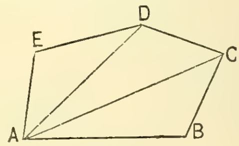


Fig.5

$$AB<BC+AC$$
$$AC<CD+DA$$
$$AD<DE+EA$$

Adding and cancelling like terms we find that,

$$AB<BC+CD+DE+EA$$

which was to be proved.

We cannot take up the subject of limits, in this short discussion; but regarding any curved line as the limit of any inscribed broken line, as the length of

each side becomes smaller and the number of sides increases indefinitely, we easily prove from the result above that a straight line is shorter than any curved line having the same extremities. So that we can state, generally, as a deduction from known principles, that *a straight line is the shortest between two points.*

40. It is evident now that there is no simplification, even in Legendre's method, of assuming the principle just proved; for the propositions necessary to prove the principle are theorems that should be proved in any case, hence we repeat that the unnecessary assumption should be rejected.

In fact it is very desirable to deduce all the theorems in geometry from the fewest possible assumptions, and those, too, so evident that every one recognizes their truth without hesitation; and this was plainly the aim of the older writers, who were obviously impelled to it by the many objections of the sophists; who thus incidentally assisted in making geometry as perfect a science as it is.

It would lead us too far to allude to some other minor objections to existing discussions of certain geometrical principles, we therefore resume the consideration of the application of the general methods previously exposed.

41. The proof for the theorems just given follows the synthetical method, which gives the results so quickly that nothing better can be desired.

We shall give some applications now of the analytical method, which in many cases it is more desirable to follow than the other.

Theorem. *Similar triangles are to each other as the squares on their homologous sides.*

We are to prove that the areas, or half the products of the bases by the altitudes, are in this ratio.

Call a and h, respectively, the base and altitude of one triangle; a' and h', the base and altitude of the other. Then we are to prove the following proportion.

$$ah : a'h' :: a^2 : a'^2,$$

which entails the following, on cancelling the common factors,

$$h : h' :: a : a';$$

and, *reciprocally*, since the first proportion can be deduced from the last.

Now the last proportion is true, since in two similar triangles the altitudes are as the sides, therefore the theorem announced is true (Article 14), since it can be synthetically demonstrated from the last mentioned known result.

Contrast the usual demonstration by deduction, as given in the books.

42. The following little exercise is taken, almost at random, from a textbook.

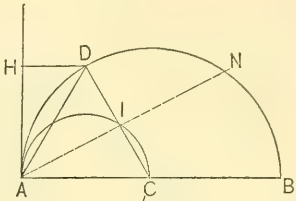


Fig. 6

Having described the semicircle AIC, on half the diameter of semicircle ABD, as shown in Fig. 6, and drawn the line CD from the center of ADB to any point D on its circumference, intersecting the smaller circumference at I, to prove that DI=DH where DH is the perpendicular from D to the common tangent at A.

Now, by analysis, we start with the relation DI=DH, we have to prove, and refer it, if possible, to another easier to prove. Thus we see at once, drawing the line AIN through I to intersection with ADNB, that AID is a right angle, being equal to AIC, an angle inscribed in the semicircle AIC; so that the two right triangles AHD and AID, having a common hypothenuse, would be equal (whence DH would equal DI) if we can only prove that the acute angles HAD and DAI are equal. We thus refer the proof of the original proposition to proving the equality of two triangles, and this in turn to proving the equality of two angles, and it is plain that the propositions are reciprocal.

Now the angles HAD and DAI are equal, the first being measured by one-half the arc AD, the second by one-half the arc DN; which arcs are equal, because CD being perpendicular to the chord AN bisects the whole arc ADN at D. Therefore the proposition is proved.

This going from the thing to be proved to known relations is much more natural, in the march of discovery, than the synthetical method in this case. By

the latter method of deduction we should first notice all the relations that had been previously proved about the lines in the figure, when by combining certain of them we could deduce the theorem.

The contrast in the two methods is much more apparent, though in more complicated exercises, where a number of successive steps have to be made, but we cannot enter into them.

43. There are certain other *methods* sometimes used in the resolution of problems.

It is sometimes easier to construct a system *like* the one we wish, and afterwards reduce it to the proper dimensions ; or it may be desirable to construct a part of the new system by itself to an assumed scale and complete the construction by putting the given lines in to the same scale and afterwards reducing.

The modern graphical statics and its applications afford many examples.

44. Another expedient is the *method of geometric loci*, or finding the position of a point by tracing a line, which is the locus of the point when one condition of the problem is omitted, and noting its intersection with another line on which the point should be found likewise. The following problem is an example, though it is not the best solution of this particular problem.

Given the base = BC (Fig. 7), the length of the altitude=MH, and length =AN, of the line bisecting the angle opposite the base, from the vertex to the base, to construct the triangle.

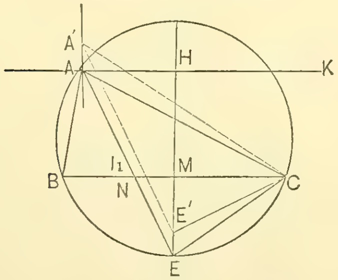


Fig.7

If we draw a right-angled triangle with the altitude for one side, and the length AN for the hypothenuse, we get at once the inclination I of this bisector to the base. The problem is to find the vertex A. It is, of course, on the line HK parallel to the base, and at the distance HM from it. Suppose the problem solved, and that ABC is the required triangle, about which circumscribe a circle, then extend AN to E, its intersection with the circumference, and draw EH perpendicular to BC, intersecting it at M. The last named point is the middle of BC, since arc BE=arc EC, being double the measure of the equal angles BAE and EAC respectively. Also, angle I=angle ACE, since double their measures are arcs (AB+EC) and (AB+BE) respectively, which arcs are equal; therefore it looks probable that the last relation may give a locus for point A.

Thus, from some point E′ on the indefinite line HME′, passing through the middle of BC and perpendicular to it, draw E′A′, making the required angle I with the base, and lay off at C the angle E′CA′=I; the intersection of the dotted lines A′, will give a vertex for a triangle BA′C (not drawn entirely) that satisfies every condition but that of altitude of the required triangle.

For conceive a circle to pass through B,E′ and C ; it will likewise pass through A′, otherwise it will cut the lines E′A′ and CA′ in some points F and G.

Then, since angle E′CA′ was laid off equal to I, their respective measures are equal; whence

$$\textit{arcs}\ GB+BE'=FB+E'C,$$

$$\textit{arc}\ GB=\text{arc}\ BF.$$

That is, the points F and G must coincide, which is only possible when they are identical with A′, therefore the circumference that passes through BE′ and C will likewise pass through A′, hence we see that A′E′ is a bisector of the angle BA′C, so that triangle BA′C satisfies every condition but height.

It follows that taking other points as E′ on HE, and constructing the corresponding angle at C equal to I, and finding the intersection of the lines such as E′A′ and CA′ that we have a locus A′. . . for the vertex with one condition omitted. The intersection of this locus with HK produced gives A the vertex required.

45. *Application of Algebra to Geometry. Equations corresponding to Geometric Loci.*

We shall now, rather abruptly, enter the domain of co-ordinate geometry

whose origin is due to Descartes, at least when the science is considered in all its generality.

Geometric loci are here represented by an equation between the co-ordinates of each of its points, these co-ordinates being supposed to be laid off in opposite directions when they have unlike signs. Thus if $+x$ represents a distance laid off to the right, from a given origin, along a certain line; $-x$ will represent the same distance, laid off to the left, from the same origin, along the given line, a conception that forms the base of the modern algebra of algebraic numbers. Unfortunately Duhamel has made no use of this algebra, and thus is constrained to remark, after verifying the generality of an equation for both + and — values of the variables, that from an algebraic point of view, there is no sense in these operations upon minus quantities; but that applying the usual rules of multiplication of polynomials, etc., to them, they can be treated as real quantities, so far as giving the greatest generalization to the results is concerned.

This limited view of the subject we do not concur in, and refer to modern treatises on algebra, or to a previous article on algebra, by the writer, where the theory pertaining to the algebraic series of numbers is logically developed from comprehensive definitions given to + and — in connection with elementary principles.

However, this very generality, obtained by conceiving the variables to have both plus and minus values, in the equations of geometric loci, should cause the greatest care to see that no strange solutions are introduced by too great generality, or that none are lost by introducing too many restrictions. These discussions are sometimes difficult, but if we neglect them we are not completely assured of the result.

It is with such interesting discussions that we purpose to continue the subject of the application of general methods previously exposed.

46. Let us propose first to find *the equation of the locus of points such that the ratio m of their distances to a point and a given straight line may be constant*, a condition expressed, for the following figure, by the equation,

$$AM=m.\ MP. \quad . \quad . \quad (1).$$

A representing the given point, and BD the given straight line.

Assume A as the origin, and let the axis of x be perpendicular to BD, the axis of y parallel to it, and call the distance $AB=a$.

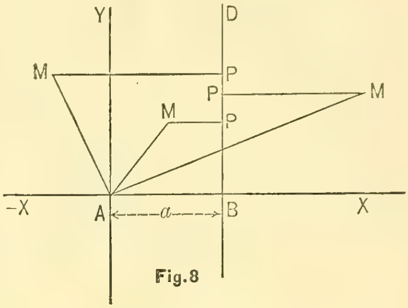


Fig. 8

Now, whether x and y are + or —, that is, wherever the point M is, we have

$$AM=\sqrt{x^2+y^2}.$$

As to MP, when M lies between AY and BD, $MP=a-x$; when M is to the right of BD, $MP=x-a$; finally, when M is to the left of AY, since x is essentially negative, we write, $MP = a - x$, though $(a-x)$ here is really an arithmetical sum.

The condition, eq. (1), is then replaced by the two following, the first referring to points to the left of BD, the second to points to the right of BE:

$$\sqrt{x^2+y^2}=m(a-x)$$

$$\sqrt{x^2+y^2}=m(x-a),$$

whether the point M is above or below AX.

Now if we square these equations, they can both be expressed by a single one.

$$x^2+y^2=m^2(a-x)^2 \quad . \quad . \quad (2),$$

which is thus the equation for the geometric locus required, for it includes every position of the point M, giving the corresponding value of y for any assumed value of x. There are thus no solutions lost; neither are there strange solutions; for in squaring the two equations above the result is the same if both radicals are negative as well as positive, as assumed, and the effect of changing the signs of the radicals above from + to — would simply transform the right

member of one equation into the right member of the other; so that the final result (eq. 2) will be the same as before; so that equation (2) of the locus contains no strange solutions.

47. *Equation of the locus of points such that the sum of their distances to two fixed points may be constant.*

Let F and F′ (Fig. 9) be the two points, $2c$ their distance apart, and $2a$ the constant sum.

Let us take for the axis of x the straight line FF′; for the axis of y the perpendicular to FF′, erected at its middle A, and call x, y the co-ordinates of any point M whatsoever of the locus.

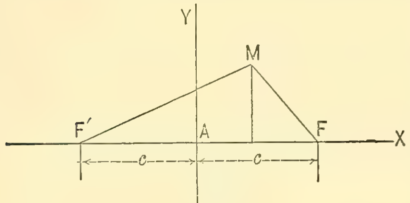


Fig. 9

The condition to which it is subjected will be expressed by the equation,

$$FM+F'M=2a,$$

or,

$$\sqrt{y^2+(x-c)^2}+\sqrt{y^2+(x+c)^2}=2a \qquad (1)$$

the co-ordinates having signs corresponding to the position of the point M. The formula is evidently true for any of the positions of M.

On changing the second radical to the right member, squaring and reducing, we find,

$$a^2+cx=a\sqrt{y^2+(x+c)^2}$$

Squaring again and reducing, we have

$$a^2y^2+(a^2-c^2)\,x^2=a^2\,(a^2-c^2) \qquad (2)$$

Now we should get the same equation, by this repeated squaring, no matter what the signs of the radicals in (1); so that although (2) comprehends the locus required it may contain some loci "strange" to the question.

To examine into the other solutions, we observe first that we must exclude the case of minus signs to both radicals at once; but one radical may be + and the other —, with the same result, eq. (2) above. *This corresponds to the difference of the two distances FM and FM′, no matter which is the greater.*

We should naturally infer that (2) represents two loci at the same time, the one corresponding to the condition $FM+F'M=2a$, the other to $FM-F'M=2a$; but it is easily seen that this cannot be, for since $FM+F'M>FF'$ or $2a>2c$ or $a>c$, for the first locus, in the second we must have $2a<2c$, since in any triangle the difference of two sides is smaller than the third. Therefore if, $a>c$ we have, placing $a^2-c^2=b^2$

$$a^2y^2+b^2x^2=a^2b^2$$

for the equation of the first locus, called an *ellipse*, and if $a<c$, placing $c^2-a^2=b$,

$$a^2y^2-b^2x^2=-a^2b^2$$

for the equation of the locus of points such that the difference of their distances from F and F′ may be $2a$, a curve known as the *hyperbola*. We thus have incidentally found the equation of another locus than the one corresponding to the original enunciation. We see plainly that it is introduced by the elimination of radicals, so that eq. (2) the so-called "consequence" of (1) (see articles 19 and 20) is really more comprehensive and includes more than the consequence of the original enunciation.

48. *Equation of the locus of points such that the sum of their distances to a point and a fixed straight line may be constant.*

Let F and UV (Fig. 10) be the point and straight line respectively; AX, per-

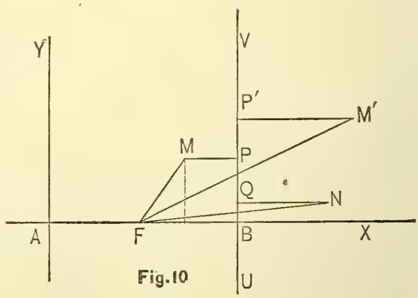


Fig. 10

pendicular to UV, the axis of x, and AY perpendicular to AX, the axis of y, drawn through a point A sufficiently far, so that all the points M that we shall consider may be on the same side of this axis.

Let x and y be the co-ordinates of any point M of the locus, situated between UV and AY; m the given sum, FB$=a$ AF$=b$, the condition of the question is expressed by the equation FM+MP$=m$, or,

$$\sqrt{y^2+(x-b)^2}+(a+b-x)=m \quad (1)$$

and we see that we evidently have $m>a$ for this case. But if we transpose $(a+b-x)$ to the right member and square both sides we see that we shall obtain the same result, if eq. (1) was written,

$$-\sqrt{y^2+(x-b)^2}+(a+b-x)=m,$$

which corresponds to the geometric locus, expressed by the condition, MP$-$FM $=m$; for which, however, we evidently always have $m<a$. Rationalizing, either equation reduces to the following,

$$y^2-2(m-a)x=(m-a)^2-2b(m-a) \quad (2)$$

which is the equation of a *parabola*, which thus evidently corresponds to two loci; the one when $m>a$ answering to the original problem, the other when $m<a$, corresponding to a new geometric problem. But so far, we have only considered points to the left of UV. For points to the right $x>a+b$ and the first member of (1) becomes the difference FM$'-$M$'$P$'$. Calling this difference m, we have for points to right of UV, $m>a$, as is evident from the figure. Equation (2) which embraces this locus, also corresponds to the case when the radical is minus, *i. e.*, F M$'$ $-$M$'$P$'=-m$; but as this has no sense, we exclude it.

Equation (2) thus corresponds to two geometric loci for points to left of UV, according as m is greater or less than a, and one locus for points to right of UV, which requires that m be greater than a. As this equation then, for points to right of UV, does not correspond to the original enunciation, let us find the equation that does. Calling x, y the co-ordinates, of a point N such that FN + NQ $=m$ there results,

$$\sqrt{y^2+(x-b)^2}+x-a-b=m \quad (3)$$

which becomes,

$$y^2+2(m+a)x=(m+a)^2+2b(m+a) \quad (4)$$

It is easy to see that eq. (3) which satisfies the geometrical condition where $x>$AB, does not satisfy it, if we take $x<$ AB, for then the first member becomes the difference FM$-$MP, which corresponds to the second case mentioned as belonging to eq. (2).

The problem proposed cannot then be completely resolved, except by two equations, and each of the two represent loci, of which a part belongs, whilst the other does not belong to the question, and yet it is impossible to separate them.

This interesting example should teach caution in assuming that an equation deduced to represent the position of certain points is necessarily general or corresponds to all points of the locus.

49. In the preceding cases the strange solutions have been introduced by the elimination of radicals. They may likewise be found when an equation contains a term of the form $\dfrac{F(x,y)}{\varphi(x,y)}$, and we multiply both members by $\varphi(x,y)$. The new equation will admit necessarily of values that annul $\varphi(x,y)$ and $F(x,y)$ at the same time. If these solutions pertain to the question, there are no strange solutions introduced by the multiplication; otherwise there will be, and it will be impossible to suppress them.

A case in point is the equation of the conchoid.

$$\frac{y}{\sqrt{y^2+x^2}}=\frac{y-a}{m} \quad . \quad . \quad (1)$$

Making $x=o$, we find $y=a\pm m$; but if we clear of fractions, the values $x=0$, $y=o$ satisfy the equation, but not the geometric condition (as is well known), and is thus strange to the question. The left member of eq. (1) takes the form $\frac{0}{0}$ for these values.

50. *Locus of all the points from which we can view, under the same angle, a straight line of given length and position.*

Let BB$'$ (Fig. 11) be the given straight line, $2a$ its length, and t the tangent of the angle under which it must be viewed from any point M of the locus.

Take BB$'$ for the axis of x, and the perpendicular erected at its middle for the axis of y.

The equations of the straight lines drawn from B and B$'$ to M, calling m and m' the tangents of a and a' and a and a, the intercepts on the axis of x, are,

$$y=m(x-a), y=m'(x+a).$$

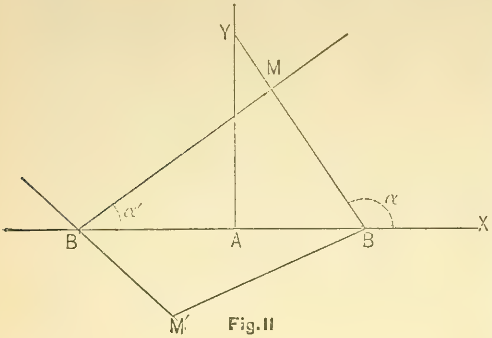


Fig. 11

If the point M is above the axis of x, the angle under which BB will be seen is $(\alpha-\alpha')$, and we have

$$t=\frac{m-m'}{1+mm'} \quad . \quad . \quad . \quad (1)$$

But if the point is at M', below AX, the angle under which BB' will be seen will be $(\alpha'-\alpha)$, whose tangent is,

$$\frac{m'-m}{1+mm'}=-t.$$

The points of the plane *satisfying* (1) will be such then, that from those above AX we shall see BB' under the angle whose tangent is t, whereas from those below AX, BB' will be seen under the angle whose tangent is $-t$, that is under the supplementary angle. We make this explanation now, as we necessarily include this result in making use of the single equation (1), and cannot get rid of it. Finding the values of m and m' from the equations of the straight lines and substituting in equation (1) and reducing, we find,

$$y^2+x^2-a^2=\frac{2ay}{t};$$

which can be put under the following form,

$$\left(y-\frac{a}{t}\right)^2+x^2=a^2\left(1+\frac{1}{t^2}\right) \quad .. \quad (2)$$

which we recognize at once as the equation of a circle, whose centre has $x=o$, $y=\frac{a}{t}$, and whose radius is $=\frac{a\sqrt{(1+t^2)}}{t}.$

Its upper and lower parts are segments capable of supplementary angles, as we have foreseen.

The locus of points below AX, from which BB' would be viewed under the given angle whose tangent is t, would correspond to eq. (1) if t were replaced in it by $-t$, and both the resulting equation and (2) above would correspond to circles, of which only a part belongs to the question proposed.

We have here an example of lost and strange solutions at the same time, for eq. (1) does not include the locus of *all* the points above and below AX, from which BB' can be viewed under the same angle, so that by Article 17 there will be *lost* solutions; but we recognized at the start, also, that eq. 1 satisfies two different geometrical conditions for points above and below AX, so that all the solutions of eq. 1 are not solutions of the original problem or the conditions are not reciprocal, whence by the reasoning of Article 19 the *strange* solutions are accounted for.

We see here again how erroneous it would have been to have assumed that an equation deduced by considering one point only, M above the axis of x, was general and applicable to all points of the required locus.

An attentive consideration of the preceding examples, in connection with the exposition of general methods, previously given, shows that great care must be exercised, in endeavoring to find a single equation to represent a given geometric locus, in order to avoid lost or strange solutions, which will surely be found if the conditions of the two problems are not reciprocal.

51. The converse problem, that of finding the geometric locus corresponding to a given equation, is often simpler, for here there are no lost or strange solutions, but only those that can regularly be deduced from the equation; but even in this case we must consider both plus and minus values of the quantities that admit of them entering the equation; for when an equation is given we tacitly assume it to be general, unless it is expressly stated that it pertains only to certain values or quadrants or other limits. But this very generality, without which analytical geometry could not exist as a complete science, demands a corresponding circumspection in all the discussions pertaining to it.

This observation naturally suggests, certain remarks relative to formulæ expressed in terms of polar co-ordinates,

with which we shall conclude this discussion.

52. If we consider a point in a plane referred to rectangular co-ordinates x, y, the formulæ for passing to polar co-ordinates are

$$x = r\cos\varphi,\ y = r\sin\varphi \quad . \quad . \quad . \quad (1).$$

where r = radius vector from the origin of rectangular co-ordinates and φ, the angle made by it with the axis of x. Now, since *cos* φ is positive or negative with x, and *sin* φ positive or negative at the same time as y, it is evident that the above formulæ are general, and hold for any values of the angle φ, provided we take the signs of the functions established in trigonometry, and regard x and y as positive in one sense and negative in the other, r being regarded always as positive and in absolute value. But a little inspection of the formulas will show that if we put $(\varphi \pm \pi)$ for φ, and $-r$ for r, that the formulas will pertain to the same point, if we agree that r must be laid off in an opposite direction from the origin to the radius vector, since r and the trigonometric functions both change signs at the same time, so that the products giving the values of x and y will be the same as before. (Puckle, in his *conic sections*, page 8, has assumed this principle, without deriving it from any formulas, and given illustrations of its application.)

Now let the equation of a locus expressed in rectangular co-ordinates be

$$F(x, y) = 0\,;$$

its polar equation will be

$$F(r\cos\varphi,\ y\sin\varphi) = 0 \quad . \quad . \quad (2)$$

and we have seen that, taking positive values of r corresponding to any angle φ, eq. (2) will give all the points of the locus represented by the eq.

$$F(x, y) = 0\,;$$

which we should likewise find by substituting $(\varphi \pm \pi)$ for φ at the same time that we put $-r$ for r, since the same point of the locus is given by the co-ordinates $(\varphi \pm \pi)$, and $-r$ as by φ and $+r$, with the distinct understanding that all angles are to be laid off, going around in the same direction from the axis of x, and that $-r$ is to be laid off on the radius vector produced and of the same absolute value as $+r$. Thus we can either regard r as always positive, and vary φ from 0 to 2π to get all the points, or admitting negative values of r it will suffice to vary φ from 0 to π.

53. It is important to observe that if the original equation in rectangular co-ordinates $F(x, y) = 0$, was subjected to any restriction, that Eq. (2) will likewise be subjected to the same restriction.

To illustrate, suppose $F(x,y)$ contains a radical of the form $\sqrt{mx^2 + ny^2}$ that must always be regarded as positive. Transforming this into polar co-ordinates by means of eqs. (1), the radical becomes,

$$+\sqrt{m r^2 \cos^2\varphi + n r^2 \sin^2\varphi},$$

and will remain the same when we put $\varphi + \pi$ and $-r$ in place of φ and r.

Now, if we had passed the factor r^2 outside of the radical and written it

$$r\sqrt{m\cos^2\varphi + x\sin^2\varphi},$$

we must always take r positively, for changing φ and r to $\varphi + \pi$ and $-r$, would give a minus radical. The double construction then only applies in this case when the radical can be taken with the double sign.

54. Again, when the transformed equation (2) can be decomposed into factors, the double construction will not apply necessarily to each factor separately, for it has not been so demonstrated.

Thus the polar equation of the ellipse, with the focus for a pole, is

$$(a^2 - c^2 \cos^2\varphi)\, r^2 + 2b^2 c r \cos\varphi - b^4 = 0,$$

and we see that if r and φ satisfy the equation, $-r$ and $\varphi + \pi$ will likewise satisfy it; but if we decompose the first member into two factors, and put them equal to zero, we find the two equations,

$$r = \frac{b^2}{a + c\cos\varphi},\quad r = \frac{b^2}{-a + c\cos\varphi},$$

neither of which is satisfied when we replace r and ϕ by $-r$ and $\varphi + \pi$, although one is converted into the other.

In constructing the locus from either one of these equations, we notice that the first always gives positive values of r, whereas the second will always give negative values of r, since $a > c$; therefore if we use the last equation we must be careful to lay off the values of r in an opposite sense to those for $+r$. The

values of the last will agree with those pertaining to the first equation, when φ in the first corresponds to $\varphi+\pi$ in the second, as we have seen.

A similar investigation can be made for the hyperbola, with corresponding results.

55. Where the angle φ, and not its trigonometric function, enters an equation, the previous reasoning does not apply. Let us take a simple example, the *spiral of Archimedes*, and ascertain if any generalization can be effected by the use of minus radii vectors, and whether a double construction is possible.

Suppose a point moving uniformly along a straight line which turns uniformly around one of its points in a given plane.

Let us take for a pole the fixed point A, and for an axis AU; call $AB=a$, $AM=r$, and $MAB=\varphi$; then if b designates the amount the point moves along the straight line, whilst it turns an angle equal to unity, taking for the measure of the angles at the center the ratio of the arcs

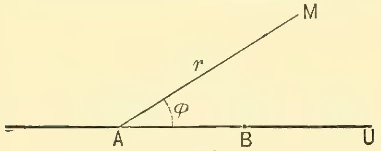


Fig. 12

intercepted to the radius, we have,

$$r=a+b\varphi \quad . \quad . \quad (1)$$

for the equation of the locus, if $a=AB$ gives the value of r on AU when $\varphi=0$.

This equation holds for all values of φ from 0 to ∞. But can it represent the positions of the moving point anterior to its arrival at B? We can answer this question in the affirmative, if we agree that angles counted in the opposite direction to φ shall be minus, for then the equation above takes the form $r=a-b\varphi'$, if φ' is the absolute value of the angle reckoned to the right.

If this turning in the negative direction is continued, so that $b\varphi'$ becomes greater than a, the same equation suffices to represent the position of points in the indefinite past, before A was reached, provided we agree to lay off the minus values of r in an opposite direction to the plus values. The equation now takes the form $-r=b\varphi'-a$.

With the conventions established we see that negative values of r and φ serve to generalize formulas in equations where the angle, and not its function, enters; so that one formula, (1) in this instance, suffices to represent all the points of the locus.

But the double construction, previously mentioned, for the same point, does not hold in this case. Thus equation (1) is not satisfied when we change the coordinates φ and r to $\varphi+\pi$ and $-r$.

It is seen from the preceding investigation that a negative radius vector is not always admissible, but that when it is, there can be no possible objection to its use, especially if an investigation is thereby simplified in any way.

In fact the use of both plus and minus quantities, in the sense of opposition throughout the whole range of mathematics, allows a condensation in the expression of results, without which the science would be in a very imperfect state; but this very contrivance imposes a corresponding attention to every phase of a question, in order to establish the generality, when it exists, or the proper limitations when it does not exist.

It is hoped that the preceding exposition and illustrations of the methods to be used will prove of assistance in such researches.

In 1862 Count de Lauture tried to simplify the Chinese language so that it could be used for telegraphing, but he was unable to solve all the difficulties of the problem. In 1866 M. Viguier experimented with autographic telegraphs, and proposed to employ a cipher code, by which the 44,000 Chinese characters could be transmitted by the Morse apparatus. This code was first published in 1870, when a cable was laid between Shanghai and Hong Kong. Every Chinese character is composed of two parts—one is called the radical or key, the other is phonetic. Every Chinese character can be classed under one of 214 radicals. In his first essay Viguier used three numbers to represent each character—that of the radical, that of the column under the radical, and that of its order in the column. This system required the use of numbers varying from three to six ciphers, which rendered the transmission of despatches slow and difficult. By eliminating the characters which are rarely used, and employing those which occur in ordinary correspondence, he was able to simplify his system, so as never to require more than four figures for a single character. To this improved method he added Chase's system of holocryptic ciphers, so that messages can now be sent readily and with perfect secrecy.

RECENT PROGRESS IN DYNAMO-ELECTRIC MACHINES.

By Professor SILVANUS P. THOMPSON, B.A., D.Sc., M.S.T.E., University College, Bristol.

From the "Journal of the Society of Arts."

I.

Fifteen months ago, I had the honor of delivering in this place a course of three Cantor Lectures on "Dynamo-electric Machinery." In the first of those lectures, the endeavor was made to trace out a physical theory of the action of dynamos, and to follow the theory into its bearings upon the construction of such machines. In the second lecture a large number of actual machines were considered and compared with one another and with the theory; and in the third lecture, the dynamo was considered in its functions as a mechanical motor.

As the present paper may be considered supplementary to the Cantor Lectures, it will be convenient to treat of the features of progress which come to-night under review in a similar order of topics. I, therefore, take up first the theory of the dynamo.

There are, in fact, three distinct theories of the dynamo: (1) a physical theory, dealing with the lines of magnetic force and lines of current in which these quantities are made, without further inquiry into their why or how, the basis of the arguments; (2) an algebraical theory, founded upon the mathematical laws of electric induction and of theoretical mechanics; and (3) a graphical theory, based upon the possibility of representing the action of a dynamo by a so-called "characteristic" curve, in the manner originally devised by Dr. Hopkinson, and subsequently developed by Frölich, Deprez, and others. The last of these three methods, though it has not received any great or striking development during the past year, has proved itself to be the most invaluable aid in the practical construction of dynamo-machines. One has only to refer to the use made of characteristics by Mr. Kapp in his articles on the winding of "compound" dynamos, and, still more recently, by Dr. Hopkinson, in solving certain problems in the electric transmission of energy, to see how invaluable the method is.

In the algebraic theory much progress has been made during the past year; and there certainly was room for it. Monsieur Joubert has published, in the *Journal de Physique* for July, 1883, a long mathematical article, the object of which is to deduce the equations of the dynamo, taking into account not only the action of self-induction in the circuit, but also some of the terms of the second order usually neglected in first approximations. It is a question whether he has not still omitted some terms of quite as great an importance as those retained in the complicated formula deduced by him. But the matter can hardly be discussed in the present paper. Still more recently, Professor Clausius has published in Wiedemann's *Annalen*, for November last, a paper expounding a mathematical theory of dynamo-electric machines far more comprehensive, and, I venture to say, far more true, than any other yet put forward. Without shirking any of the mathematical difficulties presented by the complications of mutual induction between, and self-induction in, the various organs of the machine, and by the admitted incompleteness of all our formula for connecting the magnetism of an electro-magnet with the strength of its exciting current, Prof. Clausius has succeeded in putting the equations into a shape, not only far more satisfactorily from the point of view of completeness, but in framing those equations in a manner that must commend itself to every engineer. The relative simplicity attained by Clausius is, in fact, due to his lavish introduction of a set of arbitrary constants, each one of which having values that must be determined by experiment, for each machine or type of machines. The number of new symbols thus introduced is considerable; and it would be very desirable to find names for the separate constants to be determined. An excellent translation has appeared in the *Philosophical Magazine* of this year, and another is in process of publica-

tion in *The Electrician*, in which journal the series of articles on the theory of dynamo-electric machines from the pen of Professor O. J. Lodge is still continued. It cannot be said even yet that the mathematical theory of the dynamo is near completion. A further paper by Professor Clausius is promised; and it remains to be seen whether this article will deal with some of those points in which the graphic method has been so useful in practice, for example, in determining the proper quantities of wire for the coils in "self-regulating" or "compound" machines, and in finding the best shape to give to magnets and pole-pieces.

Turning to the physical theory of the dynamo, there is much to record. Our knowledge of the inductive actions which go on between the field-magnets and armatures of dynamos, has received considerable additions during the past year from the researches of Isenbeck, Cunnynghame, Pfaundler and others. There is a good deal to be said on this head, and I have several new results to announce as the result of my own observations. Let me take as my starting point a matter mentioned in my Cantor Lectures, namely, the distribution of potential round the collector or commutator of a dynamo. Mr. W. M. Mordey, who first drew my attention to the fact that this distribution was irregular in badly-designed machines, had devised the following method of observing it. One terminal of a voltmeter was connected to one of the brushes of the dynamo, and the other terminal was joined by a wire to a small metallic brush or spring, which could be pressed against the rotating collector at any desired part of its circumference. I then made the suggestion that these indications might with advantage be plotted out round a circle corresponding to the circumference of the collector. Figs. 1 and 2, which are reproduced from my Cantor Lectures, serve to show how the potential in a good gramme machine rises gradually from its lowest to its highest value. The same values as are plotted round the circle in Fig. 1 are plotted out as vertical ordinates upon the level line in Fig. 2. I made the remark at the time that if the magnetic field in which the armature rotated were uniform, this curve would be a true "sinusoid," or curve of sines; and that the steepness of the slope of the curve at different points would enable us to judge of the relative idleness or activity of coils in different parts of the field. About the same time, I developed this method of observation a little further, and used two small metal brushes, at a distance apart equal to the width between the middle of two consecutive bars of the collector of my little Siemens dynamo, for the same purpose.* As the collector rotated, these two little brushes (see Fig. 3) gave on the voltmeter an indication which measured exactly the activity of the induction, in that section of the armature which was passing through the particular position in the field corresponding to the position of the contacts. I found, in the case of my Siemens dynamo, that the result was fairly satisfactory, for the difference of potential indicated was almost *nil* at the sections close to the proper brushes of the machine, and was a maximum about halfway between. In fact, the differences of potentials was rising most markedly at 90° from the usual

Fig. 1

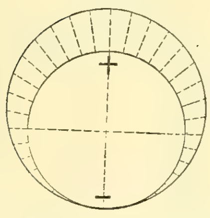
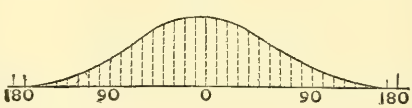


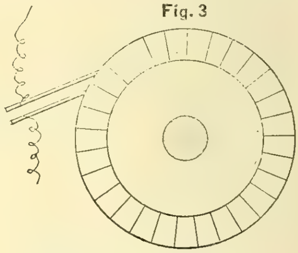
Diagram of Potential round the Collector of Gramme Dynamo.

Fig. 2



Horizontal Diagram of Potentials at Collector of Gramme Dynamo.

Fig. 3



Method of Experimenting at Collector of Dynamo.

* Dr. Isenbeck has also independently used a similar arrangement to investigate the induction going on in a Gramme dynamo.

brushes, or precisely in the region where (as seen in Fig. 2) the slope of the curve of total potential was greatest. One immediate result of Mr. Mordey's observations on the distribution of potential, and of my method of mapping it, may be recorded. I pointed out to Mr. Mordey that in a dynamo where the distribution was faulty, and where the curves of total potential showed irregularities, the fault was due to irregularities in the induction at different parts of the field; and that the remedy must be sought in changing the distribution of the lines of force in the field by altering the shape of the pole pieces. I am able now, after the lapse of fifteen months, to congratulate Mr. Mordey on the entire and complete success with which he has followed out these suggestions. He has entirely cured the Schuckert machine of its vice of sparkling. The typical bad diagram given in my Cantor Lectures was taken from a Schuckert machine before it received from his hands the modifications which are so signally successful to-day.

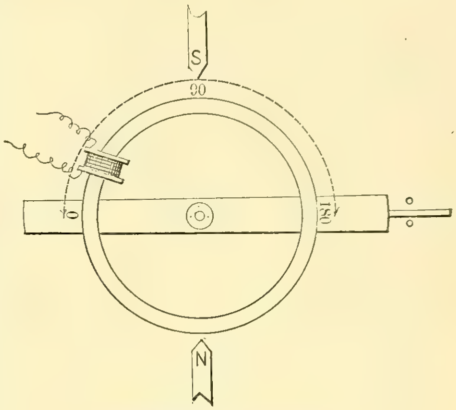
Since the experiments above detailed, I have experimented on my Siemens dynamo in another way. The machine was dismounted, and its field magnets separately excited. Two consecutive bars of the collector were then connected with a reflecting galvanometer having a moderately heavy and slow moving needle. A small lever clamped to the collector allowed the armature to be rotated by hand, through successive angles equal to 10°, there being thirty-six bars to the collector. The deflexions obtained of course measured the intensity of the inductive effect at each position. The result confirmed those obtained by the method of the two wire brushes.

I mention these methods, which have been used in my laboratory at Bristol, and have not been published before, because they relate strictly to the physical theory of the dynamo as developed in my Cantor Lectures, and also because of their practical application to all dynamos in which any such defect appears. They are also very closely related to the researches of Dr. Isenbeck, which next claim attention.

Dr. Isenbeck described, in the *Electrotechnische Zeitschrist* for last August, a beautiful little apparatus for investigating the induction in the coils of a Gramme ring, and for investigating the influence exerted by pole pieces of different form upon these actions.

Isenbeck's apparatus (Fig. 4) consists of a circular frame of wood placed between the poles of two small bar-magnets of steel, each 25 centimeters long, lying

Fig. 4



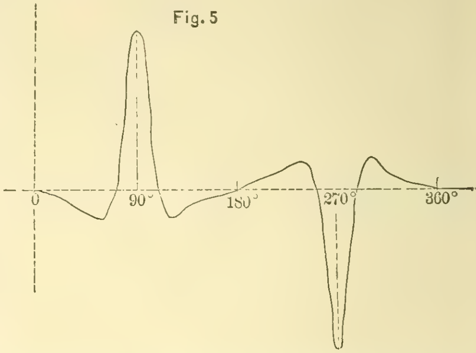
ISENBECK'S APPARATUS.

25 centimeters apart. On the frame' which is pivoted at the center, is carried a ring of wood or iron, upon which is placed at one point a small coil of fine wire. This corresponds to a single section of the coils of a Pacinotti or of a Gramme ring, of which the ring of wood or iron constitutes the core. The coil can be adjusted to any desired position on the ring, and the ends communicate with a galvanometer. On vibrating it isochronously with the swing of the needle of the galvanometer, the latter is set in motion by the induced currents, and the deflexion which results shows the relative amount of induction going on in the particular part of the field where the coil is situated. The vibrations of the frame are limited by stocks to an angle of 7° 5′. Pole pieces of soft iron, bent into arcs of about 160° so as to embrace the ring on both sides, but not quite meeting, were constructed to fit upon the poles of the magnets. In some of the experiments a disc of iron was placed internally within the ring; and in some other experiments a magnet was placed inside the ring, with its poles set, so as either to reinforce, or to oppose, the action of the two external poles. In Dr. Isenbeck's hands this apparatus

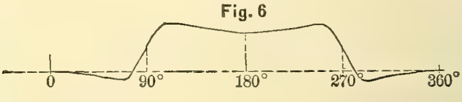
yielded some remarkable results. Using a wooden ring, and poles destitute of polar expansions, he observed a very remarkable inversion in the inductive action to take place at about 25° from the position nearest the poles.

Fig. 4 is a sketch of the main parts of Isenbeck's instrument, and shows the small coil mounted on the wooden ring, and capable of being vibrated to and fro between stops. When vibrated at 0°, or in a position on the diametral line at right angles to the polar diameter, there is no induction in the coil; but as the coil is moved into successive positions round the ring towards the poles, and vibrated there, the induction is observed first to increase, then die away, then begin again in a very powerful way, as it nears the pole, where the rate of cutting the lines of force is a maximum. This powerful induction near the poles is, however, confined to the narrow region within about 12° on each side of the pole. It is beyond these points that the false inductions occur, giving rise in the coil, as it passes through the regions beyond the 12°, to electromotive forces opposing those which are generated in the regions which are close to the poles.

These inverse inductions were found by Isenbeck to be even worse when an iron disc or an internal opposing magnet was placed within the ring; but a reinforcing magnet slightly improved matters. Of course such an action in a Gramme armature going on in all the coils, except in those within 12° of the central line of the poles, would be most disastrous to the working of the machine; and the rise of potential around the collector would be anything but regular. In Fig. 5 I have copied out Isenbeck's curve of induction for the consecutive four quadrants. From 0° to 90° the exploring coil is supposed to be vibrated in successive positions from the place where, in the actual dynamo, the negative brush would be, around to a point opposite the S pole of the pointed field-magnet. From 90° to 180° it is passing around to the positive brush; from 180° to 270° it passes to a point opposite the N pole; and from 270° to 360° returns to the negative brush. Now, since the height of this curve, at any point, measures the induction going on in a typical section as it moves through the corresponding region of the field, and since in the actual Pacinotti or Gramme ring the sections are connected all the way around the ring, it follows



that the actual potential at any point in the series of sections will be got by adding up the total induced electromotive force up to that point. In other words we must integrate the curve to obtain the corresponding curve of potential corresponding with the actual state of things around the collector of the machine. Fig. 6 gives the curve as integrated expressly for me from Fig. 5 by the aid of the very ingenious curve integrator of Mr. C. Vernon Boys. The height of the ordinate of this second curve at any point is proportional to the total area enclosed under the first curve up to the corresponding point. Thus the height at 90° in the second curve is proportional to the total area up to 90° below the first curve. And it will be noticed that though the induction (first curve, Fig. 5) decreases after 90°, and falls to zero at about 102°, the sum of the potentials (second curve, Fig. 6) goes on increasing

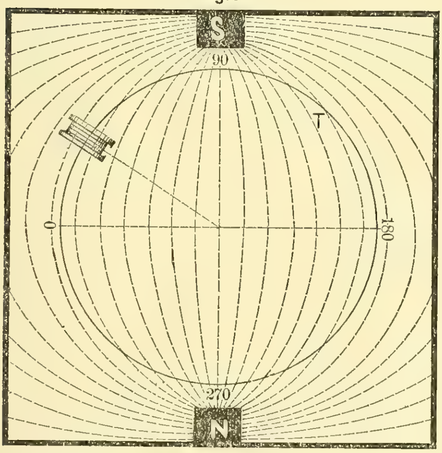


up to 102°, where it is a maximum, and after that falls off, because, as the first curve shows, there is from that point onwards till 180° an opposing false induction. If this potential curve were actually observed on any dynamo, we might be sure that we could get a higher electromotive force by moving the brush from 108° to 102°, or to 258°, where the potential is higher. Any dynamo in which the curve

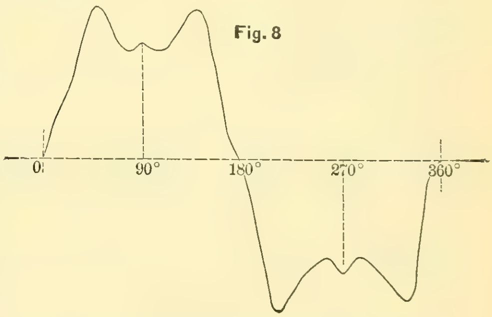
of potentials at the commutator presented such irregularities as Fig. 6, would be a very inefficient machine, and would probably spark terribly at the collector. It is evident that the induction in some of the coils is opposing that in some of the adjacent coils.

Two questions naturally arise: Why should such detrimental inductions arise in the ring? and how can they be obviated? The researches of Dr. Isenbeck supply the answer to both points. Dr. Isenbeck has calculated from the laws of magnetic potential the number of lines of force that will be cut at the various points of the path of the ring. He finds that the complicated mathematical expression for this case, when examined, shows negative values for angles between 12° and 90°. The curves of values that satisfy his equations have minima exactly in those regions where his experiments revealed them. This is very satisfactory as far as it goes. But we may deduce a precisely similar conclusion in a much simpler manner, from considering the form and distribution of the lines of magnetic force in the field. These are shewn in Fig. 7, together with the ex-

Fig. 7

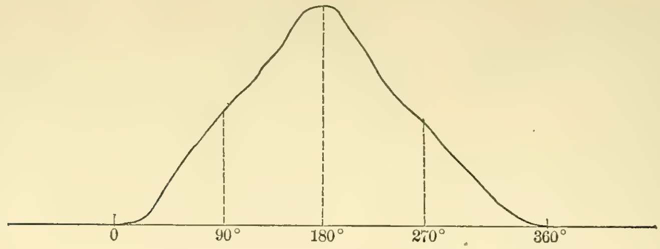


ploring coil situated as in Fig. 4. A simple inspection of the figure will show that at 0° a certain number of lines of force would thread themselves through the exploring coil. As the coil moved around toward the S pole, the number would increase at first, then become for an instant stationary, with neither increase nor decrease; after that a very rapid decrease would set in, which, as the coil passed the 90° point, would result in there being no lines of force through the coil. But at the very same instant the lines of force would begin to crowd in on the other side of the coil, and the number so threaded through negatively would increase until the coil turned around to about the position marked T, where the lines of force are nearly tangential to its path and here the inversion would occur, because, from that point onwards to 180°, the number of lines of force threaded through the coil would decrease. We see, then, that such inversions in the induction must occur of necessity to a small coil rotating in a magnetic field in which the lines of force are distributed in the curved directions, and with the unequal density which this disposition of the field magnets presents. The remedy is obvious; arrange a more uniform field in which the lines of force are more equally distributed, and are straighter.



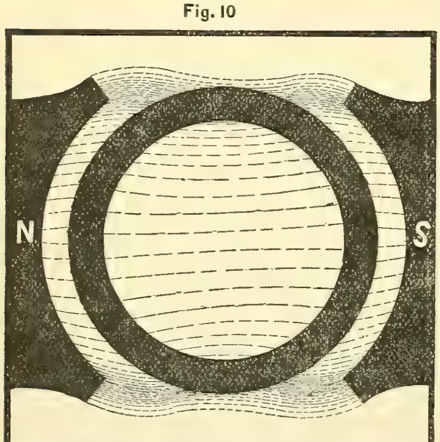
If an iron core be substituted for the wooden core the useful induction is greater and the false induction less; there is still an inversion, but it takes place at about 25° from the pole, and is quite trifling in amount. The introduction of iron pole pieces extending in two nearly semi-circular arcs from the magnets on either side has, if the wooden ring be still kept as a core, the effect of completely changing the induction, so that the curve, instead of showing a maximum at 90° from starting, shows one at about 10°, and another at 170°. If, however, we make the double improvement of using the iron pole pieces and the iron core at the same time, the effect is at once changed. There are no longer any inversions, though the induction shows some peculiarity still. Fig. 8

Fig. 9



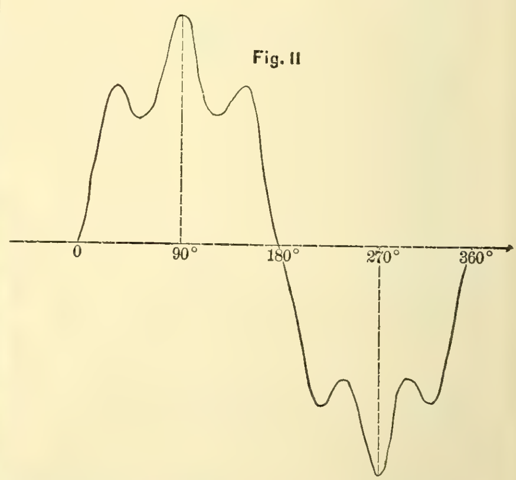
shows the curve of induction adapted from Dr. Isenbeck's paper, and Fig. 9 the curve of potential, which I have had integrated from it. Looking at Fig. 8 we see that on starting from 0° induction soon mounts up, and becomes a maximum at about 20°, where the coil is getting well opposite the end of the encircling pole piece. From this point on, though the induction is somewhat less, it still has a high value, showing a slight momentary increase as the coil passes the pole at 90°, and there is another maximum at about 160°, as the coil passes the other end of the pole piece. My integrated curve (Fig. 9) tells us what would go on at the collector if this were the action in the connected set of coils of a Pacinotti or Gramme ring. The potential rises from 0° all the way to close upon 180°. Still this is not perfect. In the perfect case the potential

Fig. 10



curve would rise in a perfect harmonic wave form, like that shown in Fig. 2. Fig. 9 departs widely from this, for it is convex from 0° to 90°, and concave between 90° to 180°. But there are no inversions. The cause of the improvement is easily told: The field—such as there is between the pole piece and the core—is "straighter," and the density of the lines of force in it more uniform. I proved this experimentally in 1878, by the simple process of examining the lines of force in such a field by means of iron filings; the actual filings, secured in their places upon a sheet of gummed glass, were sent to the late Mons. Alfred Niaudet, who had requested me to examine the matter for him. Fig. 10 shows the actual field between the encircling pole pieces and the iron ring. It will be seen that, though nearly straight in the narrow intervening region, they are not equally distributed, being slightly denser opposite the ends of the pole pieces. One other case examined by Dr. Isenbeck, we

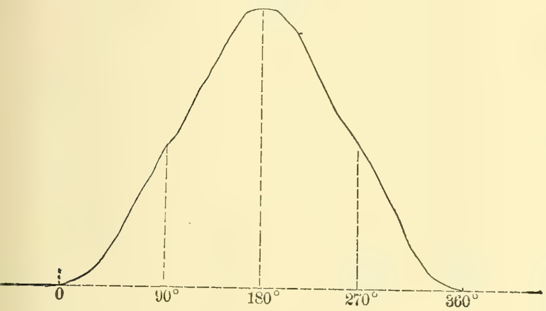
Fig. 11



will glance at.¶ The effect of introducing within the ring an interior magnet, having its S pole opposite the external S pole, and its N pole opposite the external N pole, was found to assist the action. The induction curve is represented in Fig. 11. As will be seen, there are two maxima at points a little beyond the ends

of the pole pieces, as before; but in between them there is still a higher maximum, right between the poles. This case also has been integrated on Mr. Boys' machine, and shows the potential curve of Fig. 12. This curve is a still nearer approach to the harmonic wave form, being concave from 0° to 90°, and convex from 90° to 180°.

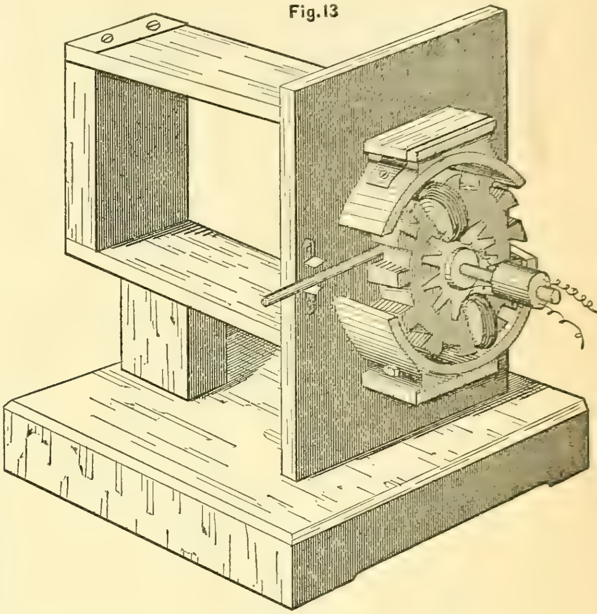
Fig. 12



I pass from Dr. Isenbeck's researches, and the integrated curves of potential which I have deduced from them, to some further researches of my own, which were undertaken with the view of throwing some light on the question whether the Pacinotti form of armature, with protruding iron teeth, or the Gramme form, in which the iron core is entirely overwound with wire, is the better. It has been assumed without, so far as I am aware, any reason assigned, that the Gramme ring was an improvement on that of Pacinotti. Pacinotti's was of solid iron, with teeth which projected both outwards and inwards, having the coils wound between. Gramme's was made "either out of one piece of iron, or of a bundle of iron wires," and had the coils wound "around the entire surface." Now the question whether the Gramme construction is better than the Pacinotti or not, can readily be tested by experiment. And experiment alone can determine whether it is better to keep a thickness of wire always between pole pieces and the core, or to intensify the field by giving to the lines of force the powerful reinforcement of protruding teeth of iron. The apparatus I have constructed for determining this point is now before you. It is sketched in Fig. 13.

First there are a couple of magnets set in a frame so as to give us a magnetic field, and there are pole pieces that can be removed at will; in fact, there are three sets of pole pieces for experimenting with different forms. Between the

Fig. 13



poles is set an axis of brass, upon which the armatures can be slid. These armatures are three in number. One is shown in Figs. 14 and 15, and consists of two coils of fine wire wound upon a wooden ring; another armature is exactly like

Fig. 14

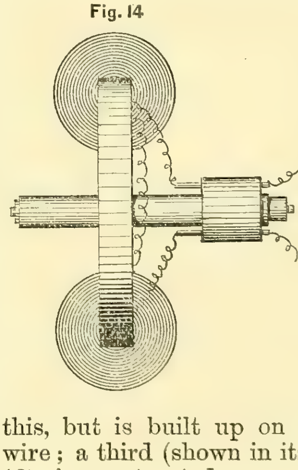
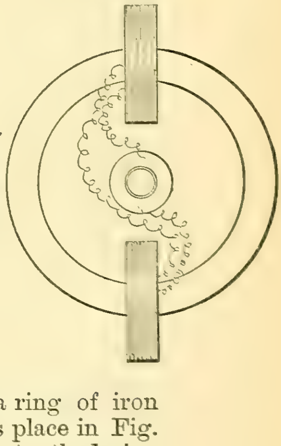


Fig. 15



this, but is built up on a ring of iron wire; a third (shown in its place in Fig. 13) is constructed upon a toothed ring made up of a number of plates of ferrotype iron, cut out and placed flat upon one

another. On each of the armatures are wound two coils at opposite ends of a diameter. The coils contain precisely equal lengths of silk-covered copper wire, cut from one piece. The cross section of the core within each of these coils is in each case a square, of one centimeter in the side, so that the number of turns in each coil is as nearly equal as possible. I can slip any one of these armatures into the field, and connect it with a galvanometer. There is a lever handle screwed to the armature, by means of which it can be moved. I have used two methods of proceeding in order to compare the coils. One of these methods is to turn the armature suddenly through a quarter of a revolution, so that the coils advance from 0° to 90°, when the "throw" of the needle of the galvanometer—which is a slow beat one—gives me a measure of the total amount of induction in the armature. The results are as follows:

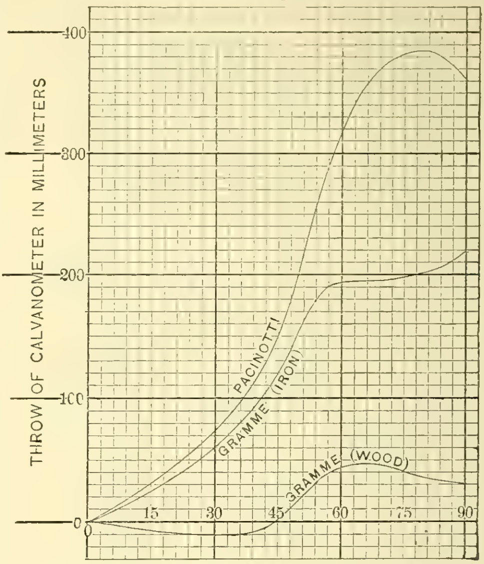
Gramme. Wooden Ring.	Gramme. Iron Ring.	Pacinotti. Iron Toothed. Ring.
5	24	50

My second method of using these armatures consists in jerking the coils through a distance equal to their own thickness, the coils being successively placed at different positions in the field, the throw of the galvanometer being observed as before. Each of the coils occupies as nearly as possible 15° of angular breadth. Accordingly I have two stops set, limiting the motion of the handle to that amount, and at the back there is a graduated circle enabling me to set the armature with the coils in any desired position. If we move the coils by six such jerks, through their own angular breadth each time, then starting at 0°, the sixth jerk will bring us to 90°. I have plotted out in Fig. 16 the three curves thus obtained, and the corresponding numbers are given in the following table:

	Gramme.	Gramme.	Pacinotti.
	Wooden Ring.	Iron Ring.	Iron Tooth'd Ring.
0°–15°	5	25	30
15°–30°	10	60	70
30°–45°	0	120	140
45°–60°	45	195	320
60°–75°	40	200	380
75°–90°	30	220	360

These figures leave no doubt as to the question at issue. The Gramme pattern of ring armature, so far from being an improvement on the Pacinotti, is distinctly a retrograde step; always supposing that the cost of construction, liability to heating, and other kindred matters be equal for the two. The significance of this point will be resumed at a later period in this paper.

Fig. 16



Before leaving the theory of armatures to pass to that of field magnets, I should wish to say that the experiments which I have made, and also those of Dr. Isenbeck, have been so instructive to myself that I have already begun a similar series of observations on other forms of armature. I hope in due time to make known the results of my investigations.

But little advance has been made in theory so far as relates to field magnets. The law of saturation of an electro-magnet remains still an empirical law. It is satisfactory, however, that such widely differing authorities as Prof. Clausius, M. Marcel Deprez, and Professors Ayrton and Perry, agree in accepting the empirical formula of Frölich as a sufficiently accurate expression for the law of saturation.

Some progress has been made in the theory of the lead that must be given to the brushes of the dynamo. Formerly this was ascribed to a sluggishness in

the demagnetization of the iron of the armature; but in 1878 the late M. Antoine Breguet suggested as a reason the influence of the actual current circulating around the armature coils, which would tend to produce in the iron of the armature a magnetization at right angles to that due to the field magnets.

Breguet showed that there would be a resultant oblique direction of the lines of magnetization in the field, and therefore, since the "diameter of commutation" is at right angles to this direction, the brushes also must be displaced through an equal angle. Clausius accepts this view in his recent theory, and adopts for the angle of the resultant field that whose tangent is the ratio of the two magnetizing forces due to the field magnets and the current in the armature respectively. Professors Ayrton and Perry have also pointed out that there will be an additional displacement of the resultant poles of the armature, consequent upon the self-induction going on in the armature coil between its different sections. In their paper on the government of motors, in which they have brought out this point, they, however, take the view that part of the displacement of the pole is due to the sluggishness of demagnetization of the iron. I do not think, however, that this can be maintained. No experimental proof has ever been given that there is any such thing as a true magnetic lag; the apparent magnetic sluggishness of thick masses of iron is demonstrably due to internal induced currents; and no one uses solid iron in armature cores for this very reason. Neither has it been shown that thin iron plates or wires, such as are used in armature cores, are slower in demagnetizing than magnetizing. Indeed, the reverse is probably true; and, until further experimental evidence is forthcoming, I shall assume that there is no magnetic lag in properly laminated iron cores.

It may here be pointed out that, assuming as a first approximation that the rule that the tangent of the angle of lead represents the ratio between the magnetizing power of the field magnets and of the armature coils, the lead may be diminished to a very small quantity, by increasing the relative power of the field magnets, a course which is for many other reasons advisable. All practice confirms the rule that the magnetic moment of the field magnets ought to be very great as compared with that of the armature. Further than this there ought to be so much iron in the armature as to be just saturated when the dynamo is working at its greatest activity. If there is less than this it will become saturated at a certain point, and when any currents greater than this are employed, the lead will alter, for then the magnetic effect due to the current in the armature will be of greater importance relatively to that due to the field magnets. For the same reason the lead will be more constant when the field magnets are under their saturation point than when quite saturated. In short, every cause that tends to reduce the lead makes the lead more constant, and therefore tends to reduce sparking at the brushes. And the best means to secure this is obviously to use an unstinted quantity of iron—and that of the softest kind—both in the field magnets and in the armature, for then the currents circulating in the armature will have less chance of perturbing the field.

In relation to the magnetism of field magnets, it may be pointed out that the "characteristic" curves now so much used for the study of the action of dynamo machines, which show the rise of the electromotive force of the machine in relation to the corresponding strength of the current, are sometimes assumed, though not quite rightly, to represent the rise of magnetization of the field magnets. Now, though the magnetization of the magnet may attain to practical saturation, it does not, under a still more powerful current, show a magnetization less than saturation. But the characteristics of nearly all series-wound dynamos show—at least for high speeds—a decided tendency to turn down after attaining a maximum; and for some machines, for example, the Brush, this diminution of the electromotive force is very marked. The electromotive force diminishes, but the magnetism of the field magnets does not. An explanation of this dip in the characteristic has lately been put forward by Dr. Hopkinson in his lecture on "Electric Lighting," before the Institution of Civil Engineers, attributing this to the reaction of self-induction and mutual induction between the sec-

tions of the armature. No doubt this cause contributes to the effect, as all such reactions diminish the effective electromotive force. I am inclined, however, to think that the greater part of the effect is due to the shifting of the effective line of the field in consequence of the iron of the field magnets becoming saturated before the armature is so. It is at least significant that in the Brush machine, where the reduction of the electromotive force is very great, there is also such a mass of iron in the armature, and so variable a lead at the brushes.

THE COMPOUND STEAM ENGINE, CONSIDERED IN CONNECTION WITH ITS STEAM-GENERATING PLANT.

By RICHARD H. BUEL, C. E.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

THE compound, or "double-cylinder" steam engine, as it is often called in distinction to the non-compound or "single-cylinder" engine, has practically supplanted the latter for marine purposes, is largely used in factories, and has recently been applied to locomotives.

Simultaneously with the introduction of double-cylinder engines, the steam pressure carried in marine boilers was increased, and in general, the ratio of expansion also; so that there were many engineers who were inclined to attribute the economy of the new system, which was indubitable, to these latter changes rather than to the compounding. Indeed, there are many engineers to-day, who hold this opinion,—and although the disputes in relation to the matter, which have for several years occupied so much space in technical journals, are less animated than they formerly were, they have not wholly ceased. The greater number of these discussions have been theoretical rather than practical, and experiments, when they were presented, have frequently proved to be of little value, on account of differences in important conditions. Many experimental results that are valuable have, however, been published; and the writer proposes, in the present article, to give a brief summary of results that seem be of practical importance and interest, relating, as they do, to the various means that have been employed to secure economy in the production of steam power.

The economy of a steam engine, considered as a whole, depends upon the cost of producing the steam in the boilers, and the efficiency of the motor in which this steam is used. Thus, if one steam engine produces an indicated horse-power with 23 lbs. of steam per hour, and this steam is furnished by a boiler which evaporates 8 lbs. of water per lb. of coal,–while another engine requires 25 lbs. of steam per hour for each indicated horse-power developed, and obtains its steam from a boiler which evaporates 9 lbs. of water per lb. of coal, the consumption of coal per hour for each indicated horse-power developed will be, for the first engine, 2.875 lbs. and for the second, 2.778 lbs. In order to make a perfectly accurate comparison of the cost of coal or steam per horse-power, the useful power exerted, or the indicated power less the power absorbed by friction of mechanism, should be used as the basis of calculation. As a general rule, however, the useful or net horse-power is not determined, in making experiments, so that it is necessary to refer the expenditure of steam and coal to the indicated horse-power, with the assumption, which is not far from the truth, that the pressure required to overcome the friction of well designed steam engines is practically constant.

In comparing experiments made with different engines, or with the same engine under different circumstances, if the steam pressures vary in the several experiments, as is generally the case, it is more accurate to reduce the weights of steam used, to equivalent weights at the pressure of the atmosphere. This is particularly useful, when the engines are considered in connection with the boilers, since it is customary to compare the performance of different boilers, with respect to their "equivalent" evaporation, or evaporation from and at 212°, that is to say, the evaporation at atmospheric pressure of water whose temperature is 212°. In the ex-

TABLE I.

FACTORS FOR REDUCING EVAPORATION FROM DIFFERENT TEMPERATURES AND AT DIFFERENT PRESSURES, TO EVAPORATION FROM AND AT 212°.

Temperature of Feed Water.	Pressure of Steam in lbs. per Square Inch above the Atmosphere.										
	0	10	20	30	40	50	60	70	80	90	100
32°	1.187	1.195	1.201	1.206	1.210	1.214	1.217	1.220	1.222	1.224	1.226
40°	1.179	1.187	1.193	1.198	1.202	1.206	1.209	1.212	1.214	1.216	1.218
50°	1.168	1.176	1.182	1.187	1.191	1.195	1.198	1.201	1.203	1.205	1.207
60°	1.158	1.166	1.172	1.177	1.181	1.185	1.188	1.191	1.193	1.195	1.197
70°	1.148	1.156	1.162	1.167	1.171	1.175	1.178	1.181	1.183	1.185	1.187
80°	1.137	1.145	1.151	1.156	1.160	1.164	1.167	1.170	1.172	1.174	1.176
90°	1.127	1.135	1.141	1.146	1.150	1.154	1.157	1.160	1.162	1.164	1.166
100°	1.117	1.125	1.131	1.136	1.140	1.144	1.147	1.150	1.152	1.154	1.156
110°	1.106	1.114	1.120	1.125	1.129	1.133	1.136	1.139	1.141	1.143	1.145
120°	1.096	1.104	1.110	1.115	1.119	1.123	1.126	1.129	1.131	1.133	1.135
130°	1.085	1.093	1.099	1.104	1.108	1.112	1.115	1.118	1.120	1.122	1.124
140°	1.075	1.083	1.089	1.094	1.098	1.102	1.105	1.108	1.110	1.112	1.114
150°	1.065	1.073	1.079	1.084	1.088	1.092	1.095	1.098	1.100	1.102	1.104
160°	1.054	1.062	1.068	1.073	1.077	1.081	1.084	1.087	1.089	1.091	1.093
170°	1.044	1.052	1.058	1.063	1.067	1.071	1.074	1.077	1.079	1.081	1.083
180°	1.033	1.041	1.047	1.052	1.056	1.060	1.063	1.066	1.068	1.070	1.072
190°	1.023	1.031	1.037	1.042	1.046	1.050	1.053	1.056	1.058	1.060	1.062
200°	1.013	1.021	1.027	1.032	1.036	1.040	1.043	1.046	1.048	1.050	1.052
212°	1.000	1.008	1.014	1.019	1.023	1.027	1.030	1.033	1.035	1.037	1.039

amples of engine performance that follow, this reduction has been made, on the assumption that the temperature of the feed water was 32°, and using the pressure noted in each experiment. Such reductions are facilitated by the factors of Table I. For the sake of illustration suppose, in the case of experiment 1, Table IV. that the result there given is to be reduced to the equivalent evaporation of water which is actually evaporated from a temperature of 150° and at a pressure of 67.6 lbs. sq. in. above the atmosphere. By Table I. it appears that the equivalent evaporation, under these circumstaces, is

$$\frac{1.097}{1.219}\times 22.4=20.2 \text{ lbs.}$$

In discussing the cost of the production of steam, and its use in an engine, it will be convenient to treat these two topics separately.

I. THE ENGINE.

Experiment and theory both show clearly that in order to use steam economically in an engine, it must be allowed to expand in the cylinder. Precisely what ratio of expansion it is most economical to adopt for any special case may not be so well settled; but this much seems to be clearly established: that such ratios of expansion as are known to be economical cannot be used without serious inconvenience in engines which, as is the case with those of the marine type, are not provided with fly-wheels. This is because the pressure on the crank-pin of an engine in which steam is used with a high ratio of expansion, varies so much that the frame and other parts of the engine are unduly strained. The effect of a fly-wheel in preventing serious injury to an engine subjected to varying strains, and the effect of these strains when they are transmitted directly to the machine, can be most simply illustrated by experiment. The following test was made by the writer : A vertical steam engine, with plain slide-valve admitting steam for the full stroke, was used to drive a train of wire rolls in a steel mill. The speed of the engine was controlled by a centrifugal governor which acted on a throttle-valve, and on the engine shaft there was an enormous fly-wheel, 23 ft. in diameter and weighing 27.5 gross tons. The maximum power of this engine—436 horse-power—was exerted when 8 simultaneous passes were made in the rolls (6 in the wire rolls and 2 in the roughing

rolls), and this power was instantly reduced to 36 horse-power when the rolls were running idle. Under these circumstances, there was no perceptible change in the speed of revolution of the engine, and it had been used in work of this kind for a number of years, at a very slight expense for repairs. In the same mill, connected to another train of rolls, where the work was not so hard, was an automatic cut-off engine, of much greater power than the vertical engine, but with a very light fly-wheel,—and after this engine had come to a stop several times, when two simultaneous passes were made in the rolls, the fly-wheel was taken off and a heavier one was substituted, which change put an end to the trouble.

A striking example of the effect of expansion in an engine which had no fly-wheel, is contained in Chief-Engineer J. W. King's "Report on European Ships of War, etc.," Washington, 1877, as follows:

"The most expensive and notable attempt to realize the benefits of the compound system by the simple engine at sea was made two years ago by the proprietors of the Allen Line of steamers. This company made the comparative test on a large scale. Two ships were built, the one fitted with compound engines, and the other with simple expansive engines. The boilers were identically alike, made from the same drawings, having the same grate and heating surface, and the same pressure of steam was used in each vessel.

"I have not at command all the details of these ships, but I received from the designer of the machinery the following particulars:

	Name of ship, Polynesia.	Name of ship, Circassian.
Length, feet........	400	360
Breadth, "	42	40
Draught of water, ft.	25.5	23
Kind of engines....	Compound.	Non-Comp'nd.
Number of cylinders	4	2
Diameter, inches...	43 and $80\frac{3}{4}$	62
Stroke, feet........	4	4
Number of boilers..	10	10
Number of furnaces	20	20
Pres're of steam, lbs.	60	60

"The expansion valves were fitted in the simple engines to work the steam, when desired, to the same degree of expansion as in the compound engines. The workmanship and materials were equally good, and the parts equally strong in each set of engines. The two ships were put on the line between Liverpool and Quebec, Canada, and, as was anticipated, the results as to economy of fuel were not materially different, about two pounds of good Welsh coal per indicated horse-power per hour being expended in each ship. This satisfactory result, however, soon found an offset in the shape of unexpected difficulties with the simple engine, consequent upon the serious shocks resulting from the rapidly varying pressures on the crank-pins. So serious were these, that not only the crank-shaft, but also the stationary parts of the engines, began at an early day to show signs of weakness, and in a short time gave out altogether. The superintending engineer of the company was the designer of the machinery, and it was only after his skill and efforts failed to keep the ship running that he reluctantly decided to remove the engines and to substitute compound engines in their stead. The engines substituted had a pair of vertical inverted cylinders, with a diameter for the high pressure of 55 inches, and for the low pressure of 92 inches.

"The performance of the Polynesia was satisfactory from the first, the voyages never having been interrupted; and the performance of the Circassian has also been satisfactory since the substitution in her of the compound engines for the simple ones."

It frequently happens that a wasteful single-cylinder steam engine can be made much more economical by changing it to a compound engine. The principal reason for the economy that results from such a change seems to be the suppression of a large part of the cylinder condensation. The cylinder of an engine is exposed, during each stroke, to the extremes of temperature due to the entering steam and the exhaust. Now, if the steam is used in a double-cylinder engine, the extremes of temperature in the small cylinder are inconsiderable, and in consequence the condensation in this cylinder in usually slight in amount, and

the extremes of temperature in the large cylinder are also less than they would be if the small cylinder were removed.

Tables II. and III. contain the results of some experiments made by the writer with small engines, both before and after

TABLE II.

TEST OF SMALL ENGINE BEFORE AND AFTER ADDITION OF SECOND CYLINDER—CYLINDERS NOT JACKETED—CUT-OFF ALIKE IN BOTH CYLINDERS—VOLUME OF LARGE CYLINDER $=2\times$VOLUME OF SMALL CYLINDER.

Item.	1	2	3	4	5
	Single Cylinder.		Compound.		
	Cut-off, $\frac{1}{4}$.	Cut-off, $\frac{1}{6}$.	Draining water from steam before admission into 2d cylinder.		Not draining water from steam
			Cut-off, $\frac{1}{2}$.	Cut-off, $\frac{2}{3}$.	Cut-off, $\frac{1}{2}$.
Initial pressure of steam, lbs. per sq. in. above atmosphere	90	99	106	100	97
Lbs. of water evaporated per hour from and at 212°, per indicated horse-power......	47.1	51.9	29.5	33.2	34.8

TABLE III.

EXPERIMENTS ON SMALL ENGINE, SINGLE CYLINDER AND COMPOUND, LARGE CYLINDER STEAM JACKETED, SUPERHEATER FOR STEAM DISCHARGED FROM SMALL CYLINDER, AND FEED WATER HEATER—CUT-OFF ALIKE IN BOTH CYLINDERS—VOLUME OF LARGE CYLINDER$=2\times$VOLUME OF SMALL CYLINDER.

Item.	1	2	3	4	5	6	7
	Single cylinder, condensing.		Compound, condensing.				Compound Non Condensing
	Steam jacket in use.	Steam jacket not in use.	Steam jacket, superheater and heater in use.		Steam jacket and superheat'r in use. Heater not in use.	Steam jacket, superheat'r and heater not in use.	Steam jacket, superheat'r and heater in use.
	Cut-off, $\frac{1}{4}$	Cut-off, $\frac{1}{4}$	Cut-off, $\frac{2}{3}$	Cut-off, $\frac{1}{2}$	Cut-off, $\frac{1}{2}$	Cut-off, $\frac{1}{2}$	Cut-off, $\frac{1}{2}$
Initial pressure of steam, lbs. per sq. in. above atmosphere	89.8	90.4	84.7	90.3	93.5	95.3	90.7
Lbs. of water evaporated per hour from and at 212°, per indicated horse-power......	34.8	41.1	29.9	29.	29.3	30.4	35.6
Water per hour not accounted for by diagrams from small cylinder.............	—	—	76	26	43	22	28
Water per hour not accounted for by diagrams from large cylinder........	157	341	368	141	143	304	95

compounding. These experiments show, in addition, the gain produced by the means usually employed to prevent cylinder condensation, viz., superheating and steam jacketing. It will be seen that the effect of the various changes made in the engines is to furnish examples of nearly every form of single and double-cylinder engines in use to-day, with the exception of double-cylinder engines having both cylinders jacketed, illustrations of which are given in subsequent tables. These examples may be classed as follows:

Table.	Experiment.	
II.	1, 2	Single cylinder without jacket.
III.	2	Single cylinder without jacket.
III.	1	Single cylinder, steam jacketed.
II.	3, 4	Double cylinder, with receiver, large cylinder not jacketed.
II.	5	Double cylinder, without receiver, large cylinder not jacketed.
III.	6	Double cylinder, with receiver, large cylinder not jacketed.
III.	3, 4, 5, 7	Double cylinder, with receiver, large cylinder steam jacketed.

The results of some experiments made with engines of United States Revenue steamers are contained in Table IV. These experiments are selected from a large number published in the "Transactions of the American Society of Civil Engineers," III., 368, and in the "Report of the Trial of the Steam Machinery of the United States Revenue Steamer Gallatin," Washington 1875.

It will be seen that the several experiments of each set are not in all cases strictly comparable, owing to variations in steam pressure and ratio of expansion, although the results here given have been selected with care from all the experiments, as being the most available for comparison. It is probable, however, that these experiments, taken as a whole, represent about the best, as well as the average performance of single and double-cylinder marine engines using saturated steam.

It has been stated that one of the means employed for preventing cylinder condensation is to superheat the steam before its admission into the cylinder of the engine; and the results obtained with a single-cylinder engine, not jacketed, and given in Table V., show that this method is very efficient. In these experiments, which were made in 1862, the amount of water evaporated by the boilers was not measured. After the completion of the experiments with saturated steam, however, and before putting in the superheaters, the evaporative performance of the boilers with anthracite coal was determined, so that an estimate can be made, with considerable accuracy, of the evap-

TABLE IV.

EXPERIMENTS ON SINGLE AND COMPOUND ENGINES OF UNITED STATES REVENUE STEAMERS.

Item.	1	2	3	4	5	6	7	8	9
	Rush.	Bache	Bache	Dext'r	Dallas	Gallatin.			
	Compound, condensing, with receiver. Both cylinders steam jacketed.	Compound, condensing, with receiver. Large cylinder steam jacketed.	Single, condensing, cylinder jacketed.	Single, condensing, cylinder not jacketed.	Single, condensing, cylinder not jacketed.	Single, condensing.		Single, condensing without vacuum.	
						Steam jacket not in use.	Steam jacket in use.	Steam jacket not in use.	Steam jacket in use.
Initial pressure of steam, lbs. per sq. in. above atmosphere	67.6	74.1	76.0	65.7	32.2	39.2	40.0	41.6	64.1
Ratio of expansion........	6.22	5.1	5.11	4.46	5.07	5.07	4.49	5.92	3.52
Lbs. of water evaporated per hour from and at 212°, per indicated horse power....	22.4	27.3	28.3	29.2	32.3	29.0	30.0	31.5	33.0

TABLE V.

EXPERIMENTS WITH SATURATED AND SUPERHEATED STEAM IN ENGINE OF STEAMER GEORGEANNA.

Item.	1	2	3	4	5	6	7
	Saturated steam.		Superheated steam.				
	Cumberland coal.		Cumberland coal.		Anthracite coal.		
Initial pressure of steam, lbs. per sq. in. above atmosphere.	23.1	17.2	22.5	28.2	31.3	22.7	16.4
Cut-off......	0.45	0.65	0.45	0.65	0.28	0.45	0.65
Indicated horse-power........	429	393	466	453	.456	451	433
Coal per indicated horse-power per hour..................	3.38	3.71	2.91	2.99	2.43	2.52	2.72
Temperature of steam........	265°	253°	344°	338°	336°	336°	322°
Probable evaporation per hour from and at 212°, per indicated horse-power, lbs......	—	—	—	—	23.45	24.41	26.25

TABLE VI.

EXPERIMENTS WIRH SATURATED AND SUPERHEATED STEAM IN SMALL NON-CONDENSING ENGINE.

Item.	1	2	3	4	5	6
	Saturated steam.			Superheated steam.		
Initial pressure of steam, lbs. per sq. in. above atmosphere.	50.4	50.2	50.3	50.4	50.0	50.2
Cut-off......................	0.247	0.465	0.701	0.248	0.461	0.685
Temperature of steam in steam pipe near throttle...........	302°	303°	303°	478°	441°	406°
Temperature of steam in cylinder.....	278° to 297°	279° to 296°	282° to 300°	313°	316°	315°
Temperature of steam in exhaust pipe................	210°	210°	213°	217°	210°	212°

oration in the engine experiments when anthracite coal was used. With regard to the temperatures given in Table V., it is stated in the original report of the trials that they are all probably too low, but it is probable also that the temperatures of the saturated and superheated steam all differ from the true figures by an amount which is sensibly constant. Objections have frequently been made to the use of superheated steam in the cylinders of engines, on the ground that it is injurious to the rubbing surfaces, since it prevents thorough lubrication. This objection is undoubtedly well founded, if the superheating is excessive, but when it is just sufficient to prevent condensation, the foregoing objection loses its force. This is illustrated in Table VI., which contains the results of some experiments made with the apparatus designed by Mr. G. B. Dixwell—consisting of a superheater so arranged that superheated steam could be mixed with saturated steam before its admission into the cylinder of the engine, to produce a mixture of any desired temperature—and a sensitive cylinder pyrometer by means of which the changes of temperature in the cylinder during each stroke could easily be determined. These experiments show that the amount of superheating necessary to prevent cylinder condensation is different for each ratio of expansion, and that if the required temperature is not exceeded, the resultant cylinder tempera-

tures are by no means excessive. Many instances are known to the writer of the benefits of superheating steam before admitting it into the cylinder of an engine, the effect of superheating being ordinarily to render the engine more economical and more powerful at the same time.

The double-cylinder engine is largely used on land, particularly in the case of engines employed for pumping water for public supply. The designer of a land engine is not limited to the space prescribed for the engine of a vessel, and there are several considerations which render it possible to make a land engine, in general, more economical than one of the marine type. Table VII. contains but few examples, but these have been chosen from a large number, with the idea of giving the extremes and average of the economy obtained from the most prominent types of double-cylinder land engines.

The modern forms of stationary steam engines, commonly known as automatic cut-off engines, are ordinarily single-cylinder engines, of excellent construction, and their performance is exceedingly economical, more so, in fact, than that of the average double-cylinder engine. The results obtained with such engines seem to be due to the high piston-speed, the small fraction of clearance, and the carefully designed valve-gear; the effect of these conditions being to maintain the cylinder pressure nearly equal to that in the boiler, until the point of cut-off, when the admission valve is quickly and tightly closed–while the high piston-speed makes the cylinder condensation a comparatively small fraction of the total steam consumption. A few examples of the performance of automatic cut-off engines, single-cylinder, with and without steam jackets, are shown in Tables VIII. and IX.

The examples that have been given in the preceding tables, although comparatively few in number, are believed to be representative ones. It becomes, therefore, an interesting question, to determine the commercial economy of these different forms of engines, when using steam furnished by boilers of varying evaporative efficiency.

In order to do this, the most economical and the most wasteful examples of each type of condensing engine have been selected, and the cost in coal and combustible of the steam used per hour for each indicated horse-power developed has been determined, with data given in the second part of the present article, for the most economical and the most wasteful boiler performance there illustrated, excluding the experiments in Table XXVI, where the conditions were abnormal. The results of this comparison are contained in Table X.

Table VII.

Tests of Compound Pumping Engines.

Item.	1	2	3	4	5
	Holly, Buffalo, no receiver, Cylinders steam jacketed.	Leavitt, Lawrence, no receiver Cylinders steam jacketed.	Warden, Cincinnati, receiver, Cylinders not jacketed.		Worthington, Roxbury, no receiver, Cylinders not jacketed.
			1st trial.	2d trial.	
Initial pressure of steam, lbs. per sq. in. above atmosphere......	67.9	89.5	125.	125.	39.
Ratio of expansion............	6.9	16.4	3.33	3.38	3.1
Lbs. of water evaporated per hr. from and at 212°, per indicated horse power............... ...	26.2	20.1	39.2	39.1	35.7

Table VIII.

Tests of Single Cylinder Steam Engines Automatic Cut-off, Condensing. Cylinders not Jacketed.

Item.	1	2	3	4
	Engines at Miller's International Exhibition, Cincinnati, 1880.			Harris-Corliss Engine at Works of National RubberCo.
	Reynolds-Corliss.	Harris-Corliss.	Wheelock.	
Initial pressure of steam, lbs. per sq. in. above atmosphere	92.5	91.6	91.4	70.1
Ratio of expansion	6.67	6.95	6.35	9.72
Lbs. of water evaporated per hour from and at 212° per indicated horse-power	23.9	23.7	23.6	23.3

Table IX.

Test of Corliss Steam Engine, Condensing, with and without Steam-Jacket, at Mulhouse.

Item.	1	2	3	4
	Steam-jackets not in use.	Steam-jacket of cylinder in use.	Steam-jackets of cylinder and piston in use.	
Initial pressure of steam, lbs. per sq. in. above atmosphere	66.2	68.1	67.9	67.2
Ratio of expansion	7.9	10.83	10.83	7.9
Lbs. of water evaporated per hour from and at 212° per indicated horse-power	30.45	23.11	22.56	22.59

Table X.

Pounds of Anthracite Coal and Combustible per hour per indicated Horse-Power, used by different types of Condensing Engines, obtaining steam from Boilers of different Economical Efficiency.

Type of Engine.	Reference Number.		Pounds of coal per hour, per indicated horse-power.		Pounds of combustible per hour, per indicated H. P.	
	Table.	Experiment.	Boiler, Table XXV, Experim't 5.	Boiler, Table XXI, Experim't 9.	Boiler, Table XXV, Experim't 5.	Boiler G. Table XV.
Double cylinder, steam jacketed.	VII	2	1.71	3.00	1.35	2.39
	III	3	2.55	4.47	2.00	3.56
Double cylinder, not jacketed	II	1	2.51	4.40	1.97	3.51
	VII	2	3.33	5.85	2.63	4.66
Single cylinder, steam jacketed.	IX	3	1.92	3.37	1.51	2.68
	III	1	2.96	5.20	2.33	4.14
Single cylinder, not jacketed	VIII	4	1.98	3.48	1.56	2.77
	II	2	4.41	7.75	3.48	6.18
Single cylinder, not jacketed, superheated steam	V	5	1.99	3.50	1.57	2.79
	V	7	2.23	3.92	1.76	3.12

REPORTS OF ENGINEERING SOCIETIES.

American Society of Civil Engineers, April 2d, 1884.—The Society met at 8 P.M., Vice President Wm. H. Paine in the Chair, John Bogart, Secretary.

Ballots were canvassed and the following candidates elected:—

As Members:—Thomas W. Baldwin, Bangor, Me.; Thomas E. Brown, Jr., New York City; Oren B. Colton, Chicago, Ill.; Stewart Derbishvie, Aylmer, Canada; Joshua L. Gillespie, St. Paul, Minn.; Minard L. Holman, St. Louis, Mo.; William T. Jennings, Toronto, Canada; Henry F. Juengst, St. Joseph, Mo.; Moritz Lassig, Chicago, Ill.; John F. O'Rourke, New York City; Geo. W. Rafter, Fredonia, N. Y.; Irving A. Stearns, Wilkesbaire, Pa.

As Juniors:—Frank E. Bissel, Sedalia, Mo.; Joseph A. Powers, Landsingburg, N. Y.; Commodore P. Ruple, Wilson's Point, La; William H. Starr, Buffalo, N. Y.

A paper by the late Wm. R. Morley, M. Am. Soc. C. E. on the Proper Compensation for Railroad Curves, which had been read at a previous meeting of the Society, was discussed.

Mr. A. A. Robinson, M. Am. Soc. C. E., stated that the location of the New Mexican Extension of the Atchison, Topeka and Santa Fe Railroad was made with the standard of compensation, based upon the theory that each degree of curvature was the equivalent in resistance to the movement of trains of $\frac{5}{100}$ of a foot of ascent. After the construction it was found: first, that upon maximum gradients 0.6 per cent. where a full train was 30 to 32 loaded cars, this compensation was hardly sufficient; second, upon maximum gradients $1\frac{13}{100}$ per cent. with full train of 18 to 20 cars, it was fully sufficient; third, upon maximum gradient of 3.4 per cent. with full train of 7 to 8 cars it was evidently greater than was needed. Mr. Robinson considers that the resistance due to curvature is affected by so many conditions that it cannot be determined by mathematical formula except for the particular conditions assumed in a special case. The speed of the train, the elevation of rail, the greater length of outer rail, the gauge of track as compared with that of the wheels, all affect the question, and the resultant of all the forces will be a function not only of the rate of curvature and of the gross tonnage of the train, but also of the number of cars in the train. A train of 10 cars will produce a resistance greater than 10 times that of a single car. Upon a division of the railroad where locomotives can pull 30 cars, the rate of compensation should be greater than upon a division where engines can pull but 10 cars. In practice Mr. Robinson has adopted the following rules for compensation:

Rate of maximum grade 0.0 to 0.6 per 100,
Compensation 0.06 per 100.
Rate of maximum grade 0.6 to 1.6 per 100,
Compensation 0.05 per 100.
Rate of maximum grade 1.6 to 3.0 per 100,
Compensation 0.04 per 100.

Mr. Wm. H. Searles, M. Am. Soc. C. E., discussed mathematically the nature and amount of the increase of resistance on curves due to the increase in number of cars upon a train, and deduced a general formula for the total tractive force necessary to be applied at the head of a train of cars moving at a uniform velocity on a given curve, and presented tables giving the coefficient for solving such formula in terms of the number of cars and the degree of curve. He also presented tables of resistances for given trains upon certain grades, and a summary of equivalent grades per station per degree for a given train upon various curves and grades. Also a table of resistances for a consolidation engine of 60 tons hauling its maximum train on a 20 degree curve. He also expressed the opinion that widening the gauge on curves was not an advantage as far as a four-wheeled truck is concerned.

Mr. Lewis Kingman, C. E., stated that on the Atlantic and Pacific Railroad the compensation $\frac{5}{100}$ per degree was adopted for all curves; that 10-degree curves were the maximum, and that all curves were cased off at both ends by compounding gradually from the tangent to the full degree of curvature. This practice he considered of great value and of very slight additional expense. From careful observation upon the action of locomotives pulling trains upon curves, it is his opinion that the compensation should vary with the grade.

Mr. A. M. Wellington, M. Am. Soc. C. E., said that the rule adopted by him was for the high grades, the same as that of Mr. Morley, viz: $\frac{4}{10}$ per degree of curvature, but that Mr. Morley increased this to $\frac{6}{10}$ per degree on lower grades. The theory upon which this was done was formerly advocated by Mr. Wellington, but he now believes from further investigation, both experimental and theoretical, that no sensible difference exists due to the longer trains which could be run upon the lower grades. No absolutely fixed rate of compensation ought to be made. Circumstances of location may make it inexpedient to adopt a rate of compensation which otherwise might be desirable. The rate of compensation of $\frac{1}{10}$ per degree is higher than is ever necessary unless in certain cases at stations or where it may become a question whether to admit certain sharp curves at all. It is extremely probable that curve resistance is materially greater at very slow speeds. It is unfortunate that further and more complete experiments cannot be secured than have already been made.

Mr. M. N. Forney, M. Am. Soc. C. E., referred to the fact that the conditions of rolling stock and of track were very important elements in any experiments that could be made upon this subject. Different car-builders made cars and trucks and wheels according to their individual notions. Engineers made the section of rail according to their individual notions. The actual condition of the rolling stock of the country was such that it was doubtful whether any experiments could be relied upon as determining results which could be applied generally.

Mr. Wellington expressed the opinion that while it was perfectly true that the condition of rolling stock was as stated by Mr. Forney, yet that experiments properly made could be relied upon to give fair average data.

The subject was further discussed by members present.

ENGINEERS' CLUB OF PHILADELPHIA— REGULAR MEETING, APRIL 5TH.—Past President Henry G. Morris in the chair; 36 members and 2 visitors present.

Mr. Henry G. Morris exhibited, on behalf of Mr. Israel W. Morris, two ancient and curious works upon mining: "The Golden Treasury, or the Complete Miner; being Royal Institutions or Proposals for Articles to Establish and Confirm Laws, Liberties and Customs of Silver and Gold Mines," by Thomas Houghton, London, 1699; and "A Collection of Scarce and Valuable Treatises upon Metals, Mines and Minerals," by James Hodges, London, 1740. The latter contains, *inter alia,* "How to know the Condition of the Earth by Taste," "Of Juices, and first of Allum." "The Opinion that Quicksilver and Sulphur are the Matter whereof Metals are made, is defined." "How to know the ill Qualities that infect the Oar, and how to Purge them away." "Wherein is showed how true and perfect Gold may be made by Art, with loss to the Workman," etc.

Mr. Henry G. Morris gave a brief description of an atmospheric elevator, consisting of a closed cage or car working in an air-tight well, the air pressure, supplied by a "root" or other pressure blower, being admitted to the top or bottom of the cage in descending or ascending. The doors at the different stories opening inwards, the pressure of air keeps them closed until the interior of the car is brought opposite, when the pressure being relieved the door can be opened into the car. The car being counterbalanced, only a comparatively slight pressure of air, equal to a water column of 6 to 8 inches only, is required to move an average load on a car six feet square. The escape of air beneath the car being at all times readily controlled by the attendant, it is impossible for the car to descend at a dangerous speed, and other obvious features render this form of elevator comparatively safe.

Mr. Henry G. Morris also exhibited a sample of seamless copper tube which had been compressed endwise under a steam hammer, and showed peculiar foldings of the metal into overlapping equilateral triangles forming an interior hexagonal section. The absence of fractures showed great purity of material.

Mr. John T. Boyd described a new design for parlor cars for the Pennsylvania R. R.

The Secretary presented, for Mr. Edward Parrish, an illustrated description of Powers' Disinfecting Tank and Automatic Siphon.

The Secretary presented, for the Reference Book, a table which he had prepared of vulgar fractions of 1 in. reduced to *exact* decimals of 1 in.

Mr. Wm. L. Simpson exhibited a remarkably perfect casting of a toad, the pattern used being the toad himself.

ENGINEERING NOTES.

AN ARTESIAN WELL, AT BOURNE, LINCOLNSHIRE. By James Pilbrow, M. Inst., C. E. The subject of artesian wells is not without interest to the engineer, whose attention is chiefly directed to the supply of towns and other places with water. For this reason, the description of a small but productive artesian well, completed at Bourne, in Lincolnshire, in 1856, is presented. The well was intended to supply the town of Bourne with water, the undertaking being in the hands of a small joint-stock company. The town had been until then without any public supply, and almost without a private one. The wells were shallow, as in most of the towns in that part of the county; but many houses were wholly dependent upon carts, which fetched water from a considerable distance. These circumstances gave increased importance to the fact of such a supply being found under the site of the place.

The boring, 4 inches in diameter, passed through several oolitic strata, to a depth of 92 feet. Below the alluvial soil and gravel, a hard shelly limestone, 32 feet in thickness, was encountered. The bore hole here was made slightly conical to admit of the taper end of a cast-iron pipe being inserted and driven tightly, to exclude any surface water, and to prevent water from the bore escaping into the gravel, and thus lose its full power to rise above the surface. The boring was then continued, through various beds, till it reached a stratum, 6 feet thick, of compact and hard rock, in passing through which, at 92 feet below the surface, the tool fell suddenly about 2 feet, evidently into a chasm or hollow, striking upon the hard surface of the underlying rock. The water immediately rushed up with great force, and drove the men from their work; and it was not without difficulty that the joints for attaching the curved pipe and sluice-valve at the surface could be accomplished.

The site of the town of Bourne partakes of the ordinary character of the county, and is flat; the highest part, where the well is situated, being only about 6 feet above the general level. It had been the intention of the author, should the water rise with sufficient force, as he believed it would do, to supply the town direct from the boring, and in this way the work was carried out, the flow and pressure having proved even greater than was anticipated.

An air chamber was fixed at the well to regulate the pressure, and to equalize the supply of water to the town. The water rose at the Town Hall exactly 39 feet 9 inches above the ground. The yield at the bore and surface level, ascertained by filling a tank capable of containing 5,000 gallons, was at the rate of 567,000 gallons per day, and there was no diminution on letting the whole run continuously to waste. The yield was also tested by a "notch-board," which, by using the coefficient 0.563, and measuring at still water and not at the "crest," gave 575,201.8 gallons.

The author knows of no other boring of like dimensions, either in this country or on the Continent, which yields so large a quantity of water, or where, the boring being made on the general level of the surrounding district, the water from which flows to so great a height above the ground.

It is needless to say that the town of Bourne has since enjoyed an unlimited supply of pure

water without the assistance of engine, pumps, or reservoirs, and in far greater quantity than it requires. The town of Spalding, several miles distant, has subsequently been supplied from the same source, the water being conveyed by pipes laid under the turnpike road.

The water mains were laid under every street, with fire-cocks at intervals, and it was satisfactory to all, and surprising to some, to see the water thrown upon the roofs of houses by a hose and jet-pipe, as from a fire-engine, and that only by the natural pressure of the spring.

The water, by Professor Brand's test, gave 19.4 degrees of hardness, arising chiefly from the presence of bi-carbonate of lime; but by boiling it is rendered much softer.—*Papers of the Institution of Civil Engineers.*

IRON AND STEEL NOTES.

The American Bessemer Steel Industry.—The following table shows the comparative production of ingots and rails by Great Britain and the United States in the last four years:

	1880. Gross Tons.	1881. Gross Tons.	1882. Gross Tons.	1883. Gross Tons.
Ingots.				
Great Britain....	1,044,382	1,441,719	1,673,649	1,553,380
United States ...	1,074,262	1,374,247	1,514,687	1,447,345
Excess, U. S...	29,880	—	—	—
Excess, G. B..	—	67,472	158,962	76,035
Rails.				
United States....	852,196	1,187,770	1,284,067	1,148,709
Great Britain....	739,910	1,023,740	1,235,785	1,097,174
Excess, U. S..	112,286	164,030	48,282	51,535

It will be observed, says the *Bulletin* (New York), that since 1880 Great Britain has annually made more Bessemer ingots than the United States, and that in the last four years the United States has annually made more Bessemer Steel rails than Great Britain. The shrinkage in the production of Bessemer steel rails in the United States in 1883 as compared with 1882 was 151,601 net tons, or 135,358 gross tons, which is 3,253 tons less than the shrinkage of 138,611 gross tons in the British production in the same period.

The New B.G. Wire Gauge.—The following is a copy of the new B.G. wire gauge, which has been drawn up by the South Staffordshire Mill and Forge Wages Board, and submitted to the Board of Trade for approval:—

No. Gauge.	Inch.	Inch.	Millimetres.	Lb.
3/0	$\frac{1}{2}$	0.500	12 700	20
2/0		0.4452	11.308	17.808
1/0		0.3964	10.068	15.856
1		0.3532	8.971	14.128
2		0.3147	7.993	12.588
3		0.2804	7.122	11 216
4	$\frac{1}{4}$	0.250	6.350	13
5		0.2225	5.651	8.90
6		0.1981	5.032	7.924
7		0.1764	4.480	7.056
8		0.1570	3.988	6.28
9		0 1398	3.551	5.592
10	$\frac{1}{8}$	0.1250	3.175	5
11		0.1113	2.827	4.452
12		0 0991	2.517	3.964
13		0.0882	2.240	3.528
14		0.0785	1 994	3.14
15		0.0699	1.775	2.796
16	$\frac{1}{16}$	0.0625	1.587	2 50
17		0 0556	1.412	2.224
18		0.0495	1.257	1.98
19		0.0440	1.118	1.76
20		0.0392	0.996	1.568
21		0.0349	0.886	1.396
22	$\frac{1}{32}$	0.03125	0.794	1.25
23		0.02782	0.707	1.1128
24		0 02476	0 629	0.9904
25		0 02204	0.560	0.8816
26		0 01961	0.498	0 7844
27		0.01745	0.4432	0.698
28	$\frac{1}{64}$	0.015625	0.3969	0 625
29		0.0139	0.3531	0.556
30		0.0123	0 3124	0.492
31		0.0110	0.2794	0.440
32		0.0098	0.2489	0.392
33		0.0087	0.2210	0.348
34		0.0077	0.1956	0.300
35		0.0069	0.1753	0.276
36		0.0061	0.1549	0.244
37		0.0054	0.1371	0.216
38		0.0048	0 1219	0.192
39		0.0043	0.1092	0.172
40		0.00386	0.0980	0.1544
41		0.00343	0 0871	0·1372
42		0.00306	0.0777	0.1224
43		0.00272	0.0691	0.1088
44		0 00242	0.0615	0 096
45		0.00215	0.0546	0.0868
46		0.00192	0.0488	0 076
47		0.00170	0.0432	0.0688
48		0.00152	0.0386	0.0608
49		0.00135	0.0343	0.054
50		0.00120	0.0305	0.048
51		0.00107	0.0272	0.0428
52		0.30095	0.0241	0.038

The Oxidizability of Iron.—M. Gruner has published in *La Metallurgie* the result of a year's researches on the oxidizability of iron and steel under the influence of moist air, fresh, sea, and acidulated water. The results obtained are very instructive. Iron is dissolved rapidly by sea water, cast iron losing about half

as much as steel, and spiegeleisen is the most powerfully acted on by sea water.

Long Steel Rails.—The Osnabruck Steel Works have recently been manufacturing steel rails 88 feet 6 inches long, which have been laid down on railway bridges crossing the city of Hanover. It was found that the noise caused by passing trains was becoming such a nuisance that a remedy had become a necessity. The cause of it was the violent vibration at the rail joints, and the engineers hit upon the expedient of having rails made long enough to cover the whole length of the bridges. Since they were laid down, the nuisance caused by the rail joints has ceased. The use of rails of the length stated is, as far as we know, without a parallel in the history of railway construction, and reflects credit alike on the engineers who suggested it and the manufacturers who made them.

Trials of Armorplates.—A competitive trial of armorplates took place on the island of Amager, near Copenhagen. There were four plates in all, one of steel from the Creusot Company, in France; one plate by Marrel, Rive de Gier, in France; and two compound plates from Sheffield, one made by Cammell & Co., and the other by Brown & Co. These plates were 9 inches in thickness, and were about 6 feet 6 inches long by 5 feet wide, and were attached by bolts to a wood backing 9 inches in thickness, supported by iron skin plates and frames in the rear. The guns used were the 6 inch and 10 inch rifled guns, with steel projectiles. The shot from the 6-inch gun appeared to have much the same effect upon each of the plates, the Sheffield plates showing the least penetration. The shot from the larger gun knocked the left half of the Creusot steel plate completely away, thus laying open the target without any protection whatever from any other projectile, that might be fired at it. The same projectile passed through the other plates, but it was broken up by the English plates, which were not otherwise seriously damaged. It appeared as if the energy from the 10-inch gun was too great for any plate having a thickness of only 9 inches, but it was evident that the Sheffield plates proved to be the best under the exceptional conditions of firing.

RAILWAY NOTES.

A writer in the *Economiste Francais* describing the present condition of communications by water and railway in France, states that there are now 4,575 miles of navigable rivers, and 2,900 miles of canals. In the year 1852 there were only 4,190 miles of river navigable, and 2,440 miles of canal, the increase in the length of river being 385 miles, and in that of canals 460 miles. But, as the writer in the *Economiste Francais* points out, this increase has been effected at an enormous expense, the total amount spent from 1852 up to the year 1878 being close upon £14,000,000, while since then, under the scheme of M. de Freycinet for the development of communication by land or water, a further sum of £11,000,000 has already been spent. Thus in round figures the cost since 1852 has been £25,000,000 sterling, and yet the quantity of goods carried by water has only increased 4,000,000 tons. While the increase in the goods traffic upon the railways has been 4 per cent., upon the rivers and canals it has not exceeded 1 per cent., and this the writer in the *Economiste Francais* attributes, not to the unfair competition of the railways, but to the fact that canals cost as much to make as railways, and that the time occupied in transit is so much longer by water than it is by rail.

ORDNANCE AND NAVAL.

The Russian Minister of War has ordered a large number of Krupp guns, which were procured from Essen some years ago, to be considerably lengthened, and they are at present undergoing this process at the St. Petersburg Arsenal. The barrels of the guns are first bored out to a considerable depth, and steel tubes of the required length are then inserted. Guns converted on this principle are reported to have given satisfactory results.

A steel gunboat for the French Navy was recently launched at Rochefort: she was named "Le Gabes," and is of the following dimensions: Length between perpendiculars, 149ft. 9in.; extreme breadth of beam, 23ft. 7in.; depth of hold, 10ft. 6in.; draught of water, 8ft. 6in. Her displacement ready for sea is 455 tons, and she will be fitted with engines of 450 indicated horse-power. It is estimated that she will realize a speed of upwards of 10 knots an hour.

BOOK NOTICES.

Publications Received.

Proceedings of the American Academy of Arts and Sciences; papers XXI., XXII., XXIII.

Geological Survey of New Jersey. Annual Report of the State Geologist for 1883.

Notes on Cylinder Bridge Piers. By John Newman, As. M. I. C. E.

Papers of the Institution of Civil Engineers; the following have been received from Mr. Jas. Forrest, Sec'y:

Water-Raising Machines in Holland, by G. Cupari.

Mining and Treatment of Gold Ores in Japan, by Robt. James Freckeville, Assoc. M. I. C. E.

Examination and Testing of Portland Cement, by Henry Faija, Assoc. M. I. C. E.

Timaru Water Supply, by Arthur Dudley Dobson, M. I. C. E.

Electrical Conductors, by William Henry Preece, M. I. C. E.

Energy in Nature. By Wm. Lant Carpenter, B. A., B.Sc. London: Cassell & Co.

This is a popular treatise designed to afford useful hints to teachers of physical science. The topics treated are Matter and Motion, Heat, Chemical Attraction, Electricity and Chemical Action, Magnetism and Electricity, and Energy in Organic Nature.

The book is tastefully printed and illustrated, and may be read with profit by young learners.

HEAT. By P. G. Tait, M. A. London: Macmillan & Co.

The intention of the author in preparing this work was to satisfy the wants of "the students who, without any intention of entering on a scientific career, whether theoretical or experimental, are yet desirous of knowing accurately the more prominent facts and theories of modern science to such an extent as to give them an intelligent interest in physical phenomena."

The book will doubtless be widely read, but the learner will be frequently at a loss to comprehend the exact meaning of the author, and at those points where special pains have been taken to be precise and clear.

TABLES FOR COMPUTING METRIC AND NON-METRIC WEIGHTS AND MEASURES. By W. A. G. Emonts, C. E. Philadelphia: William Syckelmore.

Inquiries are often made for convenient tables for reduction of weights and measures from the yard to the meter and *vice versa*. This little pamphlet of Mr. Emonts' fulfills the demand completely. In six pages of tables are all the ratios between the two systems which can reasonably be asked for.

THE PRINCIPLES AND PRACTICE OF ELECTRIC LIGHTING. By Alan A. Campbell Swinton. New York: D. Van Nostrand.

The aim of this work, as explained by the author, has been to prepare a treatise which should be on the one hand sufficiently simple and devoid of technicalities, to be easily understood by unscientific readers, and on the other, sufficiently comprehensive and up to date to give reliable information on all the principal appliances and systems. The plan is well carried out. Starting with the theory of electric lighting and explaining the nature of the problem to be solved, the author next gives a clear though popular account of the systems of units and the appliances for their measurement.

Sources of Power, Generators, Lamps, Accumulators, Systems of Lighting, are then presented in order The work closing with applications and their cost.

To people desiring the clearest and briefest exposition of this very interesting subject we recommend this book.

THE ART OF SOAP MAKING; a Practical Handbook of the Manufacture of Hard and Soft Soaps, &c. Including many new Processes, and a Chapter on the Recovery of Glycerine from Waste Leys. By Alexander Watt. With numerous Illustrations. London: Crosby Lockwood & Co.

Our London contemporary *Engineering*, in an introduction to a long review of this book, says:

"In a short preface, the author of this book modestly disclaims all pretension to originality, assuming only the credit due to an industrious compiler from many sources, and to a painstaking recorder of some of the chief processes in a great and ancient industry, unrepresented previously in this country by any special volume devoted to it. We think the readers of Mr. Watt's work will be disposed to accord to him more credit than he appears to consider his due, and manufacturers will doubtless derive much useful information from its pages."

The manufacture of many varieties of soap is described with special reference to the chemical and mechanical details, and to the improvement of new over old methods. Nine different modes of recovering the glycerine are described.

Useful notes and tables are appended, together with an excellent index.

MISCELLANEOUS.

M. HEDDEBAULT has discovered a method of preparing soluble wool from tissues in which wool and cotton are combined. When subject to a current of superheated steam, under a pressure of five atmospheres, the *Scientific American* says, the wool melts and falls to the bottom of the pan, leaving the cotton, linen, and other vegetable fibres clean and in a condition suitable for paper making. The melted wool is afterwards evaporated to dryness, when it becomes completely soluble in water, and is called azotine. The increased value of the rags is sufficient to cover the whole cost of the operation, so that the azotine is produced without cost. It contains all its nitrogen in a soluble condition, and can, therefore, be compared to dry blood, which is worth 2.50 francs per kilogramme of nitrogen. M. Ladureau regards this discovery as one of great interest for agriculture and mechanical industry.

DURAND has explained the spontaneous ignition of coal as being due to the presence of pyrites, which on oxidization under suitable conditions inflames and then sets fire to the coal in which it is imbedded. According to Fayol's experiments, however, the real cause of this phenomenon is the oxidation of the coal itself and not of the pyrites. The absorption of oxygen by coal—carbon—takes place more or less readily according to the temperature and the coal being more or less finely divided. According to the "Journal" of the Society of Chemical Industry, lignite in the state of fine dust takes fire at 150 deg., gas carbon at 200 deg., coke at 250 deg., and anthracite at 300 deg. or above. On heating a mixture of finely-powdered coal and pyrites to 200 deg. for four days the coal took up 6 per cent. of oxygen, whilst the pyrites absorbed only 3.5 per cent. Hence coal absorbs oxygen much more energetically than pyrites does, which has also been confirmed by the following experiment: About 900 grams of powdered coal and 3,350 grams of powdered pyrites were placed in tin cans and put in a drying chamber. Up to 135 deg. both behaved similarly, but from there the temperature of the pyrites remained almost stationary, whilst that of the coal quickly rose, ignition taking place after a few hours. Two other samples of coal and pyrites were put in a chamber at 200 deg. The temperature of the coal quickly increased. After forty minutes it got up to 200 deg., and the coal took fire, whilst the pyrites had at the same time only been raised to 150 deg. The ignition of the coal was not at all hastened by an admixture of pyrites.

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THE COMPOUND STEAM ENGINE, CONSIDERED IN CONNECTION WITH ITS STEAM-GENERATING PLANT.

By RICHARD H. BUEL, C. E.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

II.

II. THE BOILER.

The use of higher steam pressures in marine boilers, which was a part of the change made in introducing the double-cylinder engine, rendered it necessary to abandon the rectangular form of boiler, and to replace it by a boiler with cylindrical shell, this shell containing large flues in which the furnaces were located, and return fire-tubes over these flues. Such a boiler may be regarded as the standard type adopted for marine use at present. The material composing the shell and flues must be made very thick, so that a modern marine boiler is not only very heavy; but the great thickness of the flues prevents the ready transfer of heat, and repairs are frequently necessary. It is very evident, from the costly experiments made by steamship companies with several types of water-tube or sectional boilers, that a reliable and efficient substitute for the cylindrical marine boiler would gladly be adopted, if it could be found. Some examples of the performance of the most prominent types of steam boilers may aid in the solution of this question, and may furnish some hints to those who use boilers in connection with stationary engines.

How much water a good boiler ought to evaporate for each pound of fuel consumed is a question which requires for its answer some knowledge of the theoretical value of this fuel, or its total heat of combustion. Messrs. Favre and Silbermann have determined the total heat of combustion of carbon and hydrogen, which form the combustible elements of coal, and it has been inferred, very naturally, that the results obtained by these experimenters could be used to determine the total heat of combustion of a variety of coal whose analysis was known. So natural, indeed, did this inference appear to be that, so far as can be determined from published records, it was not until 1867 or 1868 that any one thought of testing the truth of the inference by experiment. About this time Messrs. Kestner and Meunier commenced to make experiments on the total heat of combustion of the elementary combustible compounds of the coal, and then of the coal itself. These experimenters made use of apparatus substantially similar to that employed by Messrs. Favre and Silbermann, and obtained results which were nearly the same as those given by the earlier experiments. On testing the total heat of combustion of different varieties of coal, however, Messrs. Kestner and Meunier

Table XI.

Summary of Experiments by Messrs. Kestner and Meunier on Heat of Combustion of Various Coals.

Number for reference.	Name.	Combustible and volatile constituents.			Heat of combustion per lb. of combustible, by experiment.	
		Carbon.	Hydrogen.	Nitrogen and oxygen.	Thermal units.	Pounds of water evaporated from and at 212°.
1	Saarbrück, Sulzbach	83.05	4.95	12.	15,485	16.02
2	" Von der Heydt	81.56	4.98	13.46	15,232	15 76
3	Creusot, caking (Chaptal shaft)	88.48	4.41	7.11	17,320	17.92
4	" anthracite (St. Pierre shaft).	92.36	3.66	3.98	17,021	17.61
5	" semi-bituminous (St. Paul)	90.07	4.1	5.13	16,965	17.55
6	" flaming (St. Paul)	90.79	4.24	4.97	16,673	17.25
7	Blanzy, Montceau,	78.58	5.23	16.19	14,985	15.5
8	" anthracite	87.02	4.72	8.26	16,400	16.97
9	Anzin	84.45	4.21	11.32	16,663	17.24
10	Denain	83.94	4.43	11.63	16,290	16.85
11	English, Bwlf	91.08	3.83	5.09	15,804	16.35
12	" Powell Duffryn	92.49	4.04	3.47	16,108	16.66
13	Russian, Grouchefski anthracite	96.66	1.35	1.99	14,866	15.38
14	" Miouski caking	91.45	4.5	4.05	15,651	16.19
15	" Goloubofski flaming	82.67	5.07	12.26	14,438	14.94
16	Lignite, blue, from Rocher	72.98	4.04	22.98	11,669	12.07
17	" flaming, from Manosque	70 57	5.44	23.99	13,253	13.71
18	" lean, from Manosque	66.31	4.85	28.84	12,584	13.02
19	" caking, from Bohemia	76.58	8.27	15.15	14,263	14.76
20	" changing to fossil-wood	66.51	4.72	28.77	11,444	11.84
21	Fossil-wood, changing to lignite	67.60	4.55	27.85	11,360	11.75
22	Russian lignite, from Toula	73.72	6.09	20.19	13,837	14.31

Table XII.

Combustible Constituents of American Coals, and Pounds of Air supplied per Pound of Coal, as determined by Prof. Johnson.

Number for reference.	Locality.	Designation of Coal.	Combustible constituents, parts in 100.		Lbs. of water evaporated from and at 212° per lb. of combustible, in experimental boiler.	Lbs. of air supplied per lb. of coal.
			Fixed Carbon	Volatile combustible.		
1	Pennsylvania.	Beaver Meadow, Slope No. 3.	97.38	2.62	10.11	20.4
2	"	Beaver Meadow, Slope No. 5.	97.11	2.89	10.84	28.5
3	"	Forest Improvement	96.71	3.29	11.02	21.7
4	"	Peach Mountain	96.77	3.23	11.05	18.4
5	"	Lehigh	94 41	5.59	9.75	47.9
6	"	Lackawanna	95.61	4.39	10.93	19.4
7	Virginia	Natural Coke of Virginia	85.33	14.67	10.63	19.7
8	Maryland	Neff's Cumberland	85.47	14.53	10.81	20.1
9	"	Atkinson & Templeman	83.16	16.84	11.87	18.3
10	Virginia	Midlothian, screened	60.77	39.23	10.18	19.9
11	Pennsylvania.	Cannellton	63.23	36.77	7.88	15.9

found that it exceeded, in every instance, the heat as calculated from the combined effects of the combustible elements of the fuel. Their experiments were very extended, and their mode of experimenting and their results have been published in the "Bulletin de la Société Industrielle de Mulhouse," 1868, 1869, 1871; in the "Annales de Chimie," 4th Series, XXX., 5th Series, II, XXI, 6th Series, II; and in the "Comptes Rendus de l'Académie," 2d Series, 1869. Table XI. contains a summary of the most important results of these experiments, and shows conclusively that the ordinary method of estimating the heat of combustion of a given fuel from its elementary analysis is entirely erroneous. There are no American coals in this table, but as the best anthracite coal of the United States contains more fixed carbon than any specimen of Table XI. (See Table XII.) It is reasonable to suppose that it is at least equal in heating effect to the best European coal tested by Messrs. Kestner and Meunier.

The results given in the preceding table are now generally used by European engineers in their calculations. Dr. Percy, in referring to the experiments of Messrs. Kestner and Meunier, states that more extended data will be necessary for the explanation of the results. M. L. Gruner, however, in a very interesting article on "The Classification and Heating Power of Coals" (a translation of which is published in the *Engineering and Mining Journal*, vol. XVIII.), shows clearly that it is the generally accepted formula which is anomalous, rather than the results. He remarks as follows:

"Dulong proposed the formula:

$$P=14544\,C+62032\,(H-\frac{O}{8})\,;$$ where P=heating power in thermal units, C= weight of carbon in pounds, $H-\frac{O}{8}$= weight of *free* hydrogen, *i. e.*, the total hydrogen less that already burnt to *water* by the oxygen that the coal contains. Doubtless Dulong considered this formula as giving only a kind of *industrial* value, for he knew well enough that we cannot, in a calorific sense, assimilate a ternary chemical compound to a simple mixture of C and H; and that the H is not simply combined with the O in coal. But at that time, at least, it was thought that C and H, considered as simple bodies, possessed always the same calorific power. The influence of molecular construction on the calorific power of bodies was ignored; it was not known that the heat of combustion of a body, simple or compound, is, in general, greater in proportion as its molecular condensation is less advanced.

"It is now established by the labors of Favre, Silbermann, Regnault, Berthelot and others, that the heat of combustion, like the specific heat, varies with the density.

	Heat units.
"We know that if carbon from wood charcoal develops	14544,
The charcoal of gas retorts, which is more dense, gives only	14485.
Natural graphite	14035.
The diamond only	13986.
We know also that the heating power of crystallized sulphur is	4072,
While that of the denser amorphous S, run in a melted state into water, is only	3991.

"It follows from this that to apply Dulong's formula to coals, we should substitute for the calorific power of hydrogen in a gaseous state that of hydrogen in a solid state, and, instead of 14544, which represents the heat of combustion of carbon having, according to M. Violette, a density greater than 2, we should put the greater number corresponding to the less condensed state of the carbon in coals.

* * * * *

"The actual heating power of all fuels, except the bituminous lignite of Bohemia (which, from the large amount of hydrogen it contains, resembles the petroleums) is not only greater than the heating power calculated by Dulong's formula, but, also, for the anthracites and bituminous coals, it is greater than the sum of the heat units due to the entire amount of carbon and hydrogen contained in them, taking them as isolated, and after making deduction for the oxygen.

"Thus, coal from the Chaptal mine gave
17320 heat units against C+H=15606
Coal from the Ronchamp mine gave
16399 heat units against C+H=15822
Coal from the Denain mine gave
16290 heat units against C+H=14958
Coal from the Louisenthal mine gave
14787 heat units against C+H=14083

"M. Scheurer Kestner, noticing this apparent anomaly, concluded that the combination of carbon and hydrogen in coals was attended with absorption of heat, as is the case with explosive compounds. The anomaly is, however, only apparent. Coal has none of the properties of explosive substances, and if the actual heating power is higher than that obtained from calculation, it is, as we have already remarked, because we take the figures 14544 heat units for carbon, while, in reality, we should take a number nearer to 20186, which is the theoretic heating power of carbon in the form of gas.

"The number 20186 heat units is obtained as follows: We know that carbon develops 4451 heat units when it is transformed into oxyd of carbon, and this gas, in its turn, gives $(14544-4451) = 10093$ heat units in combining with a new equivalent of oxygen; that is, equal quantities of oxygen apparently develop very unequal quantities of heat; I say *apparently*, for in the first case the solid carbon passes to the gaseous condition, while in the combustion of carbonic oxyd it is carbon in the condition of gas that burns. Now, if we admit with Rankine that Welther's law obtains so long as the chemical reactions are neither accompanied nor followed by a change of condition, we see that the excess of $(10093 - 4451) = 5642$ heat units should correspond exactly with the heat absorbed by the gasification of carbon, and consequently gaseous carbon would develop $(14544 + 5642) = 20186$ thermal units, if it gave carbonic acid directly.

"In any case it is evident that considering this number 20186 heat units merely as the result of a purely theoretical speculation, the but *slightly condensed* carbon of bituminous coal should produce more heat than pure carbon from wood charcoal. On the other hand, for *solid* hydrogen we should take a number smaller than 62032 heat units, which corresponds to gaseous hydrogen producing water, which is also taken in the gaseous state.

"We can readily satisfy ourselves that we would obtain values nearer the truth by assuming, for example, 16200 heat units as the heat of combustion of carbon in coal, and 54000 heat units as that of solid hydrogen; we would thus find, after deducting for the oxygen:

"For coal from Anzin...	15957	instead of	16663
For coal from Denain..	15989	"	16290
For short-flaming fat coal from the Chaptal mine (Creuzot)...........	16715	"	17320
For coal from Ronchamp.	16893	"	16399
For dry coal from Montceau..................	15554	"	14985
For dry coal from Louisenthal.................	14980	"	14787

"The agreement is not, however, satisfactory; we note that the calculation gives too low a value for coals rich in fixed carbon, and, on the other hand, too high a value for those which leave but little coke. In brief, it is evident that the manner of combination of the elements in coals is too variable to make it possible to determine their true heating power from simple elementary analysis. We must, therefore, determine by direct experiment the heating power of each kind of coal, or else we must be satisfied with the mean results which we obtain by combining the Mulhouse experiments—a summary of which is given in the above table—with the numerous industrial tests made by Dr. Brix, in Berlin, and by the French and English navies."

The useful heating effect obtained from coal consumed in the furnace of a boiler is, of course, considerably less than the total heating effect, on account of such inevitable losses as the amount of heat carried off in the products of combustion, the unconsumed fuel that drops through the air spaces of the grates, the loss due to imperfect combustion, and the heat radiated from the external surfaces of the boiler or its setting. The useful heating effect depends also upon the proportions and design of the boiler, and upon the amount of air admitted to the furnace. In regard to the amount of air required for combustion it is commonly assumed, from the experiments of Prof. Johnson ("Report to the Navy Department of the United States, on American coals," Washington, 1844), that with natural draft, the air supplied should be twice the amount theoretically required, or about 25 lbs. of air per lb. of coal burned. The experiments of Prof. Johnson hardly bear out this idea, as will appear from an inspection of Table XII., which has been taken from the report cited above. Messrs. Kestner and Meunier, after determining the total heating effect of different varieties of European

TABLE XIII.

HEAT OF COMBUSTION OF FOREIGN COALS, AND POUNDS OF AIR SUPPLIED PER POUND OF COAL, AS DETERMINED BY MESSRS. KESTNER AND MEUNIER.

Number for reference.	Designation of Fuel.	Heat of combustion, in thermal units, per lb. of combustible, by experiment.	Useful effect, per pound of combustible, in experimental boiler. Thermal units.	Useful effect, per pound of combustible, in experimental boiler. Pounds of water evaporated from and at 212°.	Pounds of air supplied per pound of coal.
1	Ronchamp, No. 1	16,346	10,057	10.4	12.6
2	" No. 2	16,411	10,438	10.8	12.8
3	Friedrichsthal	15,223	8,928	9.24	12.6
4	Duttweiler	15,703	9,484	9.81	13.4
5	Luisenthal	14,787	8,348	8.64	11.5
6	Altenwald	15,539	9,421	9.75	12.8
7	Heinitz	15,277	8,941	9.25	11.8
8	Sulzbach	15,212	8,847	9.15	13.5
9	Von der Heydt	15,232	8,789	9.09	12.6
10	Blanzy, Montceau	14,985	8,446	8.74	10.6
11	" anthracite	16,380	9,920	10.26	12.5
12	Creusot	16,942	10,472	10.83	20.5
13	$\frac{2}{3}$ Creusot, $\frac{1}{3}$ Ronchamp No. 1	16,758	11,268	11.66	17.1
14	$\frac{2}{3}$ Creusot, $\frac{1}{3}$ Ronchamp No. 2	16,758	11,131	11.52	16.1
15	Wood charcoal	14,544	8,698	9.	23.4

coal, experimented with several of the varieties in the furnace of a steam boiler. They also tried the effect of admitting different amounts of air, and found that the best results with natural draft were obtained when the air supply was about $1\frac{1}{3}$ times the amount theoretically required. A summary of the results obtained by Messrs. Kestner and Meunier is given in Table XIII.

It may be useful to illustrate by an example, the effect of employing an erroneous assumption instead of an experimental result in a discussion of boiler performance. The experiment selected for discussion is No. 9, Table XXV., and the actual performance will be compared with the maximum that is theoretically possible by experiment and by assumption.

	By experiment, Table XI, Example 3.	By assumption, Table XIII, Example 15.
Total heating effect of a lb. of combustible, thermal units	17,320	14,544
Per cent. of combustible in a lb. of coal, Table XXV, Experiment 9	82.7	82.7
Thermal units per lb. of coal, maximum heating effect	14,324	12,028
Temperature of fire-room	68°	68°
Temperature of escaping products of combustion, at least	600°	600°
Lbs. of air supplied per lb. of coal	19	25
Specific heat of products of combustion	0.237	0.238
Thermal units carried off in products of combustion	2,406	3,165
Maximum possible evaporation, thermal units	11,918	8,863
Evaporation by experiment, thermal units	10,636	10,636
Temperature of furnace	3832°	2440°

The reader will find the results of a number of boiler experiments in the following tables, experiments generally regarded as being among the most reliable that have ever been made, all of which must be repudiated by those who believe that the maximum heating effect attainable from the combustible in the best varieties of American anthracite is 14,544

thermal units per lb., and that the average amount of air supplied to furnaces consuming American anthracite is from 25 to 30 lbs. per lb. of coal.

Supposing the furnace of a boiler to be so arranged that the proper amount of air for economical evaporation is admitted, it will be found that the evaporation actually obtained depends upon the proportions of the boiler, and the character and disposition of the heating surfaces. The rate of transmission of heat through a metallic plate covered with water on one side, and exposed to the action of flame or heated gases on the other, has been found by experiment to depend upon the difference of temperature on the two sides of the plate, or rather upon the square of this difference, from which fact it can reasonably be inferred that the heating surfaces nearest to the fire are the most efficient, so that, other things being equal, the boiler which has the largest proportion of furnace heating-surface will be the most efficient and the most economical. This inference is fully sustained by experiment, as will be shown. To determine the effect of different arrangements of heating-surface and other details, it is useful to examine the results of experiments in which the same kind of coal was used and the same method of experimenting, with boilers of various design. Such a series of experiments is contained in the "Reports and Awards, group XX," of the United States International Exhibition, 1876. Philadelphia, 1878. In this report will be found full details of experiments made with fourteen different boilers, eight of which were of the sectional or water-tube variety. These experiments have sometimes been incorrectly reported in the trade circulars of boiler manufacturers, and have been accompanied by discussions of results that were generally in the interest of particular boilers. The report, however, is of great value to any one who examines it impartially, and the writer hopes that the following tables will prove useful both to boiler manufacturers and steam users. Nearly all the boilers tested at the Centennial Exhibition were of well-known forms, and designating letters have been applied to them, as follows:

Boilers Tested at Centennial Exhibition, 1876.

A.—*Wiegand*, water-tube boiler.

B.—*Harrison*, sectional boiler, cast-iron.

C.—*Firmenich*, water-tube boiler.

D.—*Rogers & Black*, vertical cylinder, suspended over grate, the cylinder having external water-tubes arranged vertically around it.

E.—*Andrews*, double-return tubular, with sheet-iron smoke-connections.

F.—*Root*, water-tube boiler.

G.—*Kelly*, water-tube boiler.

H.—*Exeter*, sectional boiler, cast-iron.

I.—*Lowe*, cylindrical tubular boiler, with combustion chamber in front-connection.

J.—*Babcock & Wilcox*, water-tube boiler.

K.—*Smith*, cylindrical tubular boiler, with generator attachment, consisting of water bridge-wall, and pipes at sides of furnace and under shell of boiler beyond bridge-wall.

L.—*Galloway*, flue boiler, furnaces in two flues, which unite beyond the grates in an elliptical flue which is crossed by conical water-tubes.

L'.—*Galloway*, same as preceding, tried with bituminous coal.

M.—*Anderson*, water-tube boiler.

N.—*Pierce*, tubular cylinder, revolving on trunnions over the grate.

An attempt is made, in the following discussion, to determine the relative commercial value of each boiler, and to find the reasons that influence this value.

Table XIV. shows the relative standing of the boilers in respect to their economical performance, and in Table XV. the relative consumption of fuel per unit of capacity or horse-power has been determined, on the assumption that a boiler horse-power is represented by the evaporation of 30 lbs. of water per hour, from and at 212°. The same unit of horse-power is used in all the boiler experiments that follow. The results given in Tables XIV. and XV. show that the effect of increasing the rate of combustion generally decreases the economical performance to a marked extent, so that a boiler which is quite economical when tested for economy may have its position materially changed when it is operated at full capacity. The

TABLE XIV.

CENTENNIAL BOILER TESTS.—POUNDS OF WATER EVAPORATED FROM AND AT 212°, PER POUND OF COMBUSTIBLE.

Relative Number.	Capacity Tests.		Economy Tests.		Average of Capacity and Economy Tests.	
	Boiler.	Evaporation.	Boiler.	Evaporation.	Boiler.	Evaporation.
1	K	11.925	L′	12.125	K	11.916
2	L′	11.609	F	12.094	L′	11.867
3	L	11.216	C	11.988	I	11 543
4	I	11.163	I	11.923	C	11.526
5	C	11.064	K	11.906	L	11.400
6	F	10.441	J	11.822	F	11.268
7	J	10.330	L	11.583	J	11.076
8	H	9.974	E	11.039	B	10.410
9	B	9.889	B	10.930	E	10 392
10	N	9.865	A	10.834	M	10.093
11	E	9.745	M	10.618	H	10.008
12	M	9.568	G	10.312	A	9.990
13	D	9.429	H	10.041	N	9.943
14	A	9.145	N	10.021	D	9.521
15	G	8.397	D	9.613	G	9.355

TABLE XV.

CENTENNIAL BOILER TESTS.—POUNDS OF COMBUSTIBLE PER HOUR, PER HORSE-POWER DEVELOPED.

Relative Number.	Capacity Tests.		Economy Tests.		Average of Capacity and Economy Tests.	
	Boiler.	Combustible	Boiler.	Combustible	Boiler.	Combustible
1	K	2.51	L′	2.47	K	2.52
2	L′	2.58	F	2.48	L′	2.53
3	I	2.67	C	2.50	I	2.60
4	L	2.67	I	2.52	C	2.61
5	C	2.71	K	2.52	L	2.63
6	F	2.87	J	2 54	F	2 68
7	J	2.90	L	2.59	J	2.72
8	H	3.01	E	2.72	B	2.89
9	N	3.04	B	2.74	E	2.90
10	B	3.04	A	2.77	M	2.98
11	E	3.08	M	2.83	H	3.01
12	M	3.13	G	2.91	N	3.02
13	D	3.18	N	2.99	A	3.03
14	A	3.28	H	3.00	D	3 15
15	G	3.57	D	3.12	G	3.24

economical performance of a boiler, generally speaking, depends upon the rate of combustion per unit of heating-surface, the economy ordinarily decreasing as the rate of combustion increases. It is important, therefore, in considering tests of boilers where the rates of combustion per unit of heating-surface vary, to reduce the results to some common standard before comparing them. This can be done approximately in a manner which will be explained. Prof. Rankine has deduced, from experiment, some formulas for determining the efficiency of a boiler at different rates of combustion. The results obtained from these formulas agree well with the actual performance of boilers of good design, are larger

TABLE XVI.

EFFICIENCY OF BOILERS, AT DIFFERENT RATES OF COMBUSTION,—CALCULATED FROM PROF. RANKINE'S FORMULAS.

Pounds of combustible per hour, per square foot of heating surface.	Evaporative efficiency.			
	Natural draft.		Forced draft.	
	Ordinary setting.	With fuel economiser.	Ordinary setting.	With fuel economiser.
5.	0.282	0.300	0.399	0.412
4.	0.330	0.350	0.456	0.468
3.	0.397	0.420	0 528	0.542
2.	0.503	0.525	0.632	0.644
1.	0.678	0.700	0.780	0.792
0.9	0.705	0.724	0.805	0.811
0.8	0.729	0.750	0.824	0.831
0.7	0.761	0.778	0.844	0.851
0.6	0.790	0.808	0.865	0.873
0.5	0.827	0.840	0.888	0.896
0.4	0.861	0.875	0.911	0.919
0.3	0.906	0.913	0.941	0.945
0.2	0.946	0.955	0.969	0.971
0.1	1.000	1.000	1.000	1.000

TABLE XVII.

CENTENNIAL BOILER TESTS.—RATIO OF ACTUAL AND THEORETICAL EFFICIENCY.

Relative Number.	Capacity Tests.		Economy Tests.		Average of Capacity and Economy Tests.	
	Boiler.	Ratio.	Boiler.	Ratio.	Boiler.	Ratio.
1	L′	1.013	L′	0.992	L′	1.003
2	K	0.982	L	0.976	L	0.975
3	L	0.974	F	0.953	K	0.964
4	N	0.953	K	0.946	N	0.928
5	I	0.914	J	0.941	I	0.915
6	D	0.889	C	0.918	J	0.908
7	J	0.875	I	0.916	F	0.906
8	C	0.870	A	0.910	C	0.894
9	B	0.858	N	0.902	B	0.874
10	F	0.858	G	0.897	E	0.865
11	E	0.841	B	0.890	D	0.860
12	M	0.832	E	0.889	A	0.858
13	A	0.806	M	0.864	G	0.849
14	G	0.801	D	0.830	M	0.848
15	H	0.795	H	0.765	H	0.780

than the actual results obtained from boilers whose design or proportions are faulty, and are too small for the most economical and efficient boilers, as will appear from the applications that follow. Prof. Rankine has tested his formulas by comparison with a number of experimental results, in his "Manual of the Steam-Engine and other Prime Movers," and the writer has applied these formulas to numerous boiler experiments, with very satisfactory results. Table XVI. is computed from the formulas referred to. In order to compare the results given in this table with those obtained experimentally, it is necessary to assume a figure for the

TABLE XVIII.

CENTENNIAL BOILER TESTS.—(EVAPORATION PER POUND OF COMBUSTIBLE) +(RECIPROCAL OF COMBUSTIBLE PER HORSE-POWER)+(RATIO OF EFFICIENCY).

Relative Number.	Capacity Tests.		Economy Tests.		Average of Capacity and Economy Tests.	
	Boiler.	Sum.	Boiler.	Sum.	Boiler.	Sum.
1	K	13.305	L′	13.522	K	13.277
2	L′	13.010	F	13.450	L′	13.265
3	L	12.565	C	13.306	I	12.843
4	I	12.452	K	13.249	C	12.803
5	C	12.303	I	13.236	L	12.755
6	F	11.647	J	13.157	F	12.547
7	J	11.550	L	12.945	J	11.352
8	N	11.147	E	12.296	B	11.630
9	H	11.101	B	12.185	E	11.602
10	B	11.076	M	11.835	M	11.277
11	E	10.911	G	11.553	N	11.202
12	M	10.719	N	11.257	A	11.178
13	D	10.632	H	11.139	H	11.120
14	A	10.256	A	11.105	D	10.698
15	G	9.478	D	10.764	G	10.513

evaporation corresponding to an efficiency of unity, and in order to conform to the principles on which the table depends, the writer has assumed that the evaporation corresponding to this efficiency is equal to 13.5 lbs. of water from and at 212° per lb. of combustible. This figure, like the several efficiencies in the table, will be found to agree well with a good average performance, to be too large for an inferior performance, and smaller than the best actual results. Applying these figures to the experiments under consideration, results are obtained as shown in Table XVII., which table probably represents the true economical values of the different boilers under the different conditions of trial.

By adding the several figures of Table XIV. to the corresponding figures of Table XVII., and to the reciprocals of the corresponding figures in Table XV., the results shown in Table XVIII. are obtained; and these figures seem to represent the relative commercial value of each boiler to the steam user, with the exception of the comparatively unimportant question of first cost, and other questions, by no means unimportant, of relative safety and durability. The last point is scarcely suitable for consideration in the present discussion, but it may be well to remark that any boiler of good design and material, substantially constructed, is quite as safe, under intelligent management (judging from experience and from the premiums demanded by boiler insurance companies), as the best forms of sectional, or, as they are sometimes called, safety boilers; so that a safety boiler managed by an attendant who can be hired cheaply because he is incompetent, will frequently cause a loss to the steam user that would more than pay for the services of a reliable attendant for a more economical boiler which would be quite as safe as the other in all things except the name.

As this view of the matter is not accepted by all engineers, it may be well to cite the opinions of a very high authority on the subject of boiler explosions. The following extract is from a letter addressed to Mr. Charles T. Porter by Mr. L. E. Fletcher, Chief Engineer of the Manchester Steam User's Association. This letter is published in the "Reports and Awards, Group XX." of the United States International Exhibition, Philadelphia, 1878.

"No. 1. *Term of Existence.* — The Association was established in the year 1854, and has been in active work ever since, increasing in the number of boilers and the area of its operations.

"No. 2. *Average and Present Number of Boilers in charge.*—The number of boilers now under inspection is, as

nearly as may be, 3,000. The average for the last five years has been 2,500.

No. 3. *Character of the Boilers, and if of Different Types, the Number of Each.*—By far the greater number of boilers enrolled with the Association are horizontal and internally fired. Speaking approximately, the relative number of the various types is as follows:

"50 per cent. are what are termed 'Lancashire' boilers—that is to say, having two internal tubes running through them from end to end in which the fires are placed.

"15 per cent. are of the 'Cornish' type—that is to say, having one furnace tube running through it from end to end, in which the fire is placed.

"15 per cent. are externally fired, such as plain, cylindrical, egg-ended, colliery boilers; French or 'Elephant' and 'Butterly' boilers.

"8 per cent. are variations of the 'Lancashire' and 'Cornish' boiler, with a number of small flue tubes, some termed 'Multiflued' and others 'Multitubular,' etc.

"6 per cent. are of the 'Galloway' type.

"6 per cent. are of the miscellaneous types, such as boilers at iron-works, heated by flames passing off from puddling and iron furnaces, water-pipe boilers, locomotive and marine boilers, and vertical internally hand-fired boilers, etc.

"These proportions vary somewhat year by year as boilers are changed.

"No. 4. *Pressure carried between what Limits.* — All the 'Lancashire' boilers made for the members under the inspection of the Association, the ruling diameter of which is 7 feet in the shell, and 2 feet 9 inches in the furnace tubes, are fit for a working pressure, as a minimum of 75 pounds on the square inch. Many are fit for a working pressure of 85 pounds, others 90 pounds and 100 pounds. No new boilers are made to the Association's standard for a lower pressure than 75 pounds on the square inch. Many smaller boilers are carrying 120 pounds.

"No. 5. *Character of the examinations made, and their frequency.*—A complete examination of each boiler is made both inside and outside, when at rest and properly prepared, at least once a year, and more often if necessary—that is to say, if the boiler does not appear thoroughly sound or repairs have to be examined. Hydraulic tests are also had recourse to when necessary. In addition to the annual thorough examination, two external examinations of each boiler are made per annum with the boilers at work and steam up. This number is a minimum.

*   *   *   *   *

"No. 7. *Instructions given to Owners and Firemen.*—We have no written code, but are thinking of preparing a list of instructions to firemen. All we ask from the owners is to get a good boiler and a careful man. We impose no arbitrary conditions. Information to the owners is always accessible at these offices.

"No. 8. *The Guarantee afforded to Members.*— The Association guarantees the members freedom from explosion year after year. As a pledge of good faith, the reports are endorsed with a pecuniary guarantee of £300; but the Association has no explosions. The only exception to this was the rending of a furnace through over-heating, in consequence of misuse by the owner, who charged the boiler heavily with caustic soda and arsenic, bringing down the incrustation, but yet neglecting to blow out. We warn our members against using compositions and neglecting to blow out. Year after year we are able to report, '*No explosion from any boiler guaranteed by the Association.*'

"No. 9. *The cost to Members of the Inspection and Guarantee.*—The charge for inspection is one guinea and a half per annum each boiler, within a radius of 40 miles of Manchester; beyond that distance, according to arrangemeut. There is no charge for guarantee. The Association's guarantee is neither to be bought nor sold. If the Association considers a boiler unsafe, nothing will induce it to say it is safe. If a boiler is safe, there is no need to charge for saying so. The expense is incurred in inspection, and the Association has no explosions to make compensation for.

"No. 10. *The Result of the Work of the Association in Immunity from Accidents.*—It is presumed by the word '*accidents*' is meant '*explosions.*' We do not approve of the word '*accidents*' as applied to explosions. Explosions in the great majority of cases are not accidental: they arise from known

causes. Inspection is able to prevent their occurrence, and is found in the experience of the Association to be quite adequate. *See here reply to question No.* 8.

"11. *Upon what do you rely for Safety?*—Upon competent periodical inspection. *See reply to questions* 8 and 10.

With regard to the relative first costs of the boilers tested at the Centennial Exhibition, they can only be inferred from the relative amounts of heating-surface in respect to capacity. If all the boilers were alike in design and made of the same materials, differing only in their proportions, a comparison of this kind would give a close approximation to the relative costs. In the actual case, where one boiler is composed principally of cast-iron, another of wrought-iron, and a third of steel, the comparison, as made in Table XIX., will merely afford an approximate idea of the selling prices.

In the use of steam boilers the demands for steam frequently vary to a considerable extent, so that the boilers are run at times with slow rates of combustion, and at other times are forced to their utmost capacity. Under such circumstances the most economical boiler for the steam user is one that has its evaporative capacity changed the least by a change in the rate of combustion, or, in other words, the boiler which shows the best ratio of evaporation at high and low rates of combustion. The final comparative table of the Centennial boiler tests, Table XX., shows this ratio, as also the theoretical ratio, calculated by Table XVI., together with the actual rates of combustion, and some of the most important proportions of the boilers.

It would, of course, be possible to extend these comparative tables much farther, but it is thought that the most important points determined by the tests have been presented. The tables are so fully detailed that a number of interesting conclusions can be drawn from them. It may be interesting to state briefly several of the most useful. It is to be noticed that the sectional or water-tube boilers represented in these tests were generally less economical than the others, except at slow rates of combustion. Considering only the tests with anthracite coal, it appears that boiler F, the most economical of the sectional boilers in this collection, which was the most economical of all the boilers at a slow rate of combustion, occupies the 6th place under a high rate of combustion, and the same average place. The great loss of economy that occurs in the case of most sectional boilers, when their rate of combustion is fixed at the limits that obtain in ordinary practice, has long been recognized by experienced engineers, and although at first thought it seem unreasonable that boilers whose heating-surface is composed of thin tubes should be less economical than shell boilers composed of thick metal, there is a very good reason for this difference, and an attempt will be made to point it out. Referring to Table XX., it will be seen that the difference in actual evaporation at high and low rates of combustion for the first seven boilers in the table is either practically the same as, or considerably less than, the theoretical difference; and a reference to the designs of these seven boilers shows that each of them has a large amount of furnace heating-surface, all the heating-surface, in fact, of boilers D and N being in the furnace Now, it has already been stated that the transmission of heat through the heating-surface varies as a function of the square of the difference in temperature of the two sides of this surface; and some experiments will be presented, before concluding this paper, to show that in a boiler with good furnace heating-surface, this surface evaporates the greater part of all the water that is made into steam by the boiler. It is probably true that with any design of boiler the evaporative results can be made to equal the best that have been recorded, if the heating-surface is sufficiently extended. But heating-surface costs money, and requires considerable space, so that, if the same results can be obtained with a less amount of heating-surface, properly distributed, the latter design is to be preferred. It seems to be the neglect of the principles here stated which causes most water-tube boilers to give such inferior results at high rates of combustion.

The experiments detailed in Tables XXI., XXII. and XXIII., while of considerable interest, are not as useful for purposes of comparison as those that have just been considered; being made generally, in different localities, with different kinds of coal, and under different

Table XIX.

Centennial Boiler Tests.—Square Feet of Heating-Surface per Horse-Power Developed.

Relative Number.	Capacity Tests.		Economy Tests.		Average of Capacity and Economy Tests.	
	Boiler.	Square Feet.	Boiler.	Square Feet.	Boiler.	Square Feet.
1	N	4.48	N	5.87	N	5.18
2	D	5.17	G	6.83	G	6.18
3	G	5.53	L	7.18	L	6 36
4	L	5.54	D	7.61	D	6.39
5	L′	5.93	A	7.90	L′	7.05
6	A	7.15	L′	8.16	A	7.53
7	B	7.25	B	9.50	B	8.38
8	M	7.30	M	9.89	M	8.60
9	E	7.50	E	10.36	E	8.93
10	J	7.81	J	10.75	J	9.28
11	K	8.03	K	10.78	K	9.41
12	I	9.24	F	11.60	F	10.48
13	F	9.35	I	13.59	I	11.42
14	H	12.60	C	15.53	C	14.33
15	C	13.12	H	18.85	H	15.73

Table XX.

Centennial Boiler Tests.—Difference of Evaporative Efficiency, Capacity and Economy Tests.

Relative Number.	Boiler	Per cent of difference in evaporation, capacity and economy tests.		Pounds of combustible per square foot of heating surface per hour.		Temperature of flue.		Ratio of	
		Actual.	Theoretical.	Capacity tests.	Economy tests.	Capacity tests.	Economy tests.	Heating to grate-surface.	Draft area to grate-surface.
1	K	0.2	3.6	0.313	0.234	435°.06	411°.48	46.16	0.130
2	H	0.3	4.1	0.239	0.159	438°.12	429°.94	52.1	0.143
3	N	1.6	7.4	0.679	0.509	455°.62	373°.82	13.97	0.046
4	D	2.0	9.2	0.615	0.410	649°.3	571°.75	19.04	——
5	L	3 3	3.1	0.482	0.361	322°.18	303°.	23.68	0.223
6	L′	4.5	6.6	0.436	0.303	383°.	324°.62	23.68	0.223
7	I	6.2	5.9	0.289	0.185	359°.71	332°.29	33.5	0 088
8	C	8.4	2.5	0.207	0.161	418°.	415°.5	66.91	0.130
9	B	10.5	6.6	0.420	0.288	584°.13	517°.5	39.17	0.285
10	M	11.1	7.1	0.429	0.286	534°.	417°.	30.97	0.300
11	E	13.3	7.2	0.410	0.263	382°.63	419°.6	27.49	0.102
12	J	14.4	6.5	0.372	0.236	472°.81	295°.82	37.67	0.202
13	F	15.9	4.2	0.307	0.214	——	393°.33	38 06	0.120
14	A	18.5	5.0	0.458	0.351	604°.62	523°.81	31.89	0.119
15	G	22.8	9.7	0.646	0.426	——	——	23.11	0 148

conditions. Some notes are added to show the relative value of the different experiments in respect to precision and other necessary items.

Table XXI. contains results of experiments made with a water-tube boiler, which, while it was not the most economical boiler of that type tested at the Centennial Exhibition, is very largely in use, so that records of its performance are

Table XXI.

Tests of Babcock & Wilcox Water-Tube Boilers, at Different Rates of Combustion.

Item.	Natural Draft.								Forced Draft.
	1	2	3	4	5	6	7	8	9
Heating surface, sq. ft.....	2807.16	1558	1676.32	1558	2122	4080	1592	1676.32	2807.16
Grate surface, "	70	30	44.5	30	50.75	103	46	44.5	70
Ratio of heating to grate surface..................	40.1	51.9	37.67	51.9	41.81	39.61	34.6	37.67	40.1
Temperature of feed water.	153°	204°.8	63°.98	205°	165°.8	110°59	150°	57°.6	160°
Temperature of flue.......	—	—	—	—	326°	453°23	467°	472°.81	—
Pressure of steam, lbs. per sq. in. above atmosphere.	75	76.3	70	81.5	62.5	71.63	70	70	85
Per cent. of refuse........	25.5	17.12	10.997	16.18	13.708	14.94	21.43	7.842	26.
Lbs. of combustible per hour. Total........	617	367	396	407	568	1301	563	623	1247
Lbs. of combustible per hour. ℔ □′ grate*...	8.81	12.23	8.89	13.58	11.22	12.63	12.24	14.00	17.82
Lbs. of combustible per hour. ℔ □′ heating*.	0.220	0.235	0.236	0.261	0.268	0.319	0.354	0.372	0 444
Lbs. of water evaporated per hour, from and at 212°. Total........	6725	3974	4678	4519	6591	14583	6339	6436	11294
Lbs. of water evaporated per hour, from and at 212°. ℔ □′ grate*...	96.07	132.15	105.12	150.63	129.86	141.58	137.81	144.64	161.34
Lbs. of water evaporated per hour, from and at 212°. ℔ □′ heating*	2.40	2.54	2.79	2.90	3.11	3.57	3.98	3.84	4.02
H. P. developed, at 30 lbs. per hour, from and at 212°	224.2	132.2	155.9	150.6	219.7	486.1	211.3	214.5	376.5
Lbs. of coal ℔ hour per H.P.	3.69	3.35	2.85	3.23	3.00	3.15	3.39	3.15	4.48
" combustible " "	2.75	2.78	2.54	2.70	2 59	2.68	2.67	2.90	3.31
□′ of grate surface " "	0.312	0.227	0.285	0.199	0.231	0.212	0.217	0.207	0.186
" heating " " "	12.52	11.79	10 75	10.34	9.66	8.39	7.53	7.81	7.46
Lbs. of water evaporated from and at 212° ℔ lb. of coal.	8.13	8.96	10.52	9.30	9.99	9.54	8.84	9.52	6.70
Lbs. of water evaporated from and at 212° " combust'e	10.91	10.81	11.82	11.10	11.58	11.21	11.25	10.33	9.05
Actual efficiency	0.808	0.801	0.876	0.822	0.858	0.830	0.833	0.765	0.671
Theoretical efficiency......	0.938	0.932	0.931	0.921	0.918	0.906	0.882	0.874	0.901
Ratio of actual and theoretical efficiency............	0.861	0.859	0.941	0.893	0.934	0.917	0.945	0.876	0.744

* Surface.

more available than those of most other varieties.

Experiments 1 and 9 were made with the same boiler, as were experiments 2 and 4, but in these latter experiments the quality of the steam was not tested, and the feed water was measured with a meter. Experiments 3 and 8 are the Centennial tests. In experiment 5, the quality of the steam was not tested. Table XXII. is intended to show the value of furnace heating-surface in improving the performance of wasteful boilers, and in rendering boilers economical when forced to their utmost capacity. The heating-surface known as a generator attachment, the effect of which has already been illustrated in the Centennial tests, is placed close to the hottest fire and the most highly heated gases of the furnace. The idea of such an attachment is very old, but it is only within a comparatively short time that the manufacturers of these devices have succeeded in rendering them durable. It needs no argument to demonstrate the utility of such an attachment, if experience proves that it does not burn out. The table also shows the economy of extending the heating-surface, or what is the same thing, running a boiler at about half capacity. This is not the usual practice in large establishments, nor is it generally desirable; for the experiments show that the six tubular boilers, experiments 4, 5, 6, could readily be made to do all the work of the 16 cylinder boilers in addition to their reported performance, and with more economy than is obtained by the combination of cylinder and tubular boilers. Experiments 1, 2, 3, Table XXII., were made with the same quality of coal. In experiments 2 and 3 the boilers were forced to their utmost capacity with natural draft, a special effort being made to burn as much coal as pos-

TABLE XXII.

TESTS OF WATER-TUBE, FIRE-TUBE, AND CYLINDER BOILERS.

Item.	1	2	3	4	5	6	7	8	9
	Stead's water-pipe boiler, with water bridge-wall, and coils on sides of furnace.	Cylindrical tubular boilers, with Stead's generator attachment, consisting of water bridge-wall, and coils in furnace and connection chamber.		6 cylindrical tubular, and 16 cylinder boilers. Tubular, 6 feet by 15, 100 tubes, 3 inch. Cylinder boiler, 30 in. by 30 ft.			Three cylinder boilers, 30 inches by 30 feet, with and without Stead's generator attachment.		
		6 feet diameter, 18 feet long, 74 tubes, 4 inches.	6 feet diameter, 21 feet long, 35 tubes, 5 inches.	One tubular boiler.	One cylinder boiler.	Averages and totals for all the boilers.	As originally set.	With water bridge-wall and coils under boilers.	With water bridge-wall, coils under boilers, and in furnace.
Heating surface, boiler...sq. ft.	—	1516.4	1202.6	—	—	—	—	353.4	353.4
" generator attachm't "	—	259.3	213.7	—	—	—	—	232.8	353.1
Heating surface, total....sq. ft.	1989.3	1775.7	1416.3	1319.5	117.8	9801.6	353.4	586.2	706.5
Grate surface..................	33	33	39	27.75	11	342.5	32	32	32
Ratio of heating to grate surface	60.3	53.8	36.3	47.5	10.7	28.6	11	18.3	22.1
" tube vent "	—	0.170	0.108	0.149	—	—	—	—	—
Temperature of feed water.....	37°	37°	37°	107°.7	152°.7	130°.2	150°	150°	150°
" flue...........	621°	433°	564°	319°	494°	406° 3	—	—	—
Pressure of steam, lbs. per sq. in. above atmosphere........	67	80	61	70.5	69.4	69.9	88	90	90
Per cent. of refuse...........	15.6	14.4	13.4	13.3	14.6	14.2	14.6	14.6	14.6
Lbs. of combustible per hour. Total............	403	534	555	179	102	2697	176	182	192
℔ □′ grate surface	12.1	16.1	14.2	6.43	9.24	7.88	5.5	5.67	6.
" heating "	0.204	0.301	0.392	0.135	0.863	0.275	0.498	0.310	0.272
Lbs. of water evaporated per hour, from and at 212°. Total............	5337	6187	5332	2370	885	28888	1609	2118	2338
℔ □′ grate surface	162.	187.5	136.7	85.4	80.4	84.3	50.3	66.2	73.1
" heating "	2.69	3.86	3.76	1.8	7.51	2.95	4.55	3.61	3.31
H. P. developed, at 30 lbs. per hour, from and at 212°......	177.9	206.2	177.7	79.	29.5	962.9	53.6	70.6	77.9
Lbs. of coal per hour per H. P.	2.68	3.03	3.61	2.61	4.04	3.26	3.84	3.02	2.89
" combustible " "	2.27	2.59	3.12	2.27	3 44	2.8	3 29	2.57	2.47
□′ of grate surface " "	0.186	0.160	0.219	0.351	0.373	0.356	0.597	0.453	0.411
" heating " " "	11.18	8.61	7.97	16.7	3.99	10.18	6.6	8.3	9.07
Lbs. of water evaporated from and at 212° Per lb. of coal...	11.16	9.92	8.32	11.5	7.38	9.19	7.8	9.97	10.39
" combustible	13.31	11.59	9.6	13.29	8.64	10.71	9.13	11.67	12.17
Actual efficiency..............	0.986	0.859	0.711	0.984	0.640	0.793	0.676	0.864	0.901
Theoretical efficiency..........	0.944	0.901	0.865	0.985	0.714	0.915	0.828	0.901	0.916
Ratio of actual and theoretical efficiency..................	1.010	0 953	0.822	0.999	0.896	0.867	0.816	0.959	0.984

sible. Boiler, experiment 2, is arched, so that the products of combustion return over the top of the boiler. Boiler, experiment 3, is not arched, and the results show the inferiority of this mode of setting when a boiler is forced. Tubular boilers, experiments 4, 6, are arched, and have fuel economizers in the flues, repre-

Table XXIII.

Tests of Cylindrical Tubular Boilers, at Different Rates of Combustion.

Item.	1	2	3	4	5	6	7
	Four boilers, each 4 feet diameter, 14 feet long, with 60 tubes, 3 inches.	Three boilers, each 5 ft. diameter, 16 feet long, with 80 tubes, 3 inches.	One boiler, 4 feet diameter, 14 feet long, with 49 tubes, 3 inches.	One boiler, 4 feet diameter, 15 feet long, with 36 tubes, 4 inches.	Four boilers each 4 feet diameter, 14 feet long, with 60 tubes, 3 inches.	One boiler, 4 feet diameter, 15 feet long, with 36 tubes, 4 inches.	Three boilers, each 3.33 ft. diameter, 17 ft. long, with 19 tubes, 4 inches.
Heating surface.... ....sq. ft.	2990.8	3372.4	626.74	688.72	2990.8	688.72	1284
Grate " .... "	64	75	18	14	64	14	58.62
Ratio of heating to grate surface............	46.73	45.	34.82	49.19	46.73	49.19	21.9
" tube vent "	0.155	0.135	0.112	0.193	0.155	0.193	0.074
Height of boiler above grate............inch	18.5	20.5	23	28	18.5	28	—
Temperature of feed water..................	153°87	109°	157°91	52°.59	161°55	71°.05	150°
" flue.........................	371°73	476°	292°94	331°14	387°72	373°18	543°
Pressure of steam, lbs. ⅌□″ above atmosphere	98.86	41.3	67.42	70.38	87.37	70.67	70
Per cent. of refuse..........	12.07	17.31	3.8	7.24	8.49	6.54	20.84
Lbs. of combustible per hour. Total........................	282	519	120	138	635	259	640
Lbs. of combustible per hour. Per sq. ft. of grate surface.....	4.41	6.91	6.68	9.84	9.90	18.49	10 92
Lbs. of combustible per hour. " " heating "	0.094	0.154	0.192	0.200	0.212	0.376	0.499
Lbs. of water evaporated per hour, from and at 212°. Total......	2970	6699	1154	1471	5796	2385	6767
Lbs. of water evaporated per hour, from and at 212°. Per sq. ft. of grate surface.	46.41	89.32	64.12	105.06	90.56	170.36	115.43
Lbs. of water evaporated per hour, from and at 212°. " " heating "	0.99	1.98	1.84	2.13	1.94	3.46	5.27
Horse power developed, at 30 lbs. per hour, from and at 212°........................	99.	223.3	38.5	49.	193.2	79.5	225.6
Lbs. of coal per hour per horse power........	3.24	2.80	3.25	3.03	3.59	3.49	3.59
" combustible " "	2.85	2.32	3.13	2.81	3.16	3.18	2.84
Sq. ft. of grate surface "	0.646	0.336	0.468	0.286	0.331	0.176	0.260
" heating " "	30.21	15.1	16.29	14.05	15.48	8.66	5.69
Lbs. of water evaporated from and at 212° Per lb. of coal....	9.25	10.70	9.23	9.90	8.35	8.61	8.36
Lbs. of water evaporated from and at 212° " combustible	10.52	12.92	9.60	10.68	9.13	9.21	10.57
Actual efficiency............................	0.777	0.957	0.711	0.791	0.676	0.682	0.783
Theoretical efficiency.......................	1.003	0.971	0.950	0.946	0.941	0.872	0.827
Ratio of actual and theoretical efficiency.....	0 775	0.985	0.748	0.836	0.719	0.782	0.946

senting, both in theory and practice, the most economical mode of setting a plain, cylindrical tubular boiler. In experiments 7, 8, 9 the quality of the steam was not tested, and the weight of ashes, not being reported, was assumed as equivalent to the refuse in experiment 5.

Table XXIII. has been prepared to show the performance of cylindrical tubular boilers in practice which represents the average, and which is generally bad.*

Experiments 1 and 5 were made with the same boilers, not arched.

Boilers, experiment 2, were not arched, and the results of this experiment were obtained after resetting the boilers, their original evaporation of water from and at 212° having been 10.01 lbs. per lb. of combustible. The experiment shows that the setting without arch may answer very well for slow combustion.

Experiments 4 and 6 were made with the same boiler, not arched, but having the Jarvis air-setting. Experiment 7, Table XXIII., gives the results of what is called in the report a comparative test with the boilers, experiment 7, Table XXI. ; but an examination of the rates of combustion in the two sets of experiments, renders it probable that it would be more accurate, if real comparative results are desired, to compare experiment

* This is exemplified by the common practice of having boilers set by masons who may be good bricklayers, but are not familiar with the laws of combustion ; and also by the practice of many boiler makers who set boilers by a fixed rule, with the same gratebars and flue proportions, regardless of the character of the fuel which is to be used.

Table XXIV.

Tests of Horizontal Fire-Tube Marine Boiler, at Different Rates of Combustion. Evaporation at Atmospheric Pressure.

Item.	1 Boiler as built.	2 Grate surface reduced.	3 Eight upper rows of tubes plugged.	4 Ferrules in tubes.	5 Grate surface reduced. Ferrules in tubes. Forced draft.	6 Boiler as built.	7 Upper row of tubes plugged.	8 Six upper rows of tubes plugged.	9 Four upper rows of tubes plugged.
Heating surface..........sq. ft.	949.93	949.93	326.01	949.93	949.93	949.93	860.04	481.99	637.97
Grate " .... "	36	18	36	36	27	36	36	36	36
Ratio of heating to grate surface	26.4	52.8	9.1	26.4	35.2	26.4	23.9	13.4	17.7
" draft area "	0.128	0.256	0.014	0.100	0.148	0.128	0.114	0.043	0.71
Temperature of feed water.....	62°.1	50°.2	51°.12	74°.08	72°.25	77°.74	72°.17	48°	56°
" fire-room......	77°.3	65°	55° 04	78°.47	74°.2	94°	93°	48°.45	57°.69
" flue.....	371°	399°	383°	477°	—	600°+	600°+	600°+	600°+
Per cent. of refuse............	13.5	20.3	21.3	13.6	22.4	20.3	20.8	21.4	21.
Lbs. of combustible per hour: Total...........	135	152	88	370	513	549	546	310	417
Lbs. of combustible per hour: ℔□′ grate surface	3.75	8.43	2.43	10.26	19.00	15.24	15.16	8.62	11.57
Lbs. of combustible per hour: " heating "	0.142	0.160	0.267	0.389	0.540	0.577	0.634	0.643	0 654
Lbs. of water evaporated per hour, from and at 212°: Total...........	1766	1868	1197	4370	6628	6681	6579	3807	5325
Lbs. of water evaporated per hour, from and at 212°: ℔□′ grate surface	48.8	103.7	33.1	121.5	245.1	185.4	182.7	106.	147.2
Lbs. of water evaporated per hour, from and at 212°: " heating "	1.85	1.96	3.63	4.60	6.96	7.02	7.64	7.91	8.32
H. P. developed, at 30 lbs. per hour, from and at 212°.......	58.9	62.2	39.9	145.7	220.9	222.7	219.3	126.9	177.5
Lbs. of coal per hour per H. P.	2.71	3.06	2.80	2.94	2.92	3.09	3.14	3.11	2.99
" combustible " "	2.35	2.44	2.21	2.54	2.27	2.47	2.49	2.44	2.35
□′ of grate surface "	0.612	0.289	0.902	0.247	0 122	0.162	0.164	0.284	0.203
" heating " "	16.13	15.26	8.17	6.52	4 30	4.27	3.92	3.80	3.59
Lbs. of water evaporated from and at 212°: Per lb. of coal...	11.31	9.80	10.70	10.20	11.03	9.70	9.54	9 65	10.09
Lbs. of water evaporated from and at 212°: " combustible	13.08	12.29	13.60	11.81	12.92	12.17	12.05	12.28	12.77
Actual efficiency..............	0.969	0.910	1.005	0.875	0.957	0.901	0.893	0.910	0 946
Theoretical efficiency	0.977	0.968	0.919	0.866	0.812	0.799	0.780	0.778	0.774
Ratio of actual and theoretical efficiency....................	0.992	0.940	1.099	1.012	1.179	1.134	1.144	1.169	1.222

7, Table XXIII., with experiment 9, Table XXI.

In all the experiments of Tables XXI., XXII., XXIII., where the exceptions have not been noted, the quality of the steam was tested, and the proper corrections have been applied.

Among the boilers that have been noticed in the preceding tables, the only one of the sectional variety which seems to have been designed with any very pronounced idea of the importance of the disposition of the heating-surface is that of experiment 1, Table XXII., and the results of this disposition seem to be very favorable. This boiler differs from most others of the water-tube variety in being composed of horizontal tubes arranged in sections connected at the ends by castings which have the general form of return-bends, and which are provided with hand-holes so as to give access to the interior of the tubes. The boiler is of quite recent design, and, not having come into very extensive use as yet, its true commercial value can only be settled by further experience. It is noticed in this connection, on account of the fact that it seems to possess many characteristics of — together with improvements upon—the only sectional boiler that has has ever been successfully employed for marine purposes, so far as the writer is aware. Reference is made to the Belleville boiler, which is manufactured in France, and which is largely used on

Table XXV.

Tests of Vertical Water-Tube Marine Boiler, at Different Rates of Combustion. Evaporation at Atmospheric Pressure.

Item.	Grate surface and draft area reduced.	Grate surface reduced.	Boiler as built.	Grate surface reduced.	Bars between tubes.			Bars between tubes. Forced draft.	
	1	2	3	4	5	6	7	8	9
Heating surface......sq. ft.	1264.81	1264.81	1264.81	1264.81	1264.81	1264.81	1264.81	1264 81	1264.81
Grate " ...... "	18	18	39	24	39	39	39	39	39
Ratio of htg. to grate surface	70.3	70.3	32.4	52.7	32.4	32.4	32.4	32.4	32.4
" draft area " "	0.111	0.308	0.142	0.231	0.100	0.111	0.100	0.111	0.100
Temperature of feed water.	40°	49°.7	58°	48°	75°	72°.17	38°	77°.74	58°
" " fire room..	59°.66	57°	68°.1	57°.8	91°	93°	39°.7	94°	67°.7
" " flue.......	316°	264°	299°	287°	322°	361°	352°	440°	600°+
Per cent. of refuse........	19.5	25.3	20.4	23.9	20.8	23.8	18.6	20.9	17.3
Lbs. of combustible per hour. Total........	180	228	254	292	304	337	369	545	736
Lbs. of combustible per hour. ꝑ □′ grate*...	9.96	12.66	6.51	12.17	7.78	8.84	9.47	13.97	18.87
Lbs. of combustible per hour. " heating*	0.142	0.180	0.201	0.231	0.240	0.266	0.292	0.431	0.582
Lbs. of water evaporated per hour, from and at 212°. Total	2396	3028	3442	3957	4517	4738	4908	7298	9804
Lbs. of water evaporated per hour, from and at 212°. ꝑ □′ grate*...	132.5	168.9	87.9	164.7	115.9	121.5	126.	187.6	251.4
Lbs. of water evaporated per hour, from and at 212°. " heating*	1.88	2.40	2.71	3.13	3.58	3.75	3.89	5.79	7.75
H. P. developed, at 30 lbs. per hour, from and at 212°	79.9	100.9	114.7	131.9	150.6	157.9	163·6	243.3	326.8
Lbs. of coal ꝑ hour, ꝑ H.P.	2.80	3.02	2.78	2.91	2.55	2.80	2.77	2.83	2.72
" combustible " "	2.25	2.26	2.21	2.21	2.01	2 13	2 26	2.24	2.25
□′ grate surface "	0.225	0.178	0.340	0.182	0.259	0.247	0.238	0.160	0.119
" heating " "	15.84	12.53	11.02	9.59	8.40	8.01	7.73	5.20	3.87
Lbs. of water evaporated from and at 212° ꝑ lb. of coal.	10.72	9.94	10.79	10.31	11.77	10.71	10.83	10.59	11.02
Lbs. of water evaporated from and at 212° " combusti'e	13.31	13.28	13.55	13.55	14.86	14.06	13.30	13.39	13.32
Actual efficiency..........	0.986	0.984	1.001	1.001	1.101	1.042	0.985	0.992	0.987
Theoretical efficiency......	0.977	0.957	0.946	0.933	0.930	0.919	0.908	0.850	0.797
Ratio of actual and theoretical efficiency.........	1.009	1.028	1.061	1.101	1.184	1.133	1.085	1.167	1.238

* Surface.

land, and has been employed successfully for several years, according to official reports, in vessels belonging to the French Navy.

In the experiments detailed in Tables XXI., XXII., XXIII. it will be observed that the actual and theoretical efficiency, together with the ratio of these efficiencies, have been given for each experiment, calculated from the data of Table XVI., on the assumption that the evaporation corresponding to an efficiency of unity is 13.5 lbs. of water from and at 212° per lb. of combustible. With ratios so obtained, the effects of the different rates of combustion are equalized, so that the different economical results are strictly comparable, except for the differences in size or quality of fuel. The reader will doubtless understand that the rate of combustion spoken of is referred to the unit of heating-surface. Experiments with boilers are sometimes compared with respect to the rate of combustion per unit of grate-surface or of time; but if the ratios of heating to grate-surface differ in the several boilers, such a comparison determines absolute rather than relative economic values.

The preceding tables cover a wide range of boiler design and management, and are worthy of careful examination by all those who are interested in the use of, or manufacture of, steam generators. The present discussion is far from exhaustive, but it is hoped that it will be suggestive to the reader. In this connection it seems proper to present some

TABLE XXVI.

EXPERIMENTS ILLUSTRATING EFFICENCY OF FURNACE HEATING-SURFACE.

Item.	1	2	2	3	5	6
	Horizontal fire-tube marine boiler.		Vertical water-tube marine boiler.		Horizontal fire-tube marine boiler, Brooklyn Navy Yard.	
	Boiler as built.	Tubes closed	Boiler as built.	Tubes as closed	Boiler as built.	Tubes closed
Heating surface.....sq. ft.	949.93	213.23	1264.81	180.73	150.3	45.5
Grate " ..... "	36	36	39	39	10.8	10.8
Ratio of heating to grate surface.....	26.4	5 92	32.4	4.63	13.9	4.21
Ratio of draft area to grate surface.....	0.128	0.067	0.142	0.068	0 092	0.023
Temperature of feed-water.....	58°	67°.4	58°	67°.4	100°	100°
Temperature of fire-room.....	68°.1	70°.9	68°.1	70°.9	—	—
Temperature of flue.....	510°	—	299°	—	—	—
Pressure of steam, lbs. per sq. in. above atmosphere.	0.	0.	0.	0.	20.	20.
Per cent. of refuse.....	19.6	17.	20.4	16.3	14.7	27.2
Lbs. of combustible per hour. Total.....	256	259	254	261	101	93
Lbs. of combustible per hour. Per sq. ft. of grate surface.....	7.12	7.19	6.50	6.70	9 38	8.57
Lbs. of combustible per hour. Per sq. ft. of heating surface.....	0.270	1.215	0.201	1.45	0.674	2 035
Lbs. of water evaporated per hour, from and at 212°. Total.....	3254	2133	3442	2123	919	731
Lbs. of water evaporated per hour, from and at 212°. Per sq. ft. of grate surface...	90.42	59.3	87.89	54.54	85.2	67.8
Lbs. of water evaporated per hour, from and at 212°. Per sq. ft. of heating surface.	3.43	10.01	2.71	11.80	6.12	16.08
H. P. developed, at 30 lbs. ℔ hour, from and at 212°	108.5	71.1	114.7	70.8	30.6	24.4
Lbs. of coal per hour, per horse-power.....	2.94	4.39	2.78	4.41	3.86	5.24
Lbs. of combustible per hour, per horse-power.....	2.36	3.64	2.21	3.69	3.30	3.82
Sq. ft. of grate surface "	0.332	0.506	0.340	0.551	0.352	0.443
Sq. ft. of heating surface "	8.76	3.00	11.02	2.55	4.91	1.87
Lbs. of water evaporated from and at 212°. Per lb. of coal.....	10.22	6.84	10.79	6.81	7.75	5.75
Lbs. of water evaporated from and at 212°. Per lb. of combustible.....	12.71	8.24	13.55	8.14	9.08	7.9
Per cent. of evaporation due to furnace htg. surface.	—	64.8	—	60.	—	87.
Actual efficiency.....	0.942	0.610	1.001	0.603	0.672	0.585
Theoretical efficiency.....	0 918	0.640	0.946	0.599	0.769	0.499
Ratio of actual and theoreical efficiency.....	1.026	0.953	1.061	1.006	0.875	1.173

of the maximum evaporative results that have been obtained in accurate trials, with sufficient detail to show the reasons for the same. These results are selected from the "Report of 'The Board of Engineers' on the Experiments tried with the Horizontal Fire-Tube and the Vertical Water-Tube Boilers." Philadelphia: 1868. Table XXIV. gives results obtained with the horizontal fire-tube boiler, and Table XXV. results of experiments with the vertical water-tube boiler, the rates of combustion for each boiler covering about the same rates as obtained in the Centennial boiler tests. In these tables the value of furnace heating-surface is shown most decisively, the highest evaporative ratio of each boiler corresponding to the highest rate of combustion, and all the actual results, with two exceptions in the case of the horizontal fire-tube boiler, exceeding the theoretical results which have been found to agree so well with the practical performance of what may be called good in distinction to the best boilers. The marine boiler of the future ought certainly to be as economical as the marine boiler of 1868. Now, in all the preceding tests with anthracite coal, there is only one boiler whose actual performance exceeds the theoretical result, and that is a sectional boiler, experiment 1, Table XXI., with some very efficient furnace heating-surface. This is a suggestive fact.

The evaporative result, experiment 5, Table XXV., 14.86 lbs. of water from and at 212° per lb. of combustible, is the

TABLE XXVII.

EXPERIMENTS ILLUSTRATING THE EFFECT OF ADDING TUBULAR HEATING-SURFACE TO THAT OF THE FURNACE.

Item.	1	2	3	4	5	6
	Horizontal fire-tube boiler.		Vertical water-tube boiler.		Horizontal fire-tube boiler.	
	Tubes closed.	As built.	Tube spaces closed.	As built.	Tubes closed.	As built.
Relative heating surface........	1.000	4.455	1.000	6.999	1.000	3.303
Relative evaporation from and at 212° per lb. of combustible..	1.000	1.543	1.000	1.666	1.000	1.149
Relative horse-power developed.	1.000	1.525	1.000	1.621	1.000	1.257

largest obtained by any reliable experiment with which the writer is acquainted. Prof. Rankine reports an experiment ("Manual of the Steam Engine, and Other Prime Movers," 5th edition, p. 298) with the Earl of Dundonald's boiler, using hand-picked Llangennech coal, in which the evaporation was 14.2 lbs.; and in an experiment made by the writer (see "Engineering," XXVIII., 378) with two boilers of the flue and return-tubular marine type, using anthracite coal of exceptional quality, fired by an expert, the evaporation was 14.54 lbs.

The experiments made by the "Board of Engineers" were so varied in character, that it is possible to determine the value of nearly every portion of the heating-surface of the boilers which they tested. Table XXVI. contains four experiments selected from the report of this Board, and two from "Experimental researches in Steam Engineering," illustrating the value of furnace heating-surface. In each pair of experiments all the conditions were approximately the same, except the amount of heating-surface. It will be noticed that the *per cent.* of evaporation due to the furnace heating-surface varies in the several sets of experiments, and this is because the ratio of furnace to total heating-surface also varies. The latter ratio is:

For experiments 1 and 2.... 0.224
" " 3 and 4.... 0.143
" " 5 and 6.... 0.303

The experiments in Table XXVI., which are there arranged to show the value of the furnace heating surface, also illustrate in a striking manner (see Table XXVII.) the relative inefficiency of the tubular heating-surface, or the comparatively great extension of this heating-surface which is requisite for a slight increase in economy or capacity.

Although the experiments cited in this paper have not been presented in full detail, the most useful items have been selected or calculated from the original data, and only those experiments have been quoted that were sufficiently detailed in the original reports to render it easy to verify every calculated item. Any one who consults for the first time the original reports of experiments, for the purpose of making such a selection, will be surprised to find what a small proportion of the whole number contain all the original data necessary for checking the calculated results. The original data given in a report ought to comprise all the observations and measurements made by the experimenter, so as to enable any one to check his calculations or to extend them if it is desirable. A good illustration of conformity to this principle is to be found in the logs accompanying the report of the Centennial tests, and it is believed that data are there given for checking every item in the tables of results, except that the measurements from which the heating surfaces and draft

OBSERVED DATA.

Number for reference.	Item.	Boiler as originally set.	Boiler with generator attachment.
1	Trial commenced at	6.48 A. M.	6.54 A. M.
2	Trial ended next day, at	6.48 P. M.	6.54 P M.
3	Lbs. of wood used to start fire	270	270
4	Lbs. of coal put into furnace	21,030	22,830
5	Lbs. of refuse	3,112	3,494
6	Lbs. of feed water (apparent evaporation)	156,205	208,394
7	Index of meter, at commencement of test	11,275	18,398
8	Index of meter, at end of test	13,779	21,731
9	Average steam pressure, lbs per sq. in. above atmosphere	69.5	70
10	Average temperature of feed water	44°	44°
11	Average temperature of flue	536°	419°
12	Average steam pressure, lbs. per sq. in above atmosphere, in tests for quality of steam	70	70
13	Average weight of steam condensed, lbs	10 1	10.3
14	Average temperature of condensed steam	115°	122°
15	Average weight of condensing water, lbs	200	200
16	Average initial temperature of condensing water	44°	44°
17	Final " "	99°	100°
	CALCULATED RESULTS.		
18	Heating-surface of boiler, sq. ft.	1414.4	1414.4
19	Heating-surface of generator attachment "	—	272.4
20	Heating-surface total "	1414.4	1686.8
21	Grate surface "	33	33
22	Tube cross-section "	4.86	4.86
23	Chimney cross-section "	3.53	3.53
24	Ratio of heating to grate-surface	42.86	51.12
25	Ratio of tube cross-section to grate-surface	0.147	0.147
26	Ratio of chimney cross-section to grate-surface	0.107	0.107
27	Duration of test, hours	36	36
28	Total weight of coal, including wood, lbs	21,138	22,938
29	Total weight of combustible, lbs	18,026	19,444
30	Per cent of refuse	14.6	15.2
31	Quality of steam compared with saturated steam (= unity)	0.9942	0.9991
32	Actual evaporation, from temperature of feed, at observed steam pressure, lbs	155,294	208,198
33	Actual evaporation from and at 212°, lbs	187,163	251,504
34	Lbs. of coal per hour: Total	587	637
35	Lbs. of coal per hour: Per sq. ft. of grate-surface	17.8	19.3
36	Lbs. of coal per hour: Per sq. ft. of heating-surface	0.415	0.378
37	Lbs. of combustibe per hour. Total	500	540
38	Lbs. of combustibe per hour. Per sq. ft. of grate-surface	15.2	16.4
39	Lbs. of combustibe per hour. Per sq. ft. of heating-surface	0.354	0 320
40	Lbs. of water evaporated per hour, from temperature of feed, at actual pressure. Total	4314	5783
41	Lbs. of water evaporated per hour, from temperature of feed, at actual pressure. Per sq. ft. of grate-surface	130.72	175.25
42	Lbs. of water evaporated per hour, from temperature of feed, at actual pressure. Per sq. ft. of heating-surface	3.05	3.43
43	Lbs of water evaporated per hour, from and at 212°. Total	5211	6986
44	Lbs of water evaporated per hour, from and at 212°. Per sq. ft. of grate-surface	157.91	211.7
45	Lbs of water evaporated per hour, from and at 212°. Per sq. ft. of heating-surface	3.68	4.14
46	Horse-power developed, at rating of 30 lbs. of water evaporated per hour, from and at 212° per horse-power	173.7	232.9
47	Lbs. of coal per hour per horse-power	3.38	2.74
48	Lbs. of combustible per hour per horse-power	2.88	2.32
49	Sq. ft. of grate-surface per horse-power	0.190	0.142
50	Sq. ft. of heating-surface "	8.14	7.24
51	Lbs. of water evaporated at actual pressure from temperature of feed. Per lb. of coal	7.35	9.08
52	Lbs. of water evaporated at actual pressure from temperature of feed. Per lb. of combustible.	8.62	10.71
53	Lbs. of water evaporated from and at 212°. Per lb. of coal	8 87	10.97
54	Lbs. of water evaporated from and at 212°. Per lb. of combustible	10.41	12.94
55	Relative heating-surface	1.000	1.193
56	Relative evaporation from and at 212°, per lb. of combustible	1.000	1.242
57	Actual efficiency	0.771	0.958
58	Theoretical efficiency	0.882	0.897
59	Ratio of actual and theoretical efficiency	0.874	1.068

areas were calculated have been omitted. It may not be amiss to conclude this paper with a report of a boiler test, detailing all the observations and measurements, with the exception of the log from which totals and averages have been computed. The observed data from which the calculations have been made are distinguished by italics. It will be seen that there are some important observations necessary for the calculation of results, and others, perhaps quite as important, which are not used in calculation, but which may be valuable for purposes of comparison, or to enable others to extend the calculations. After what has been said in regard to the value of furnace heating-surface, no excuse will be necessary for selecting an example which shows the effect of adding such surface to a boiler.

Test of a Horizontal Tubular Boiler, as Originally Set, and after the Addition of Stead's Generator Attachment.

Measurements, &c.

One cylindrical tubular boiler, *diameter of shell, 6ft.; length, 18ft.; with 64 tubes, 4 inches outside diameter*, and with steam dome 3ft. in diameter and 4ft. high. Boiler set in brickwork, without arch over top, the products of combustion passing from the front-connection through a short flue into an iron smoke-stack (which is connected with the flue of another similar boiler), *3ft. in diameter*, and 56.5ft. in height above the grate. *Furnace is 6ft. wide, and 5.5ft. long. One-half of the shell of the boiler is exposed to flame and heated gases,* $\frac{7}{10}$ *of the back tube-head and all of the front tube-head.* Boiler is set 28 inches above grate. *Generator attachment consists of bridge-wall drum, diameter, 17 inches; length, 7 feet; one-half exposed; side coils, 2 pipes, 3in. by 5½ft.; 6 pipes, 3 in. by 8ft.; 2 pipes, 3in. by 9ft.; and 8 return bends, 3 in.; bottom and back coils, 12 pipes, 3in. by 9ft.; 6 pipes, 3in. by 5ft.; and 24 return bends, 3in.; furnace arch, containing 15ft. of 2-in. pipe, and three headers, each a piece of 4-in. pipe 1ft. long.*

The test was conducted by hauling the fire from the furnace, starting a fresh fire with dry wood, which was weighed, and was charged as $\frac{4}{10}$ of so much coal. All coal put into the furnace was weighed, and at the conclusion of the trial, the fire, which had been allowed to burn down, was hauled, and the contents of the furnace and ash-pit were charged as refuse. The coal used was Lehigh anthracite, egg size, and the test was made with damper open, but without any special effort to force the fire. The general feed connection of the boiler was broken, the blow-off pipe was blanked, and an independent feed pipe was attached to the boiler and connected with a steam pump, which drew its supply from two tanks placed on platform scales. The feed water, before entering these tanks, passed through a 2-inch Worthington meter. A pyrometer was placed in the front-connection of the boiler, and the steam gauge was compared with a standard gauge before commencing the trial. The quality of the steam generated by the boiler was tested hourly by condensing a small portion in a surface condenser, and noting the pressure of the steam before condensation, its weight and temperature after condensation, and the weight, initial and final temperatures of the condensing water. Hourly observations were taken of steam pressure, feed temperature, flue temperature, and index of meter.

M. G. Tissindier has described to the Paris Academy of Science his new electrical motor for balloons. It consists of a screw propeller with two helicoidal blades nearly 10ft. in diameter, a Siemens dynamo-electrical machine of new design, and a light bichromate of potash battery. It is intended to propel an elongated balloon of about 1000 cubic yards capacity. The frame of the screw propeller weighs 15½lb., is stretched with silk varnished with india-rubber lacquer, and kept taut by steel wire stretchers. The dynamo-electric machine has four electro-magnets in the circuit, and frame parts are of cast steel, so as to bring the weight down to 121 lbs. It drives the screw by gear, which reduces the speed in the proportion of 10 to 1; thus, if the coil makes 1200 revolutions a minute the screw makes 120. It gives out 220 foot-pounds per second with a useful effect of 55 per cent. The bichromate battery gives a better yield than accumulators of the same weight. It consists of an element divided into four series and arranged in tension. The element consists of an ebonite cell holding four litres—or 0.88 gallon—and containing ten plates of zinc and eleven cakes of retort carbon, arranged alternately. The immersed surface of the zinc is one-third that of the carbons. This battery, charged with a highly concentrated and very acid solution, is constant for two hours. The liquid becomes heated as it is impoverished, and the duration of activity may be prolonged by the addition of chromic acid.

THE SIX GATEWAYS OF KNOWLEDGE.*

From "Nature."

I THANK you most warmly for the honor you have done me in electing me to be your president. I value the honor very highly; but when I look at the list of distinguished men who have preceded me in the office, I feel alarmed at the responsibility I have undertaken. A very pleasing duty, however, has been already performed in the interesting and not onerous function we have now gone through. I would gladly speak on the several subjects, for merit in the study of which these prizes have been awarded; but I am afraid that if I were to do so, it would be more for my own gratification than for your pleasure and profit, and I feel that I shall best consult your wishes in passing on at once to the subject of the address which it becomes my duty to give.

The title of the subject upon which I am going to speak this evening might be —if I were asked to give it a title—"The Six Gateways of Knowledge." I feel that the subject I am about to bring before you is closely connected with the studies for which the several prizes have been given. The question I am going to ask you to think of is: What are the means by which the human mind acquires knowledge of external matter?

John Bunyan likens the human soul to a citadel on a hill, self-contained, having no means of communication with the outer world, except by five gates—Eye Gate, Ear Gate, Mouth Gate, Nose Gate, and Feel Gate. Bunyan clearly was in want of a word here. He uses "feel" in the sense of "touch," a designation which to this day is so commonly used that I can scarcely accuse it of being incorrect. At the same time, the more correct and distinct designation undoubtedly is, the sense of touch. The late Dr. George Wilson, first Professor of Technology in the University of Edinburgh, gave, some time before his death, a beautiful little book under the title of "The Five Gateways of Knowledge," in which he quotes John Bunyan in the manner I have indicated to you. But I have said *six* gateways of knowledge, and I must endeavor to justify this saying. I am going to try to prove to you that we have six senses—that if we are to number the senses at all we must make them six.

The only census of the senses, so far as I am aware, that ever made them more than five before was the Irishman's reckoning of seven senses. I presume the Irishman's seventh sense was common sense; and I believe that the large possession of that virtue by my countrymen —I speak as an Irishman—I say the large possession of the seventh sense, which I believe Irishmen have, and the exercise of it, will do more to alleviate the woes of Ireland than even the removal of the melancholy ocean which surrounds its shores. Still I cannot scientifically see how we can make more than six senses. I shall, however, should time permit, return to this question of a seventh sense, and I shall endeavor to throw out suggestions towards answering the question —Is there, or is there not, a magnetic sense? It is possible that there is, but facts and observations so far give us no evidence that there is a magnetic sense.

The six senses that I intend to explain, so far as I can, this evening, are according to the ordinary enumeration, the sense of sight, the sense of hearing, the sense of smell, the sense of taste, and the sense of touch, divided into two departments. A hundred years ago Dr. Thomas Reid, Professor of Moral Philosophy in the University of Glasgow, pointed out that there was a broad distinction between the sense of roughness or of resistance, which was possessed by the hand, and the sense of heat. Reid's idea has not I think been carried out so much as it deserves. We do not, I believe, find in any of the elementary treatises on natural philosophy, or in the physiologists' writings upon the senses, a distinct reckoning of six senses. We have a great deal of explanation about the muscular sense, and the tactile sense; but we have not a clear and broad distinction of the

* An address at the Midland Institute, Birmingham, October 3, 1883, by Prof. Sir William Thomson, LL.D., F.R.S., President.

sense of touch into two departments, which seems to me to follow from Dr. Thomas Reid's way of explaining the sense of touch, although he does not himself distinctly formulate the distinction I am now going to explain.

The sense of touch, of which the organ commonly considered is the hand, but which is possessed by the whole sensitive surface of the body, is very distinctly a double quality. If I touch any object, I perceive a complication of sensations. I perceive a certain sense of roughness, but I also perceive a very distinct sensation, which is not of roughness, or of smoothness. There are two sensations here, let us try to analyze them. Let me dip my hand into this bowl of hot water. The moment I touch the water, I perceive a very distinct sensation, a sensation of heat. Is that a sensation of roughness, or of smoothness? No. Again, I dip my hand into this basin of iced water. I perceive a very distinct sensation. Is this a sensation of roughness, or of smoothness? No. Is this comparable with that former sensation of heat? I say yes. Although it is opposite, it is comparable with the sensation of heat. I am not going to say that we have two sensations in this department—a sensation of heat, and a sensation of cold. I shall endeavor to explain that the perceptions of heat and of cold are perceptions of different degrees of one and the same quality, but that that quality is markedly different from the sense of roughness. Well now, what is this sense of roughness? It will take me some time to explain it fully, I shall therefore say in advance that it is a sense of force; and I shall tell you in advance, before I justify completely what I have to say, that the six senses, regarding which I wish to give some explanation, are: the sense of sight, the sense of hearing, the sense of taste, the sense of smell, the sense of heat, and the sense of force. The sense of force is the sixth sense; or the senses of heat and of force are the sense of touch divided into two, to complete the census of six that I am endeavoring to demonstrate.

Now I have hinted at a possible seventh sense—a magnetic sense—and though out of the line I propose to follow, and although time is precious, and does not permit much of digression, I wish just to remove the idea that I am in any way suggesting anything towards that wretched superstition of animal magnetism, and table-turning, and spiritualism, and mesmerism, and clairvoyance, and spirit-wrapping, of which we have heard so much. There is no seventh sense of the mystic kind. Clairvoyance, and the like, are the result of bad observation chiefly, somewhat mixed up, however, with the effects of wilful imposture, acting on an innocent, trusting mind. But if there is not a distinct magnetic sense, I say it is a very great wonder that there is not.

Many present know all about magnetism. A very large number of pupils have gained an immense amount of valuable knowledge in various subjects, from the classes carried on nightly within the walls of the Birmingham and Midland Institute; and I can see from the prizes that have been awarded, and that I have just now had the pleasure of distributing for excellence and proficiency in this department, that many have learned of magnetism. I had the pleasure of seeing the class-rooms this morning, and I wished I could be in them in the evening to see the studies as carried on in them every evening. Well now, the study of magnetism is the study of a very recondite subject. We all know a little about the mariner's compass, the needle pointing to the north, and so on; but not many of us have gone far into the subject, and not many of us understand all the recent discoveries in electromagnetism. I could wish, had I the apparatus here, and if you would allow me, to show you an experiment in magnetism. If we had before us a powerful magnet, or say the machine that is giving us this beautiful electric light by which the hall is illuminated, it, serving to excite an electro-magnet, would be one part of our apparatus; the other part would be a piece of copper. Suppose then we had this apparatus, I would show you a very wonderful discovery made by Faraday and worked out admirably by Foucault, an excellent French experimenter. I have said that one part of this apparatus would be a piece of copper, but silver would answer as well. Probably no other metal than copper or silver—certainly no other one, of all the metals that are well known, and obtainable for ordinary experiments—possesses, and no other metal or substance, whether metal-

lic or not, is known to possess, in anything like the same degree as copper and silver, the quality I am now going to call attention to.

The quality I refer to is "electric conductivity," and the result of that quality in the experiment I am now going to describe is, that a piece of copper or a piece of silver, let fall between the poles of a magnet, will fall down slowly as if it were falling through mud. I take this body and let it fall. Many of you here will be able to calculate what fraction of a second it takes to fall one foot. If I took this piece of copper, placed it just above the space between the poles of a powerful electromagnet and let it go, you would see it fall slowly down before you; it would perhaps take a quarter of a minute to fall a few inches.

This experiment was carried out in a most powerful manner by Lord Lindsay (now Lord Crawford), assisted by Mr. Cromwell F. Varley. Both of those eminent men desired to investigate the phenomena of mesmerism, which had been called animal magnetism; and they very earnestly set to work to make a real physical experiment. They asked themselves, Is it conceivable that, if a piece of copper can scarcely move through the air between the poles of an electromagnet, a human being or other living creature placed there would experience no effect? Lord Lindsay got an enormous electromagnet made, so large that the head of any person wishing to try the experiment could get well between the poles, in a region of excessively powerful magnetic force. What was the result of the experiment? If I were to say *nothing!* I should do it scant justice. The result was marvelous, and the marvel is that nothing was perceived. Your head, in a space through which a piece of copper falls as if through mud, perceives nothing. I say this is a very great wonder; but I do not admit, I do not feel, that the investigation of the subject is completed. I cannot think that the quality of matter in space which produces such a prodigious effect upon a piece of metal can be absolutely without any—it is certainly not without any—effect whatever on the matter of a living body; and that it can be absolutely without any *perceptible* effect whatever on the matter of a living body placed there seems to me not proved even yet, although nothing has been found. It is so marvelous that there should be no effect at all, that I do believe and feel that the experiment is worth repeating; and that it is worth examining, whether or not an exceedingly powerful magnetic force has any perceptible effect upon a living vegetable or animal body. I spoke then of a seventh sense. I think it just possible that there may be a magnetic sense. I think it possible that an exceedingly powerful magnetic effect may produce a sensation that we cannot compare with heat or force or any other sensation.

Another question that often occurs is, "Is there an electric sense?" Has any human being a perception of electricity in the air? Well, somewhat similar proposals for experiment might, perhaps, be made with reference to electricity; but there are certain reasons, that would take too long for me to explain, that prevent me from placing the electric force at all in the same category with magnetic force. There would be a surface action that would annul practically the force in the interior, there would be a definite sensation which we could distinctly trace to the sense of touch. Any one putting his hand, or his face, or his hair, in the neighborhood of an electric machine perceives a sensation, and on examining it he finds that there is a current of air blowing, and his hair is attracted; and if he puts his hand too near he finds that there are sparks passing between his hand or face and the machine; so that, before we come to any subtle question of a possible sense of electric force, we have distinct mechanical agencies which give rise to senses of temperature and force; but that this mysterious, wonderful, magnetic force, due, as we know, to rotations of the molecules, could be absolutely without effect—without perceptible effect—on animal economy, seems a very wonderful result, and at all events it is a subject deserving careful investigation. I hope no one will think I am favoring the superstition of mesmerism in what I have said.

I intend to explain a little more fully our perceptions in connection with the double sense of touch—the sense of temperature and the sense of force—should time permit before I conclude. But I must first say something of the other

senses, because if I speak too much about the senses of force and heat no time will be left for any of the others. Well, now, let us think what it is we perceive in the sense of hearing. Acoustics is one of the studies of the Birmingham and Midland Institute, of which we have heard many times this evening. Acoustics is the science of hearing. And what is hearing? Hearing is perceiving something with the ear. What is it we perceive with the ear? It is something we can also perceive without the ear; something that the greatest master of sound, in the poetic and artistic sense of the word at all events, that ever lived—Beethoven—for a great part of his life could not perceive with his ear at all. He was deaf for a great part of his life, and during that period were composed some of his grandest musical compositions, and without the possibility of his ever hearing them by ear himself; for his hearing by ear was gone from him for ever. But he used to stand with a stick pressed against the piano and touching his teeth, and thus he could hear the sounds that he called forth from the instrument. Hence, besides the Ear Gate of John Bunyan, there is another gate or access for the sense of hearing.

What is it that you perceive ordinarily by the ear—that a healthy person, without the loss of any of his natural organs of sense, perceives with his ear, but which can otherwise be perceived, although not so satisfactorily or completely? It is distinctly a sense of varying pressure. When the barometer rises, the pressure on the ear increases; when the barometer falls, that is an indication that the pressure on the ear is diminishing. Well, if the pressure of air were suddenly to increase and diminish, say in the course of a quarter of a minute—suppose in a quarter of a minute the barometer rose one-tenth of an inch and fell again, would you perceive anything? I doubt it; I do not think you would. If the barometer were to rise two inches, or three inches, or four inches, in the course of half a minute, most people would perceive it. I say this as a result of observation, because people going down in a diving bell have exactly the same sensation as they would experience if from some unknown cause the barometer quickly, in the course of half a minute, were to rise five or six inches—far above the greatest height it ever stands at in the open air. Well, now, we have a sense of barometric pressure, but we have not a continued indication that allows us to perceive the difference between the high and low barometer. People living at great altitudes—up several thousand feet above the level of the sea, where the barometer stands several inches lower than at sea-level—feel very much as they would do at the surface of the sea, so far as any sensation of pressure is concerned. Keen mountain air feels different from air in lower places, partly because it is colder and drier, but also because it is less dense, and you must breathe more of it to get the same quantity of oxygen into your lungs to perform those functions which the students of the Institute who study animal physiology—and I understand there are a large number — will perfectly understand. The effect of the air in the lungs—the function it performs—depends chiefly on the oxygen taken in. If the air has only three-quarters of the density it has in our ordinary atmosphere here, then one and one-third times as much must be inhaled, to produce the same oxidizing effect on the blood, and the same general effect in the animal economy; and in that way undoubtedly mountain air has a very different effect on living creatures from the air of the plains. This effect is distinctly perceptible in its relation to health.

But I am wandering from my subject, which is the consideration of the changes of pressure comparable with those that produce sound. A diving bell allows us to perceive a sudden increase of pressure, but not by the ordinary sense of touch. The hand does not perceive the difference between 15 lbs. per square inch pressing in all around, and 17 lbs., or 18 lbs., or 20lbs., or even 30 lbs. per square inch, as it experienced when you go down in a diving bell. If you go down five and a half fathoms in a diving bell, your hand is pressed all round with a force of 30 lbs. to the square inch; but yet you do not perceive any difference in the sense of force, any perception of pressure. What you do perceive is this: behind the tympanum, is a certain cavity filled with air, and a greater pressure on one side of the tympanum than on the other gives rise to a painful sensation and sometimes produces rupture of it in a person going

down in a diving bell suddenly. The remedy for the painful sensation thus experienced, or rather I should say its prevention, is to keep chewing a piece of hard biscuit, or making believe to do so. If you are chewing a hard biscuit, the operation keeps open a certain passage, by which the air pressure gets access to the inside of the tympanum, and balances the outside pressure and thus prevents the painful effect. This painful effect on the ear experienced by going down in a diving bell is simply because a certain piece of tissue is being pressed more on one side than on the other; and when we get such a tremendous force on a delicate thing like the tympanum, we may experience a great deal of pain, and it may be dangerous; indeed it is dangerous, and produces rupture or damage to the tympanum unless means be adopted for obviating the difference in the pressures; but the simple means I have indicated are, I believe, with all ordinarily healthy persons, perfectly successful.

I am afraid we are no nearer, however, to understanding what it is we perceive when we hear. To be short it is simply this: it is exceedingly sudden changes of pressure acting on the tympanum of the ear, through such a short time and with such moderate force as not to hurt it; but to give rise to a very distinct sensation, which is communicated through a train of bones to the auditory nerve. I must merely pass over this; the details are full of interest, but they would occupy us far more than an hour if I entered upon them at all. As soon as we get to the nerves and the bones, we have gone beyond the subject I proposed to speak upon. My subject belongs to physical science — what is called in Scotland Natural Philosophy. Physical science refers to dead matter, and I have gone beyond the range whenever I speak of a living body; but we must speak of a living body in dealing with the senses as the means of perceiving—as the means by which, in John Bunyan's language, the soul in its citadel acquires a knowledge of external matter. The physicist has to think of the organs of sense, merely as he thinks of the microscope; he has nothing to do with physiology. He has a great deal to do with his own eyes and hands, however, and must think of them, if he would understand what he is doing, and wishes to get a reasonable view of the subject, whatever it may be, which is before him in his own department.

Now what is the external object of this internal action of hearing and perceiving sound? The external object is a change of pressure of air. Well, how are we to define a sound simply? It looks a little like a vicious circle, but indeed it is not so, to say it is sound if we call it a sound —if we perceive it is as sound, it *is* sound. Any change of pressure, which is so sudden as to let us perceive it is as sound is a sound. There [giving a sudden clap of the hands]—that is a sound. There is no question about it—nobody will ever ask, Is it a sound or not? It is sound if we hear it. If you do not hear it, it is not to you a sound. That is all I can say to define sound. To explain what it is, I can say, it is change of pressure, and it differs from a gradual change of pressure as seen on the barometer only in being more rapid, so rapid that we perceive it as a sound. If you could perceive by the ear that the barometer has fallen two-tenths of an inch to-day, that would be sound. But nobody hears by his ear that the barometer has fallen, and so he does not perceive the fall as a sound. But the same difference of pressure coming on us suddenly—a fall of the barometer, if by any means it could happen, amounting to a tenth of an inch, and taking place in a thousandth of a second —would affect us quite like sound. A sudden rise of the barometer would produce a sound analogous to what happened when I clapped my hands. What is the difference between a noise and a musical sound? Musical sound is a regular and periodic change of pressure. It is an alternate augmentation and diminution of air pressure, ocurring rapidly enough to be perceived as a sound, and taking place with perfect regularity, period after period. Noises and musical sounds merge into one another. Musical sounds have a possibility at least of sometimes ending in a noise, or tending too much to a noise, to altogether please a fastidious musical ear. All roughness, irregularity, want of regular, smooth periodicity, has the effect of playing out of tune, or of music that is so complicated that it is impossible to say whether it is in tune or not.

But now, with reference to this sense

of sound, there is something I should like to say as to the practical lesson to be drawn from the great mathematical treatises which were placed before the British Association, in the addresses of its president, Prof. Cayley, and of the president of the mathematical and physical section, Prof. Henrici. Both of these professors dwelt on the importance of graphical illustration, and one graphical illustration of Prof. Cayley's address may be adduced in respect of this very quality of sound. In the language of mathematics we have just "one independent variable" to deal with in sound, and that is air pressure. We have not a complication of motions in various directions. We have not the complication that we shall have to think of presently, in connection with the sense of force; complication as to the place of application, and the direction, of the force. We have not the infinite complications we have in some of the other senses, notably smell and taste. We have distinctly only one thing to consider, and that is air pressure or the variation of air pressure. Now when we have one thing that varies, that, in the language of mathematics, is "one independent variable." Do not imagine that mathematics is harsh, and crabbed and repulsive to common sense. It is merely the etherealization of common sense. The function of one independent variable that you have here to deal with is the pressure of air on the tympanum. Well now in a thousand counting houses and business offices in Birmingham and London, and Glasgow, and Manchester, a curve, as Prof. Cayley pointed out, is regularly used to show to the eye a function of one independent variable. The function of one independent variable most important in Liverpool perhaps may be the price of cotton. A curve showing the price of cotton, rising when the price of cotton is high, and sinking when the price of cotton is low, shows all the complicated changes of that independent variable to the eye. And so in the Registrar-General's tables of mortality, we have curves showing the number of deaths from day to day—the painful history of an epidemic, shown in a rising branch, and the long gradual talus in a falling branch of the curve, when the epidemic is overcome, and the normal state of health is again approached. All that is shown to the eye; and one of the most beautiful results of mathematics is the means of showing to the eye the law of variation, however complicated, of one independent variable. But now for what really to me seems a marvel of marvels: think what a complicated thing is the result of an orchestra playing—a hundred instruments and two hundred voices singing in chorus accompanied by the orchestra. Think of the condition of the air, how it is lacerated sometimes in a complicated effect. Think of the smooth gradual increase and diminution of pressure—smooth and gradual, though taking place several hundred times in a second—when a piece of beautiful harmony is heard! Whether, however, it be the single note of the most delicate sound of a flute, or the purest piece of harmony of two voices singing perfectly in tune; or whether it be the crash of an orchestra, and the high notes, sometimes even screechings and tearings of the air, which you may hear fluttering above the sound of the chorus—think of all that, and yet that is not too complicated to be represented by Prof. Cayley, with a piece of chalk in his hand, drawing on the blackboard a single line. A single curve, drawn in the manner of the curve of prices of cotton, describes all that the ear can possibly hear, as the result of the most complicated musical performance. How is one sound more complicated than another? It is simply that in the complicated sound the variations of our one independent variable, pressure of air, are more abrupt, more sudden, less smooth, and less distinctly periodic, than they are in the softer, and purer, and simpler sound. But the superposition of the different effects is really a marvel of marvels; and to think that all the different effects of all the different instruments can be so represented! Think of it in this way. I suppose everybody present knows what a musical score is—you know, at all events, what the notes of a hymn tune look like, and can understand the like for a chorus of voices, and accompanying orchestra—a "score" of a whole page with a line for each instrument, and with perhaps four different lines for four voice parts. Think of how much you have to put down on a page of manuscript or print, to show what the different performers are to do. Think, too, how

much more there is to be done than anything the composer can put on the page. Think of the expression which each player is able to give, and of the difference between a great player on the violin and a person who simply grinds successfully through his part; think, too, of the difference in singing, and of all the expression put into a note or a sequence of notes in singing that cannot be written down. There is, on the written or printed page, a little wedge showing a diminuendo, and a wedge turned the other way showing a crescendo, and that is all that the musician can put on paper to mark the difference of expression which is to be given. Well, now, all that can be represented by a whole page or two pages of orchestral score, as the specification of the sound to be produced in say ten seconds of time, is shown to the eye with perfect clearness by a single curve on a riband of paper a hundred inches long. That to my mind is a wonderful proof of the potency of mathematics. Do not let any student in this Institute be deterred for a moment from the pursuit of mathematical studies by thinking that the great mathematicians get into the realm of four dimensions, where you cannot follow them. Take what Prof. Cayley himself, in his admirable address, which I have already referred to, told us of the beautiful and splendid power of mathematics for etherealizing and illustrating common sense, and you need not be disheartened in your study of mathematics, but may rather be reinvigorated when you think of the power which mathematicians, devoting their whole lives to the study of mathematics, have succeeded in giving to that marvelous science.

The sense of sight may be compared to the sense of sound in this respect. I spoke of the sense of sound being caused by rapid variations of pressure. I had better particularize and say how rapid must be the alternations from greatest pressure to least, and back to greatest, and how frequently must that period occur, to give us the sound of a musical note. If the barometer varies once a minute you would not perceive that as a musical note. But suppose by any mechanical action in the air, you could cause the barometric pressure—the air pressure—to vary much more rapidly. That change of pressure which the barometer is not quick enough to show to the eye, the ear hears as a musical sound if the period recurs twenty times per second. If it recurs twenty, thirty, forty, or fifty times per second, you hear a low note. If the period is gradually accelerated, you hear the low note gradually rising, becoming higher and higher, more and more acute, and if it gets up to 256 periods per second, we have a certain note called C in the ordinary musical notation. I believe I describe it correctly as the low note C, of the tenor voice—the gravest C that can be made by a flute. The note of a two-foot organ pipe open at both ends has 256 periods per second. Go on higher and higher to 512 periods per second, and you have the C above that—the chief C of the soprano voice. Go above that to 1,024, you get an octave higher. You get an octave higher always by doubling the number of vibrations per second, and if you go on till you get up to about 5,000 or 6,000 or 10,000 periods per second, the note becomes so shrill that it ceases to excite the human ear and you do not hear it any longer. The highest note that can be perceived by the human ear seems to be something like 10,000 periods per second. I say "something like," because there is no very definite limit. Some ears cease to hear a note becoming shriller and shriller before other ears cease to hear it; and therefore I can only say in a very general way, that something like 10,000 periods per second, is about the shrillest note the human ear is adapted to hear. We may define musical notes, therefore, as changes of pressure of the air, regularly alternating in periods which lie between 20 and 10,000 per second.

Well now, are there vibrations of thirty or forty or fifty or a hundred thousand or a million of periods per second in air, in elastic solids, or in any matter affecting our sense? We have no evidence of the existence in matter of vibrations of very much greater frequency than 10,000 or 20,000 or 30,000 per second, but we have no reason to deny the possibility of such vibrations existing, and having a large function to perform in nature. But when we get to some degree of frequency that I cannot put figures upon, to something that may be measured in millions, if not in hundred thousands of vibrations per second, we have not merely passed the

limits of the human ear to hear, but we have passed the limits of matter, as known to us, to vibrate. Vibrations transmitted as waves through steel, or air, or water, cannot be more frequent than a certain number, which I cannot now put a figure to, but which, I say, may be reckoned in hundred-thousands or a few millions per second.

But now let us think of light. Light we know to be an influence on the retina of the eye, and through the retina on the optic nerve; an influence dependent on vibrations whose frequency is something between 400 million millions per second and 800 million millions per second. Now we have a vast gap between 400 per second, the sound of a rather high tenor voice, and 400 million millions per second, the number of vibrations corresponding to dull red light—the gravest red light of the prismatic spectrum. Take the middle of the spectrum—yellow light—the period of the vibrations there is in round numbers 500 million millions per second. In violet light we have 800 million millions per second. Beyond that we have something that the eye scarcely perceives—does not perceive at all perhaps—but which I believe it does perceive, though not vividly: we have the ultra violet rays, known to us chiefly by their photographic effect, but known also by many other wonderful experiments, that within the last thirty years have enlarged our knowledge of light to a most marvelous degree. We have invisible rays of light made visible by letting them fall on a certain kind of glass, glass tinged with uranium — that yellowish green glass, sometimes called canary glass or chameleon glass. Uranium glass has a property rendering visible to us invisible rays. You may hold a piece of uranium glass in your hand, illuminated by this electric light, or by a candle, or by gas light, or hold it in the prismatic spectrum of white light, and you see it glowing according to the color of the light which falls upon it; but place it in the spectrum beyond the visible violet end, where without it you see nothing, where a piece of chalk held up seems quite dark, and the uranium glass glows with a mysterious altered color of a beautiful tint, revealing the presence of invisible rays, by converting them into rays of lower period, and so rendering them visible to the eye. The discovery of this property of uranium glass was made by Prof. Stokes, and the name of fluorescence from fluor spar, which he found to have the same property, was given to it. It has since been discovered that fluorescence and phosphorescence are continuous, being extremes of the same phenomenon. I suppose most persons here present know the luminous paint made from sulphides of calcium and other materials, which, after being steeped in light for a certain time, keep on for hours giving out light in the darkness. Persistence in emission of light after the removal of the source, which is the characteristic of those phosphorescent objects, is manifested also, as Edmund Becquerel has proved, by the uranium glass, and thus Stoke's discovery of fluorescence comes to be continuous with the old known phenomenon of phosphorescence, to which attention seems to have been first called scientifically by Robert Boyle about 200 years ago.

There are other rays, that we do not perceive in any of these ways, but that we do perceive by our sense of heat: heat rays as they are commonly called. But in truth all rays that we call light have heating effect. Radiant heat and light are one and indivisible. There are not two things, radiant heat and light: radiant heat is identical with light. Take a black hot kettle into a dark room, and look at it. You do not see it. Hold your face or your hand near it, and you perceive it by what Bunyan would have called Feel Gate; only now we apply the word feeling to other senses as well as Touch. You perceive it before you touch it. You perceive it with the back of your hand, or the front of your hand; you perceive it with your face, yes, and with your eye, but you do not see it. Well, now, must I justify the assertion that it is not light? You say it is not light, and it is not so to you, if you do not see it. There has been a good deal of logic-chopping about the words here; we seem to define in a vicious circle. We may begin by defining light—"It is light if you see it as light; it is not light if you do not see it." To save circumlocution, we shall take things in that way. Radiant heat is light if we see it, it is not light if we do not see it. It is not that there are two things; it is that radiant heat has differences of quality. There are quali-

ties of radiant heat that we can see, and if we see them we call them light; there are qualities of radiant heat we cannot see, and if we cannot see them we do not call them light, but still call them radiant heat: and that on the whole seems to be the best logic for this subject.

By the by, I don't see Logic among the studies of the Birmingham and Midland Institute. Logic is to language and grammar what mathematics is to common sense; logic is etherealized grammar. I hope the advanced student in grammar and Latin and Greek, who needs logic perhaps as much as, perhaps more than, most students of science and modern languages, will advance to logic, and consider logic as the science of using words, to lead him to know exactly what he means by them when he uses them. More ships have been wrecked through bad logic than by bad seamanship. When the captain writes down in his log—I don't mean a pun here, log has nothing to do with logic—the ship's place is so-and-so, he means that it is the most probable position — the position which, according to previous observations, he thinks is the most probable. After that, supposing no sights of sun or stars or land to be had, careful observation of speed and direction shews, by a simple reckoning (called technically the dead reckoning), where the ship is next day. But sailors too often forget that what they put down in the log was not the ship's place, but what to their then knowledge was the most probable position of the ship, and they keep running on as if it was the true position. They forget the meaning of the very words in which they have made their entry in the log, and through that bad logic more ships have been run on the rocks than by any other carelessness or bad seamanship. It is bad logic that leads to trusting to the dead reckoning, in running a course at sea; and it is that bad logic which is the cause of those terribly frequent wrecks; of steamers, otherwise well conducted, in cloudy but perfectly fine weather, running on rocks at the end of a long voyage. To enable you to understand precisely the meaning of your result when you make a note of anything about your own experience or experiments, and to understand precisely the meaning of what you write down, is the province of logic. To arrange your record in such a manner that if you look at it afterwards it will tell you what it is worth, and neither more nor less, is practical logic; and if you exercise that practical knowledge, you will find benefits that are too obvious if you only think of any scientific or practical subject with which you are familiar.

There is danger then of a bad use of words, and hence of bad reasoning upon them, in speaking of light and radiant heat; but if we distinctly define light as that which we consciously perceive as light—without attempting to define consciousness, because we cannot define consciousness any more than we can define free will—we shall be safe. There is no question that you see the thing; if you see it, it is light. Well now, when is radiant heat light? Radiant heat is light when its frequency of vibration is between 400 million millions per second and 800 million millions per second. When its frequency is less than 400 million millions per second it is not light; it is invisible "infra-red" radiant heat. When its frequency is more than 800 million millions per second, it is not light if we cannot see it; it is invisible ultra-violet radiation, truly radiant heat, but it is not so commonly called radiant heat because its heating effect is known rather theoretically than by sensory perception, or thermometric or thermoscopic indications. Observations which have been actually made by Langley and by Abney on radiant heat take us down about three octaves below violet, and we may hope to be brought considerably lower still by future observations. We know at present in all about four octaves—that is from one to two, two to four, four to eight, eight to sixteen, hundred million millions —of radiant heat. One octave of radiant heat is perceptible to the eye as light, the octave from 400 million millions to 800 million millions. I borrow the word octave from music, not in any mystic sense, nor as indicating any relation between harmony of colors and harmony of sound, No relation exists between harmony of sound and harmony of colors. I merely use the word "octave" as a brief expression for any range of frequencies lying within the ratio of one to two. If you double the frequency of a musical note, you raise it an octave: in that sense I use the word for the moment in respect

to light, and in no other sense. Well now, think what a tremendous chasm there is between the 100 million millions per second, which is about the gravest hitherto discovered note of invisible radiant heat, and the 10,000 per second, the greatest number of vibrations in sound. This is an unknown province of science: the investigation of vibrations between those two limits is perhaps one of the most promising provinces of science for the future investigator.

In conclusion, I wish to bring before you the idea that all the senses are related to force. The sense of sound, we have seen, is merely a sense of very rapid changes of air pressure (which is force) on the drum of the ear. I have passed merely by name over the senses of taste and smell. I may say they are chemical senses. Taste common salt and taste sugar—you tell in a moment the difference. The perception of that difference is a perception of chemical quality. Well, there is a subtle molecular influence here, due to the touch of the object, on the tongue or the palate, and producing a sensation which is a very different thing from the ordinarily reckoned sense of touch, in the case now considered, telling only of roughness and of temperature. The most subtle of our senses perhaps is sight; next comes smell and taste. Prof. Stokes recently told me that he would rather look upon taste and smell and sight as being continuous because they are all molecular—they all deal with properties of matter, not in the gross, but molecular actions of matter; he would rather group those three together than he would couple any of them with any of the other senses. It is not necessary, however, for us to reduce all the six senses to one, but I would just point out that they are all related to force. Chemical action is a force, tearing molecules apart, throwing or pushing them together: and our chemical sense or senses may therefore so far at least be regarded as concerned with force. That the senses of smell and taste are related to one another seems obvious; and if physiologists would pardon me, I would suggest that they may, without impropriety, be regarded as extremes of one sense. This at all events can be said of them, they can be compared—which cannot be said of any other two senses. You cannot say that the shape of a cube, or the roughness of a piece of loaf sugar or sandstone, is comparable with the temperature of hot water, or is like the sound of a trumpet, or that the sound of a trumpet is like scarlet, or like a rocket, or like a blue-light signal. There is no comparability between any of these perceptions. But if any one says, "That a piece of cinnamon tastes like its smell," I think he will express something of general experience. The smell and the taste of pepper, nutmeg, cloves, cinnamon, vanilla, apples, strawberries, and other articles of food, particularly spices and fruits, have very marked qualities, in which the taste and the smell seem essentially comparable. It does seem to me, although anatomists distinguish between them, because the sensory organs concerned are different and because they have not discovered a continuity between these organs, we should not be philosophically wrong in saying that smell and taste are extremes of one sense—one kind of perceptivity—a sense of chemical quality materially presented to us.

Now sense of light and sense of heat are very different; but we cannot define the difference. You perceive the heat of a hot kettle—how? By its radiant heat against the face—that is one way. But there is another way, not by radiant heat, of which I shall speak later. You perceive by vision, but still in virtue of radiant heat, a hot body, if illuminated by light, or if hot enough to be self-luminous, red-hot or white-hot, you see it; you can both see a hot body and perceive it by its heat, otherwise than by seeing it. Take a piece of red-hot cinder with the tongs, or a red-hot poker, and study it; carry it into a dark room, and look at it. You see it for a certain time; after a certain time you cease to see it, but you still perceive radiant heat from it. Well now there is radiant heat perceived by the eye and the face and the hands all the time; but it is perceived only by the sense of temperature, when the hot body ceases to be red-hot. There is then, to our senses, an absolute distinction in modes of perception between that which is continuous in the external nature of the thing, namely, radiant heat in its visible and invisible varieties. It operates upon our senses in a way that I cannot ask anatomists to admit to be one and the same in both cases. They cannot now, at all

events, say that there is an absolute continuity between the retina of the eye in its perception of radiant heat as light, and the skin of the hand in its perception of radiant heat as heat. We may come to know more; it may yet appear that there is a continuity, Some of Darwin's sublime speculations may become realities to us; and we may come to recognize a cultivable retina all over the body. We have not done that yet, but Darwin's grand idea occurs as suggesting that there may be an absolute continuity between the perception of radiant heat and by the retina of the eye and its perception by the tissues and nerves concerned in the mere sense of heat. We must be content in the meantime, however, to make a distinction between the senses of light and heat. And indeed it must be remarked that our sense of heat is not excited by radiant heat only, while it is only and essentially radiant heat that gives to the retina the sense of light. Hold your hand under a red-hot poker in a dark room: you perceive it to be hot solely by its radiant heat, and you see it also by its radiant heat. Now place the hand over it: you feel more of heat. Now, in fact, you perceive its heat in three ways—by contact with the heated air which has ascended from the poker, and by radiant heat felt by your sense of heat, and by radiant heat seen as light (the iron being still red-hot). But the sense of heat is the same throughout, and is a certain effect experienced by the tissue, whether it be caused by radiant heat, or by contact with heated particles of the air.

Lastly, there remains—and I am afraid I have already taxed your patience too long—the sense of force. I have been vehemently attacked for asserting this sixth sense. I need not go into the controversy; I need not explain to you the ground on which I have been attacked; I could not in fact, because in reading the attack I have not been able to understand it myself. The only tangible ground of attack, was, that a writer in New York published this theory in 1880. I had quoted Dr. Thomas Reid, without giving a date; his date chances to be 1780 or thereabouts. But physiologists have very strenuously resisted admitting that the sense of roughness is the same as that muscular sense which the metaphysicians who followed Dr. Thomas Reid in the University of Glasgow, taught. It was in the University of Glasgow that I learned the muscular sense, and I have not seen it very distinctly stated elsewhere. What is this "muscular sense"? I press upon the desk before me with my right hand, or I walk forward holding out my hand in the dark, and using this means to feel my way, as a blind man does constantly who finds where he is, and guides himself by the sense of touch. I walk on until I perceive an obstruction by a sense of force in the palm of the hand. How and where do I perceive this sensation? Anatomists will tell you it is felt in the muscles of the arm. Here, then, is a force which I perceive in the muscles of the arm, and the corresponding perceptivity is properly enough called a muscular sense. But now take the tip of your finger and rub a piece of sandstone, or a piece of loaf sugar, or a smooth table. Take a piece of loaf sugar between your finger and thumb, and take a smooth glass between your finger and thumb. You perceive a difference. What is that difference? It is the sense of roughness and smoothness. Physiologists and anatomists have used the word "tactile" sense, to designate it. I confess that this does not convey much to my mind. "Tactile" is merely "of or belonging to touch," and in saying we perceive roughness and smoothness by a tactile sense, we are where we were. We are not enlightened by being told that there is a tactile sense as a department of our sense of touch. But I say the thing thought of is a sense of force. We cannot away with it; it is a sense of forces, of directions of forces, and of places of application of forces. If the places of application of the forces are the palms of the two hands, we perceive accordingly, and know that we pereeive, in the muscles of the arms, effects of large pressures on the palms of the hands. But if the places of application are a hundred little areas on one finger, we still perceive the effect as force. We distinguish between a uniformly distributed force like the force of a piece of smooth glass, and forces distributed over ten or a hundred little areas. And this is the sense of smoothness and roughness. The sense of roughness is therefore a sense of forces, and of places of application of forces, just as the sense of forces in your two hands stretched out

is the sense of forces in places at a distance of six feet apart. Whether the places be at a distance of six feet or at a distance of one hundredth of an inch, it is the sense of forces, and of places of application of forces, and of directions of forces, that we deal with in the sense of touch other than heat. Now anatomists and physiologists have a good right to distinguish between the kind of excitement of tissue in the finger, and the minute nerves of the skin and sub-skin of the finger, by which you perceive roughness and smoothness, in the one case; and of the muscles by which you perceive places of application very distant, in the other. But whether the forces be so near that anatomists cannot distinguish muscles, cannot point out muscles, resisting forces and balancing them—because, remember, when you take a piece of glass in your fingers every bit of pressure of every ten-thousandth of an inch pressed by the glass against the finger is a balanced force—or whether they be far asunder and obviously balanced by the muscles of the two arms, the thing perceived is the same in kind. Anatomists do not show us muscles balancing the individual forces experienced by the small areas of the finger itself, when we touch a piece of smooth glass, or the individual forces in the scores or hundreds of little areas experienced when we touch a piece of rough sugar or rough sandstone; and perhaps it is not by muscles smaller than the muscles of the finger as a whole that the multitudinousness is dealt with; or perhaps, on the other hand, these nerves and tissues are continuous in their qualities with muscles. I go beyond the range of my subject whenever I speak of muscles and nerves; but externally the sense of touch other than heat is the same in all cases—it is the sense of forces and of places of application of forces and of directions of forces. I hope now I have justified the sixth sense; and that you will excuse me for having taxed your patience so long in not having done it in fewer words.

NOTE ON THE OPTICAL FORMULA EXPRESSING THE RELATION OF CONJUGATE DISTANCES, AND ON THE THEORY OF THE STADIA.

By R. S. WOODWARD, C. E.

Contributed to Van Nostrand's Engineering Magazine.

In No. 4, Vol. 30 of this magazine, in an article on "The Theory of Stadia Measurements," by Mr. Arthur Winslow, some doubt is raised as to the correctness and range of applicability of the formula expressing the relation between conjugate distances and the principal focal length of a lens or system of lenses. Some new errors are also added in this article to a system of optics already abounding in limitations and obscurities, and the resulting theory of the stadia is left on a very insecure basis, especially since an appeal is made to experiment to show that the usual formula for this instrument is applicable if the telescope used has the ordinary achromatic objective instead of a single bi-convex lens.

It is desired in this note to show, 1st, that although the formula for conjugate distances as commonly understood is inaccurate, yet, if properly interpreted, this formula is not only approximate, but exact; and, moreover, applies equally without modification to any combination of lenses as well as to a single bi-convex lens: 2d, that the ordinary formula for the stadia instrument if properly understood is exact, whatever may be the number, kind, or disposition of the lenses in the telescope so long as they are properly centered.

The theory of the action of a system of lenses on a ray of light passing through them was first adequately discussed by Gauss in his Dioptrische Untersuchungen, 1840 (see list of references below). So exhaustive was his treatment of the subject that few improvements have since been made. His memoir is a master-

piece of mathematical investigation, and the results derived possess at once great generality and simplicity. It is an unfortunate fact, however, that this theory has not found a place in our English text-books, but instead we have repeated errors and assumptions which lead to results confessedly approximate without showing the degree of approximation, and, moreover, results no less complex than the results of the true theory.

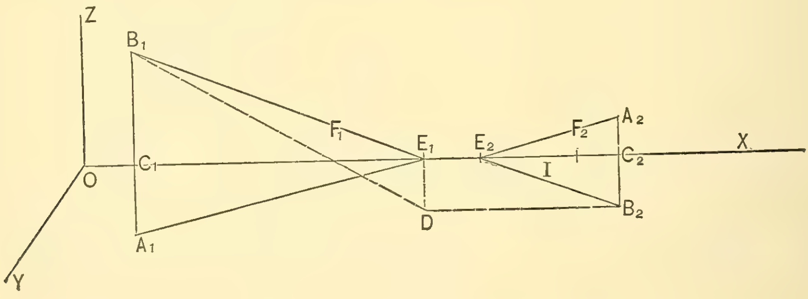
The fundamental assumptions of Gauss's theory of the path of a ray of light traversing a system of lenses are:

1. The bounding surfaces of the media (lenses) through which the light passes are spherical and have their centers of curvature in the same straight line or axis.

2. The inclination, i, of the ray at any point of its path is such that sin i and tan i may be replaced by i.

3. The angular width, a, of either spherical surface measured at its center, is such that sin a and tan a may be replaced by a.

Let the centers of curvature of the several bounding surfaces of a system of lenses lie in the axis of X (see diagram). Then whatever the number of lenses or



whatever their indices of refraction, there exist always two pairs of points F_1, F_2 called focal points, and E_1, E_2 called principal points. Rays parallel before incidence, and traversing the system of lenses in the direction OX, will be brought to a focus in a plane cutting OX perpendicularly at F_2; conversely, rays parallel before incidence and traversing the system in the opposite direction, will be brought to a focus in a plane cutting OX perpendicularly at F_1. $E_1 F_1$ and $E_2 F_2$ are the principal focal lengths of the system, and in case F_1 and F_2 lie in the same medium, $E_1 F_1 = E_2 F_2$. These focal and principal points are fixed for any fixed system of lenses. For a system composed of a single double convex or a single double concave lens, the points E_1, E_2 lie somewhat within (not on) the bounding surfaces. For methods of determining the positions of these points in any case, reference may be made to the works cited at the end of this note. These four points afford the following simple relations between the coördinates of an object and the coördinates of its image. Let $x_1\ y_1\ z_1$ be the coördinates of any point of the object, and $x_2\ y_2\ z_2$ the coordinates of the image of this point. Also denote the abscissas of the points E_1, E_2, F_1, F_2 by these same letters respectively. Then when the incident and emergent rays are in the same medium, and f is the principal focal length of the system,

$$(F_1 - x_1)(x_2 - F_2) = f^2 \quad . \quad . \quad (1)$$

$$\frac{1}{E_1 - x_1} + \frac{1}{x_2 - E_2} = \frac{1}{f} \quad . \quad . \quad (2)$$

$$\frac{y_2}{y_1} = \frac{z_2}{z_1} = -\frac{f}{F_1 - x} = -\frac{x_2 - F_2}{f} \qquad (3)$$

Equations (1) and (2) express the relation of conjugate distances according as they are referred to the pair of points F_1, F_2 or E_1, E_3. Equation (3) gives the ratio of corresponding linear dimensions of the object and image. From this equation we have evidently

$$\frac{y_2}{y_1} = \frac{z_2}{z_1} = -\frac{f + x_2 - F_2}{f - x_1 + F_1} = \frac{E_2 C_2}{E_1 C_1} \quad . \quad (4)$$

This shows that the line $B_2 E_2$ is parallel to $B_1 E_1$, and in connection with (3), gives an easy geometrical construction of the image of any point of an object

when the principal and focal points are given, as indicated by the dotted line in the diagram.

To apply this theory to the case of the stadia instrument put

$$C_1E_1=f_1,\ C_2E_2=f_2$$
$$A_1C_1=z'_1,\ A_2C_2=z'_2$$

Then equations (2) and (4) become

$$\frac{1}{f_1}+\frac{1}{f_2}=\frac{1}{f} \quad . \quad . \quad . \quad . \quad (5)$$

$$\frac{f_1}{f_2}=\frac{z_1}{z_2}=\frac{z'_1}{z'_2}=\frac{z_1+z'_1}{z_2+z'_2} \quad . \quad . \quad (6)$$

Now $z_1+z'_1=A_1B_1$ may represent the space on a stadia target covered by a fixed interval $z_2+z'_2=A_2B_2$ in the plane of the image of the target, the optical axis of the telescope being perpendicular to $A_1\,B_1$ at C_1. Put

$$z_1+z'_1=s, \text{ and } z_2+z'_2=s_0.$$

Then (6) gives

$$\frac{1}{f_2}=\frac{1}{f_1}\frac{s}{s_0}$$

which substituted in (5) gives

$$f_1=\frac{f}{s_0}s+f$$

Denote the distance from the center of the instrument I to the target by d, and the distance IE_1 from the outer principal point of the objective to the center of the instrument by c. Then

$$d=f_1+c=\frac{f}{s_0}s+f+c \quad . \quad . \quad (7)$$

This, therefore, is the correct formula for the stadia instrument (for "horizontal sight") whatever the kind of telescope through which the space s on the target is observed. Whether the telescope be composed of one lens or any number of lenses it is only necessary to remember that f and c are distances between fixed points as defined above, and that s_0 is a fixed interval in the plane of the image of the object. Hence $\frac{f}{s_0}$ and $f+c$ are constants for any telescope.

It remains to show how the constants $\frac{f}{s_0}$ and $f+c$ may be determined with precision. Obviously, if two different distances d, and the corresponding spaces s, be observed, these constants can be computed. In all such cases, however, it is desirable to have more than the algebraically sufficient data, and resort to the method of least squares. In applying this method we might consider both the observed quantities d and s subject to error, and hence to correction; but inasmuch as d may be observed with much greater precision than s, the latter only will be considered subject to error. Since the observed quantity must be the absolute term in an observation-equation, we write (7) in the form

$$d\,\frac{s_0}{f}-\frac{s_0}{f}(f+c)-s=v \quad . \quad . \quad (8)$$

in which v is the residual or most probable correction to the observed value of s. The error in s will vary directly as the distance f_1, or with sufficient accuracy as d. Hence the proper weight of the above observation-equation is $\frac{1}{d^2}$. Now put

$$\frac{s_0}{f}=u+\Delta u \text{ and } (f+c)=w+\Delta w$$

in which u and w are approximate values of the left-hand members of these equations respectively, and such that the product of the corrections Δu and Δw may be neglected. Then (8) becomes

$$\Delta u(d-w)-u\Delta w+ud-uw-s=v$$

with weight $\frac{1}{d^2}$

Multiply this by the square root of its weight, and put for brevity,

$$1-\frac{w}{d}=a, \quad \frac{u}{d}=b \text{ and}$$

$$u-\frac{1}{d}(uw+s)=-n$$

Then the observation-equation will be of the type,

$$a\Delta u-b\Delta w-n=\frac{v}{d} \quad . \quad (9)$$

and the normal equations are,

$$\begin{aligned} [aa]\,\Delta u-[ab]\,\Delta w-[an]&=o \\ -[ab] \quad +[bb] \quad +[bn]&=o \end{aligned} \quad (10)$$

To show the application of these form-

ulas we take the data furnished by Mr. Winslow on p. 319, Vol. 30, No. 4, of this journal. These data and the derived quantities are shown in the following table. The approximate values assumed for u and w are:

d	s	a	b	n	$\frac{v}{d}$
50.0 ft.	0.4855 ft.	0.9700	0.0002000	−0.0000100	−170
100. "	0.9850 "	.9850	1000	000	+437
200. "	1.9850 "	.9925	500	000	+240
300. "	2 9878 "	.9950	333	− 090	−725
400. "	3.9840 "	.9962	250	+ 025	+392
500. "	4.9867 "	.9970	200	− 030	−177

$$u=0.01 \qquad w=1.5 \text{ feet.}$$

The values of the residuals in the last column of the table are expressed in units of the eighth decimal place.

The normal equations are, after multiplying them by 10^8,

$$587260000\Delta u-42010\Delta w-1915.6=0$$
$$-42010 \quad +5.3636 \quad +0.22975=0$$

The values of the unknowns and their probable errors are,

$$\Delta u=+0.00000045\pm0.00000208$$
$$\Delta w=-\overset{\text{ft.}}{0.0393}\pm\overset{\text{ft.}}{0.0213}$$

Hence

$$\frac{1}{u+\Delta u}=\frac{f}{s_0}=99.9541\pm0.0029$$
$$w+\Delta w \;.\; f+c=\overset{\text{ft.}}{1.461}\pm\overset{\text{ft.}}{0.021}$$

These results indicate that the values of d and s used as data were carefully observed. They also show that a high degree of precision is attainable with the stadia, as compared with chaining, over rough ground.

List of works giving Gauss's theory of a system of lenses:

1. The original Dioptrische Untersuchungen is contained in Vol. V. of Gauss's works, published by the Royal Society of Göttingen, 1867.

2. A complete translation in French of Gauss's memoir, by M. A. Bravais, may be found in Annales de Chemie et de Physique, Vol. 33, 3d Series, 1851, and also in Liouvilles Journal for 1856.

3. A very clear though incomplete exposition of Gauss's theory is given in Die Haupt und Breun-Puncte eines Linsen Systemes, by Carl Neumann. B. G. Teubuer, Leipzig, 1866.

4. A brief development of the theory is also given by Jamin in Cours de Physique, Vol. 3. Gauthier Villars, Paris, 1879.

Straightening Hardened Steel.—It is well known that files are not usually drawn after being hardened, and that the hardening frequently springs them out of line. But notwithstanding that the files are made as hard as they can be by heat and cold water, they are readily straightened after being hardened. This operation is performed at once, as soon as the files have been dipped. The files are taken from a bath of melted lead and chilled while red hot in a tank of running water. This immersion for the instant hardens only the surfaces, while the interior is soft and pliant with heat. At this time the file may be straightened by bending over and under bars. By similar means crooks in steel, arbors, reamers, and other long tools may be removed, even after they have been hardened and tempered. A cast steel saw arbor had received an offset or crook in the journal at one end just inside the shoulder. The crook was at the worse end, that next the saw, and although scarcely perceptible to the eye when the arbor was turned on its centers, it was sufficient, when the arbor was in the boxes, to throw the periphery of a two-foot saw considerably out. The arbor at the bearing part was very gradually heated—not enough to change color but to a "black heat." A V-shaped block was placed in a vice bearing against the offset side of the journal, and the vice screwed up. At the third trial the arbor came out perfectly true. A tempered reamer was straightened in the same way, the point at which it was crooked being heated by an alcohol lamp. The heat was sufficient to allow the steel to give, but not enough to start the temper. Steel that has a blue temper only may be straightened by blows with a pony hammer on a smooth, clean anvil, the face of which should be warmed enough to remove the chill.—*Scientific American.*

TESTING MACHINES, THEIR HISTORY, CONSTRUCTION AND USE.

By ARTHUR V. ABBOTT.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

IV.

DEFORMATION.

Upon all materials of a ductile character the effect of the stress is to produce more or less deformation in the size and the shape of the piece. In tension specimens the piece is elongated considerably, and the point at which the fracture occurs is by the flow of the metal drawn down more or less to a conical point. Pieces that are subjected to compression are shortened in length, and by a reversed process are bulged out into a sort of a barrel shape form; while in transverse stress the piece is bent into a U-shaped form. In tension pieces the amount of elongation and the amount of contraction of area are regarded as exceedingly good indications of the value of the material for structural purposes.

ELEVATION OF THE ULTIMATE AND THE ELASTIC LIMIT.

It was first observed by Prof. R. H. Thurston and Commander L. A. Beardsley, U. S. N., independently, that if a ductile material be subjected to a stress beyond the elastic limit, but not beyond its ultimate resistance and then allowed to rest for a definite period of time, a considerable increase of elastic limit and the ultimate resistance may be detected. In other words, the application of stress and the subsequent rest increases the resistance of the material. Table No. 4 gives a number of experiments made by the U. S. Board on specimens of various sizes. Each specimen was subjected to two tests, and the length of time allowed between first and second tests is given in the table. It will thus be seen from the inspection of this table that the ultimate resistance was very largely increased by allowing the piece to rest more or less between the experiments. The cause of this may perhaps be explained by the theory that, as soon as the elastic limit is reached and the metal begins to flow, the molecules, subjected to the force of the stresses, take up new positions with reference to each other. As long as this stress is continued the moving molecules have acquired a certain momentum.. If,

TABLE 4.

Size of bar	Strength in Pounds per Square Inch.		Interval between	Gain in Strength
	1st Test.	2d Test.	Tests.	Pr. ct.
1¼	50,825	51,351	1 minute	1
1⅞	48,809	49,110	1 minute	0.6
1¼	49,877	50,614	3 minutes	1.5
1⅜	49,024	49,637	3 minutes	1.3
1½	49,865	50,388	3 minutes	1.0
⅞	49,345	49,993	1 hour	1.3
⅞	49,358	50,219	2 hours	1.7
⅞	49,459	51,362	3 hours	3.8
⅞	49,484	51,546	4 hours	4.2
⅞	49,401	51,561	5 hours	4.3
⅞	49,206	51,996	6 hours	5.6
⅞	50,257	52,886	7 hours	5
⅞	50,013	52,572	8 hours	5
1	51,536	60,631	3 days	17.6
1¼	49,935	58,251	3 days	17
1	49,962	56,207	3 days	12.5
1⅜	49,175	57,635	3 days	17 2
1½	49,267	58,049	3 days	17.8
1⅝	50,143	58,136	3 days	14.1
1¾	49,266	57,263	3 days	16.2
1¾	49,438	57,991	3 days	17.3
1⅞	48,537	54,655	3 days	12.6
2	48,597	57,124	3 days	17.5
1⅜	48,853	57,443	8 days	17.6
1½	50,015	59,047	8 days	18
1⅛	50,474	59,864	18 days	18.6
1⅛	50,178	58,314	18 days	16.1
1⅛	50,165	54,749	18 days	9
1⅝	49,676	59,184	25 days	19.1
1⅝	49,867	55,949	42 days	12.2
1	51,128	60,902	6 months	19.1
1⅛	50,530	59,626	6 months	18.
1¼	49,101	57,877	6 months	17.8
1⅜	48,819	56,885	6 months	16.6
1½	51,838	57,188	6 months	10.3
1⅝	49,144	58,188	6 months	18.3
1¾	48,792	57,403	6 months	17.2
1⅞	49,370	58,880	6 months	19.4
2¼	49,250	58,020	6 months	17.7
2½	47,871	58,976	6 months	22.6
2¾	46,702	54,458	6 months	16.6
3	47,665	57,250	6 months	20

Abstract from Detail of Tests.

	Per cent.	Tests.
Average gain in less than 1 hour.	1.1 ..	5
Average gain in less than 8 and over 1 hour..................	3.8 ..	8
Average gain in 3 days	16.2 ..	10
Average gain in 8 days..........	17.8 ..	2
Average gain in over 8 days and less than 43 days...............	15.3 ..	5
Average gain in 6 months......	17.9 ..	12

however, the stress be removed and the piece be left to itself, the molecules come to rest and assume new positions with reference to each other, and the forces of cohesion again come into play and give the piece an increase of tenacity.

EFFECT OF MECHANICAL TREATMENT.

The amount of work that is done upon a piece of iron or steel, whether under the hammer or in the rolling mill, plays an important part in the quality of the material. It has been found by experiment that bars of ductile material, which are apparently precisely alike in every respect, except in area of section, do do not give the same elastic or ultimate

Table No. 5.

Diameter of bar.	Tensile Strength.	Elastic Limit.	Diameter of bar.	Tensile Strength.	Elastic Limit.	Diameter of bar.	Tensile Strength.	Elastic Limit.
1/4	59,885	——	1 3/8	53,016	35,379	1 13/16	50,969	30,814
3/8	54,090	40,980	"	51,296	31,992	"	50,307	29,767
1/2	62,700	——	"	50,594	34,940	"	48,953	——
"	59,000	——	1 1/2	57,052	38,417	1 7/8	55,803	31,031
"	57,700	——	"	56,505	32,496	"	53,100	32,074
"	55,400	——	"	55,131	33,771	"	52,875	35,641
"	52,275	39,126	"	54,540	——	"	52,505	32,312
5/8	55,450	——	"	55,415	32,869	"	51,459	27,816
"	52,050	——	"	54,354	34,617	"	50,363	——
"	57,660	——	"	54,544	33,027	"	51,039	33,067
3/4	51,546	35,933	"	53,512	——	"	49,744	35,615
7/8	50,630	33,931	"	52,819	34,840	"	48,670	23,250
1	61,727	——	"	52,736	34,901	2.0	60,213	31,441
"	57,363	37,415	"	52,700	35,880	"	52,914	31,198
"	57,807	39,230	"	52,155	27,708	"	49,164	——
"	56,790	36,885	"	51,994	32,054	"	51,684	33,104
"	51,921	31,300	"	51,456	34,591	"	52,127	32,461
"	52,819	32,267	"	51,047	——	"	52,011	34,702
"	51,400	34,600	1 5/8	56,344	35,889	"	51,146	28,567
1 1/8	60,458	37,344	"	57,402	35,701	"	50,000	36,184
"	57,470	31,900	"	56,227	33,207	"	50,171	28,983
"	57,498	41,311	"	54,334	32,163	"	47,812	35,864
"	55,927	37,250	"	53,339	33,540	"	48,249	31,413
"	54,644	34,695	"	53,614	30,664	"	46,151	36,050
"	53,900	26,787	"	52,675	33,745	2 1/16	51,559	——
"	53,035	34,410	"	52,314	29,364	"	49,422	——
"	52,267	32,019	"	52,401	34,012	2 1/8	50,481	——
1 1/4	59,461	36,501	"	51,205	33,318	"	51,225	——
"	57,897	32,469	"	50,970	33,625	"	48,382	30,459
"	55,782	35,596	1 11/16	56,595	38,310	2 3/16	51,666	——
"	56,334	33,921	"	54,114	——	2 1/4	51,530	——
"	55,253	34,784	"	57,789	34,160	"	49,290	32,163
"	53,893	32,712	1 3/4	57,874	——	"	48,898	——
"	53,247	32,520	"	54,410	31,354	"	46,866	28,241
"	53,752	——	"	53,846	36,573	2 1/2	48,475	28,932
"	52,970	32,075	"	55,018	34,283	"	47,428	29,941
"	53,022	——	"	53,264	——	"	47,344	29,758
"	50,040	30,730	"	53,154	35,323	2 3/4	46,446	26,333
1 3/8	58,926	37,548	"	51,509	20,404	3.0	47,761	26,400
"	58,021	32,152	"	50,395	36,254	3 1/4	47,014	24,591
"	54,949	31,030	"	50,547	35,954	3 1/2	47,000	24,961
"	54,277	33,622	"	49,816	31,214	3 3/4	46,667	23,636
"	52,733	34,606	"	50,129	32,271	4.0	46,322	23,430
"	53,557	33,650	1 13/16	56,577	——			
"	52,537	34,469						

TABLE No. 6.—RECTANGULAR BARS.

Number.	Kind of Iron.	Size of Bar.	Stress in Lbs. per Square Inch.	
			Elastic Limit.	Ultimate.
1	Single Refined.	3×1 inches.	29,000	52,470
2	Double "	3×1 "	31,000	53,550
3	Single "	3×1¼ "	27,330	50,410
4	Double "	3×1¼ "	27,170	50,920
5	Single "	3×1 "	28,330	48,700
6	Double "	3×1 "	29,170	51,370
7	Single "	5×1¼ "	24,830	49,240
8	Double "	5×1¼ "	27,170	51,010

TABLE No. 7.

Progress No.	Blow No.	Bar Section.	Specimen.			E. Lim. lbs. □″	Ult. St. lbs. □″	Elong. %	Reduc %	C.
			L′	D″	□″					
A......	98190	6″×1	1	1×1	1	46000	81000	21	57.75	20.5
B.......	"	"	"	1×.970	.970	50515	82744	20	57.99	
C.......	"	2′×2	"	.980×980	.960	44792	79166	25	58.51	
D......	"	"	"	980×.612	.600	46647	79966	25	52.42	
Direct...	"	1×1	"	1.002×1.002	1.004	50797	80677	16.5	85.43	
A......	98204	6×1	"	1.003×.998	1.001	44955	76423	21.75	56.19	15
B.......	"	"	"	1×1.006	1.006	46223	76451	21.75	48.43	
C.......	"	2×2	"	1.007×.990	.997	44133	76981	23	63.39	
D......	"	"	"	.998×.605	.605	41322	75207	24	39.67	
Direct...	"	1×1	"	1.002×.998	1.000	42000	69250	20	50.30	
" ...	"	"	"	1.×1.	1.	43000	69000	22.2	71.23	
A......	98221	6×1	"	1.005×.990	.995	43216	72361	24.25	40.14	18
B.......	"	"	"	1.020×.995	1.015	43352	72660	23.	42.37	
C.......	"	2×2	"	1.005×1.005	1.010	42079	70544	27.	53.48	
D......	"	"	"	1.005×.598	.598	41807	72324	23.	57.41	
Direct...	"	1×1	"	1×1	1.000	43000	72000	21.	71.91	

resistance per square inch. Other things being equal, bars of the smallest cross section give the greatest intensity of ultimate resistance. Table 5 from the report of the U. S. Board to test iron and steel, gives a very complete exposition of the differences in various sized round iron bars.

The following experiments given in Table No. 7 were made at the Cambria Iron Co. during the investigation of the steel for the East River Bridge, to determine the difference between large and small specimens of the same steel. From several blows of steel, containing varying proportions of carbon, three different sized pieces were rolled. One piece was rolled to be two inches square, a second piece was rolled one inch thick and six inches wide, and the third piece was rolled one inch square. The pieces two inches square were set on a planer and two pieces cut out of them. One piece, marked C, in the accompanying table, was cut directly from the center, while a second piece, marked D, was cut from the side. It will thus be seen that the piece C came directly from the center of the large bar, and was taken from the spot where it had been subjected to the least possible amount of mechanical work, and had no rolled skin on it. The piece D came from the side of the bar, and had been subjected to a trifle more mechanical work, and had one surface of rolled skin. The 6×1-inch bar was cut into two parts, marked respectively A and B. By cutting from the side of the bar one piece, marked B, consequently had three surfaces of rolled skin, while A being cut next to it had only two surfaces of rolled skin. The bars marked direct from the rolls were

rolled to one inch square, and had rolled skin on all four sides.

Table No. 8 gives a series of comparative tests between specimens as usually

TABLE No. 8.

	Blow No.	Bar Section.	Specimen. L′	D″	□″	E. Lim. lbs. □″	Ult. St. lbs. □″	Elong. %	Re-duc. %	M. Elas. lbs. □″	C.
	93709	1″×1″	1	1.002 1.002	1.004	49004	74203	22.	68.60	28871000	18
	"	3½×½ flat	"	1.512 .483	.730	52192	75573	23.	54.38	31111000	18
	93711	1″×1″	"	1.000 1.000	1.000	51000	77500	21.	73.95	29411000	23
	"	6″ channel C	"	1.754 .447	.784	49489	75893	14.	64.92	29663000	23
	"	" F	"	1.385 .336	.507	53254	78895	19.	59.57	26627000	23
	"	" F	"	1.766 .436	.770	51948	77922	20.	51.66	29514000	23
	93854	1″×1″	"	.980 .980	.960	49200	76780	18.	72.6	31049000	21
	"	1½″×9/16″ flat	"	1.455 .565	.822	55960	80280	22.4	64.23	29290000	21
	93921	1″×1″	"	.990 .990	.980	53058	82899	20.	77.23	30300000	19
	"	5′×¼″ flat	"	2.980 .237	.706	56657	87819	21.	62.61	25753000	19
	94187	1″×1″	1	1.000 1.000	1.000	52000	81000	19.5	74.30	27808000	25
Full size	"	7″×½″ flat	3	7.03 .510	3.585	49512	84240	11.66	65.91	——	25
	94229	1″×1″	1	1.002 1.002	1.004	49800	76942	19.75	75.72	27977000	21
	"	3″×3″ angle	"	1.178 .373	.439	52392	82004	20.	66.97	27894000	21
	94855	1″×1″	"	.992 .992	.984	50813	75477	21.5	72.56	26774000	17
	"	6″×½″ flat	"	— —	—	56833	78484	23.25	63.06	26023000	17
	99767	1″×1″	"	1.007 1.007	1.014	49310	76923	22.	69.92	29527000	19
	"	⅝″ round	"	.666	.348	54885	84770	22.5	43.10	23950000	19
	100706	1″×1″	"	1.000 .998	.998	52104	81062	20.	71.54	26815000	18
Full size	"	9″×⅜″ flat	"	8.950 .370	3.311	53126	76292	29.	49.84	——	18
	101079	1″×1″	"	1.002 1.002	1.004	50798	80179	19.	67.69	25384000	18
	"	9″ channel C	"	1.504 .508	.764	45812	74607	22.1	54.58	——	18
	"	" F	"	1.501 .503	.755	47682	74172	22.6	65.96	——	18
	"	" F	"	1.505 .509	.766	48303	73107	29.9	62.01	——	18
	101140	1″×1″	"	1.000 1.000	1.000	54000	84000	20.	74.82	2425000	18
Full size	"	5″×½″ flat	"	4.854 1.524	7.397	47081	76418	19.4	55.09	30000000	18
	101237	1″×1″	"	1.000 1.000	1.010	50485	79208	19.	75.80	26710000	18
Full size	"	3½″×½″ flat	"	3.489 .512	1.786	47368	72172	28.	61.82	26200000	18
	104452	1″×1″	"	1.002 1.006	1.018	49116	77603	22.	65.26	27000000	17
Full size	"	6″×¼″ flat	2	5.980 .260	1.555	47267	79331	20.4	38.26	26000000	17

sent to the testing machine, and pieces cut from the same grade of steel after having undergone rolling into structural shapes. Those marked "1×1" were test pieces of steel broken at the Cambria Iron Co's. works in Johnstown. While the piecesmarked as having been cut from variousshapes were test pieces of channels, beams and flats of the same blow, numbered as each of the preceding pieces, but after each steel had been rolled into the desired shapes, the letters "C" and "F" denoting that the piece was cut from the center or flange of the bar.

Another curious fact observed during making these steel tests for the East River Bridge was the fact that the time which elapsed between the rolling of the bar and the experiment in the testing machine had a considerable effect upon the amount of reduction and elongation obtained. Bars which were placed in a testing machine as soon as they were cold were generally found to give a reduction and elongation of not over eighteen to twenty per cent. for elongation, and not over twenty-five to thirty-five of reduction; whereas test pieces cut from the same bar, after having been allowed to lie quietly for some weeks, gave an elongation of from twenty to thirty per cent. and a reduction of from forty to sixty per cent. It would seem that the molecular condition of the bar just from the rolls is exceedingly unsettled, and if the bar be allowed to remain quietly for some time, a process analogous perhaps to that of crystallization takes place, and the molecules have time to arrange themselves in more accommodating positions so as to present a great accommodation to the stresses applied by the testing machine.

UNIFORM SIZE OF TEST PIECE.

It is obvious that some agreement with reference to the standard size for test pieces which is extremely desirable. At the present time experimenters all over this country and Europe are constantly making experiments upon a great variety of materials, and collecting a vast amount of data which can be only partly used, from the fact that the pieces experimented upon are so diverse in character, in chemical constitution, in size, and in the shapes from which they are cut, as to render comparison of various results almost an impossibility. The adoption by investigators of some system regarding the size and dimension of the pieces to be used for experimenting would therefore seem to be a necessity. The following standard sizes, as illustrated in Fig. 30, having been found advantageous by the author, are suggested, not by any means as absolute and unvaried standards, but simply as one step towards the desired end.

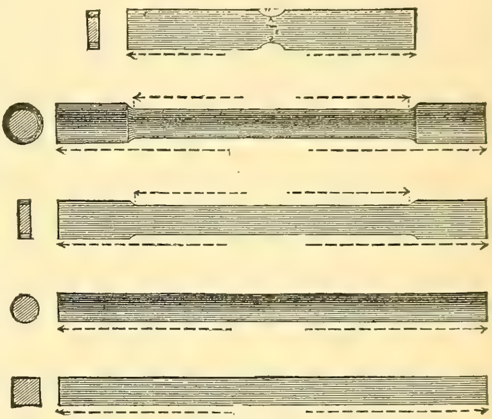


Fig. 30.

For making experiments on steel and iron intended to fulfill bridge and railway specifications pieces direct from the rolls may be used, as indicated in the first two samples.

For sample pieces of squares and rounds, pieces twenty-four inches long may be cut directly from the bar, having as nearly as may be one square inch of cross section. In squares this of course simply means a bar rolled one inch square, while in rounds the piece should be one and one-eighth inch in diameter. Similar observation will apply to flats and boiler plates. In the case of boiler plates, strips should be cut off having a length of twenty-four inches and having, as near as possible, widths so as to give one-fourth, one-half, three-fourths and one inch, or one square inch of cross section. Rounds, squares or flats which cannot readily be obtained in lengths of twenty-four inches, pieces may be prepared, as shown in the second two samples of the above cut. Here the pieces are from sixteen to twenty inches in length, having the center part, for a

Table 9.—Chemical Analyses of Ingot Irons and Steels.

ARRANGED ACCORDING TO PER CENT. OF CARBON.

Number.	Carbon. Total.	Combined.	Graphitic.	Sulphur.	Phosphorus.	Silicon.	Manganese.
1	.009	——	——	.009	.084	.163	.020
2	.057	.049	.008	.007	.179	.219	.063
3	.130	.116	.014	.029	.045	.011	.192
4	.234	.230	.004	trace	.039	.084	none
5	.238	——	——	.012	.034	.105	.184
6	.401	——	——	.006	.032	.085	.112
7	.463	——	——	.002	.020	.121	trace
8	.577	.459	.118	.026	.108	.108	.185
9	.639	.627	.012	trace	.007	.154	.050
10	.691	.675	.016	.028	.065	.028	.459
11	.756	.744	.012	.043	.104	.074	.465
12	.806	.793	013	none	.019	.172	.193
13	.873	.833	.040	trace	.015	.134	.046
14	.923	.908	.015	.002	.014	.141	.036
15	.996	.984	.008	none	.019	.157	.245
16	1.072	1.059	.013	"	.022	.162	.252
17	1.121	1.108	.013	"	.023	.206	.269
18	1.154	1.142	.012	"	.020	.204	.282
19	1.328	1.244	.084	"	.017	.246	.262

distance of ten inches, planed or turned so as to give a reduced section. In order to break a piece of uniform size, so as to have the fracture occur exterior to the jaws of the machine, it is necessary to have the test piece sufficiently long, so that the crushing or indenting occasioned by the jaws may be distributed over quite a large area of the test piece, thereby avoiding any danger of crushing or cutting into the piece itself, so as to cause a fracture to occur inside of the jaw. In order to secure this it is essential to have a piece which is sufficiently long so as to present the required length between the jaws and give an additional quality to be placed between the gripping surfaces In order to be certain that no crushing or indenting action occurs, it has been found by experiment best to have at least ten times as much surface in the jaws of the testing machine as there are inches in the cross section in the piece to be tested. If, however, the piece is by machine work so reduced in the center as to present a less cross section than is obtained between the jaws of the machine, this indenting action is entirely avoided, and samples which cannot be obtained in lengths of twenty-four inches or more, may be prepared as above shown. The last figure in the cut indicates the shape of boiler plate test pieces which has previously been used as a standard size by the Government Inspectors of Steam Boilers. The piece is prepared very simply by cutting a strip from a boiler plate and planing or milling a semicircular groove directly in the center of the piece. While it may frequently be necessary to make experiments on pieces of this shape, they are, as has readily been seen from the foregoing experiments, much to be avoided. There is no opportunity for measuring the elastic limit of the piece, as there is no uniform section upon which to take it; also the grooved section of the piece very largely increases the ultimate strength of the material, at the same time decreasing the amount of reduction and entirely precluding the possibility of any stretch measurement. In order to obtain an accurate determination of the elastic limit with a fair estimate of the ultimate strength, elongation and reduction of a material, a length of at least six inches is necessary. In previous experiments it has generally been customary to use a length of eight inches, from the fact that most testing machines have been built of so small capacity that longer pieces were not conveniently handled. This length of eight inches is, however, unfortunate for many reasons. It verges so closely upon the limiting size of the test as to at last raise the question of the accuracy of the result. All of the measurements made

for determining the elastic limit and modulus of elasticity have to be reduced to decimals of the length of the test piece, involving laborious calculation for each reading of the micronometer or vernier gauge. The length of ten inches now proposed as a standard of size of test piece will avoid all computations of this kind, for every reading thus being made in inches, is made in per cent. of the length of the test piece.

Furthermore, the length of ten inches corresponds to two hundred and fifty millimeters, approximately the length of test piece commonly used abroad.

EFFECTS OF CHEMICAL CONSTITUTION.

The resistance of wrought iron to tension varies very marked with its chemical constitution. At present it is quite uncertain to what extent foreign elements in the iron affect its quality.

Variations in the amount of phosphorus and sulphur in wrought iron play a very important part in its physical characteristics, and cannot be too carefully considered in presenting an opinion of the value of the material for structural use.

In steel the effect of variations in chemical constitution is far more marked than it is in the case of iron. The elements, phosphorus, silicon, manganese, carbon and sulphur, all enter into the constitution of most steels in small proportions. A very slight variation in any one of these may make a very marked difference in the quality and value of the steel. The elements phosphorus, silicon, manganese and carbon have been termed by Dr. Dudley as hardening elements, inasmuch as each one of them is capable of conveying to the steel a greater or less degree of hardness. Dr. Dudley, in some experiments presented to the American Institute of Mining Engineers, assumes the hardening effect of phosphorus, silicon, manganese and carbon to be in proportion to the numbers three, five, seven and a-half and fifteen, and reckons the sum of their effects in what he very aptly terms "phosphorus units."

Dr. Dudley concludes that the sum of all these above constituents reckoned in phosphorus units should not exceed thirty to thirty-two units in steels intended for rails. The above figures, thirty-one units, are obtained by adding together the phosphorus percentage, half the per cent. of silicon, one-third that of carbon and one-fifth of manganese. Taken singly, the limit of phosphorus should be placed at $\frac{1}{10}$ of one per cent.; silicon at $\frac{4}{100}$ of a per cent.; manganese at $\frac{3}{10}$ to $\frac{4}{10}$ of a per cent. And carbon from $\frac{25}{100}$ to $\frac{30}{100}$ of a per cent. Any higher proportions than the preceding make the steel too brittle for structural purposes.

The experiments in Tables 9 and 10, made by Prof. Thurston for the Committee on Chemical Research of the United States Board for testing iron and steel, exhibited the chemical constitution with the corresponding physical qualities of ingot irons and steels.

TABLE 10.

Number.	Elongation.	Elastic Limit.	Ultimate Strength.
	Per cent.	Lbs. pr. sq. in.	Lbs. pr. sq. in.
1	29.67	26,500	43,000
2	25.50	34,500	55,000
3	34.33	28,500	52,000
4	20.83	26,000	60,000
5	12.00	49,400	69,700
6	21.67	50,743	71,300
7	20.17	44,000	71,000
8	19 50	47,800	83,100
9	2.75	50,000	94,500
10	3.58	50,500	101,000
11	1.00	65,190	101,400
12	9.75	50,500	112,400
13	8.17	50,500	113,100
14	11.08	51,000	118,900
15	10.08	61,900	122,200
16	7.67	68,100	123,000
17	8.08	68,100	125,500
18	8.67	75,200	130,380
19	7.33	75,300	135,300

A gradual increase in tensile strength and elastic limit may be observed as the proportion of carbon is increased. The quality of the metal isusually fixed by the proportion of carbon but it is also varied to a large extent by the elements, silicon and manganese, as well as by the amount of phosphorus. Prof. Thurston has given the following formula, by means of which a very fair estimate of the strength of steel may be obtained when the proportion of carbon is known: $T=60,000+70,000\ C$. T is the tensile strength in pounds per square inch, and C is the percentage of carbon for annealed samples, and $T=50,000+60,000\ C$. As illustrating this excess

Prof. Thurston has made the following experiments :

TABLE 11.

Carbon.	Tensile Strength—Lbs. per sq. in.	
Per cent.	By Test.	By Calculation.
0.53	79,062	81,740
0.65	93,404	88,940
0.80	99,538	98,060
0.87	106,579	102,020
1.01	109,209	110,300
1.09	116,394	113,480

The preceding examples illustrate very clearly the importance of the chemical constitution, and in all specifications for building materials, whether of iron or steel, the chemical constitution of the metal should be required by the engineer or the architect. It is very true that the knowledge of the engineer is not supposed to be that of the manufacturer, and it would be highly unreasonable for the engineer to prescribe to the manufacturer exactly what formula he should use to obtain the material wished for. At the same time a clause inserted under the head of tests in specifications, requiring a chemical analysis of every melt of steel and frequent analyses of wrought iron, would add very largely to the amount of knowledge obtained of the material to be used, to say nothing of the immense advantage which would accrue, both to the manufacturer, to the engineer, and to the general scientific community, by a correlation between the physical tests and the chemical constituents. While the manufacturer should be allowed a wide margin of chemical constitution, it would be well in specifications, especially those designed for steel work, to prescribe limits for the elements of manganese, carbon, phosphorus and silicon, which the manufacturer should not be allowed to transcend. For it is very possible to make a steel which may come up to a liberal specification and at the time be in many respects unfit for use in a bridge or a building, and in order to avoid difficulties of this kind, limiting maximum and minimum values to the proportions of carbon, phosphorus, manganese and silicon should be assigned in the specifications as well as the maximum and minimum values for elastic limit, tensile strength, reduction and elongation.

WORK DONE IN TESTING.

One of the most important qualities of the material is its resilience or power of resisting shock. This quality is measured by the mechanical work done during the processes of testing. Until recently this quality has been rather overlooked by engineers, but at the present time is demanding more and more attention. The mechanical work done upon a piece tested in tension is most readily obtained by the autographic method, for it is simply the area of the curve enclosed between the axis of X, the curved line given by the test of the piece and the bounding ordinate parallel to the axis of Y. It will thus be seen that the tensile strength, together with the elongation of the piece, are factors in its resilience, so that a piece very stiff and brittle, while it might have an exceedingly high tensile strength, would, having very little elongation, give a very low resilience, and again, softer material having an exceedingly low tensile strength, but a very large amount of elongation, might err on the other side and give equally unfavorable results.

Practical experience has shown that the quality demanded in our bridges, roofs and other structures, is the power of resisting sudden shocks and jars due to the work to which they are subjected, and this power of resisting shock is by far the most important one. A piece of tool steel, while exceedingly strong, will, under a suddenly applied stress, snap like a piece of brittle glass, while the weaker and more resilient structural steel will, under a suddenly applied load, stretch like a piece of india-rubber and come back again to its normal position. While the amount of work done in breaking a test piece is an exceedingly valuable and important characteristic of amount of work up to the elastic limit, is by far the most valuable. And this quantity may be readily calculated by taking the elongation at the elastic limit and multiplying it by half the elastic limit in pounds per square inch. Assuming Hook's Law to be true, this gives the value of the triangle included by the line whose equation is $Y = ax$, and may be as-

sumed to be very accurately the measure of the resilience of the piece up to the elastic limit.

EFFECT OF TEMPERATURE.

As a result of a mechanical work done on the piece by a machine, and of the flowing of the molecules of the metal, the piece gradually becomes warm, so that if the testing machine is very rapidly worked, and the bar is comparatively small, the ends near the fracture may at the end of the experiment become so hot as to be most unbearable to the hand. Probably this elevation of the temperature during the experiment has some effect upon the ultimate resistance, for unquestionably an elevation of the temperature causes the flow of the metal to take place with slightly increased facility. Yet in ordinary experiments this elevation of the temperature is so slight as to produce no sensible effect upon the test piece. Some experimenters have gone so far as to carefully make a thermometrical measurement of the heat produced in this way. While all possible commendation should be given to accuracy and care in making observations, it seems that examinations of this kind are hardly worth the amount of expenditure and trouble that they cause.

That the variation of temperature produces some effect upon the tensile strength of iron and steel is a well understood fact.

From a recent number of the *London Engineering*, a synopsis of some German experiments is obtained. The resistance of the materials at zero centigrade is taken at 100, and that of other temperatures in the proper proportional part of that number, Table 12.

TABLE 12.

Temperature. Cent.	Temperature. Fahr.	Fibrous Iron.	Fine Grained Iron.	Bessemer Steel.
0°	32°	100	100	100
100	212	100	100	100
200	392	95	100	100
300	572	90	97	94
500	932	38	44	34
700	1,292	16	23	18
900	1,652	6	12	9
1,000	1,832	4	7	7

Sir William Fairbairn gives the following as the results of numerous experiments made upon specimens of plate and rivet iron at different temperatures, Tables 13 and 14.

TABLE 13.—EXPERIMENTS ON PLATE IRON.

Temperature, Fahrenheit.	Breaking weight in lbs. per. sq. in.	Stress in Reference to Fibre.
0°	49,009	With.
60	40,357	Across.
60	43,406	Across.
60	50,219	With.
110	44,160	Across.
112	42,088	With.
120	40,625	With.
212	39,935	With.
212	45,680	Across.
212	49,500	With.
270	44,020	With.
340	49,968	With.
340	42,088	Across.
395	46,086	With.
Scarcely red	38,032	Across.
Dull red,	30,513	Across.

TABLE 14.—RIVET IRON.

Temperature. Fahr.	Breaking weight in lbs. per sq. in.	Temperature. Fahr.	Breaking weight in lbs. per sq. in.
−30°	63,239	250	82,174
+60	61,971	270	83,098
66	63,661	310	80,570
114	70,845	325	87,522
212	82,676	415	81,830
212	74,153	435	86,056
212	80,985	Red heat.	36,076

From these tables it will be observed that between the ordinary variation and temperature there is little or no variation of the strength of iron or steel. That many accidents occur from the breakage of railroad rails, tie rods, axles, and many other parts of our common structure during cold and frosty weather, is beyond question; but these failures must be attributed not to a decrease in actual strength of the metal, but to exterior circumstances. Under severe cold of a winter's night the road bed enveloping the track of the railway may become frozen exceedingly hard, and to the passage of the moving train present a structure which is firm and unyielding as that of the most solid granite. Thus the

blow struck by the engine driver against a projecting rail end is much more severe on a cold and frosty day than it is on a warm and sunny one, so that the possible breakage of the rail under circumstances of this kind may be attributed, not to a decrease in the strength of the iron, but to exterior circumstances.

CAST IRON TESTS.

The investigator in making experiments on cast iron is obliged to exercise more care than in experiments on almost any other material. Cast iron is of such an unyielding and crystalline nature, that unless care is taken in setting the piece in the testing machine the results are almost certain to be vitiated by the presence of lateral stresses. Again, unless the piece is prepared in some manner by turning and planing at the center, so as to give a reduced section, there is great danger that the jaws of the testing machine will not grip the piece sufficiently tightly so as to avoid slipping.

If the piece should be taken direct from the sand, without any machining or planing, it becomes quite a difficult matter to get an accurate measurement upon which to calculate the area of the piece, for the outside of the specimen contains a large quantity of sand, which penetrates to a depth of one or two hundredths of an inch, so as to almost preclude the possibility of making accurate measurements.

The effect of remelting on the strength of cast iron is well exemplified by the experiments in Tables 15 and 16.

TABLE 15.

	Lbs. per sq. in.
First melting	14000
2d "	22900
3d "	30229
4th "	35786

TABLE 16.

Time of fusion.	Lbs. per sq. in.
½ hour	17843
1 "	20127
1½ "	24387
2 "	34496

The accompanying table gives a fair average of the results to be generally obtained from tension tests on cast iron:

Good pig iron from 15,000 to 20,000 lbs. per square inch.

Tough cast iron from 18,000 to 24,000 lbs. per square inch.

Hard cast iron from 20,000 to 28,000 lbs. per square inch.

Gun-metal from 25,000 to 32,000 lbs. per square inch.

These are the averages that have been obtained from a large number of miscellaneous irons obtained from various foundries all over the country. Here and there will be found an iron that either exceeds or falls below the preceding averages, as an example of which may be mentioned a phenomenal one sent to the author to be tested by the author within the past year from the foundry of Mr. Gridley, at Warsaw, N. Y. This piece, of pig iron unrefined stood over 40,000 lbs. per square inch.

CEMENT TESTING.

Next to iron and steel, probably the materials which are most subjected to experiment, are the various cements and concretes used for all architectural purposes. Among architects and engineers it is very customary upon buying a lot of cement to take small samples from several of the barrels in each cargo, and after mixing them up and pressing them into a mold, to break the sample by tension, and to judge of the quality of the cement by the results thus obtained.

In making cement tests the following precaution should be observed:

First, the relative quantity of cement and water should be carefully measured, preferably by weighing out the necessary amount of each.

Second, the cement and the water should be carefully and thoroughly incorporated so as to make a mixture which is perfectly uniform in every respect. This is best accomplished by placing the cement on a plate of glass, and slowly pouring the water on it while the mass is being stirred and rubbed by means of a towel.

Third, after the mixture is completed, the cement should be pressed into the mold designed to form the briquette with a perfectly steady and uniform pressure. This is best accomplished by arranging the mold so that it may be filled with cement, then subjected by means of a press to an amount of force that can be definitely weighed.

Fourth, the length of time elapsing between the mixing of the cement and the breaking of the sample should be accurately noted.

Fifth, the temperature of the room in which the samples are kept between the mixing and the breaking, should be maintained as nearly uniform as may be, and preferably should be kept at from 60 to 70 degrees Fahrenheit.

Sixth, the fineness of the grinding of the cement should be carefully noted. It has been customary among English and American experimenters to weigh out a portion of the cement to be tested, and sift it through a sieve containing 2,500 meshes per square inch. The fineness of the cement is expressed by the per cent. of the amount which will pass through the measures of the sieve.

Seventh, the hydraulic properties of the cement are best ascertained by mixing a sample to a requisite quantity of water, and then testing it in two ways:

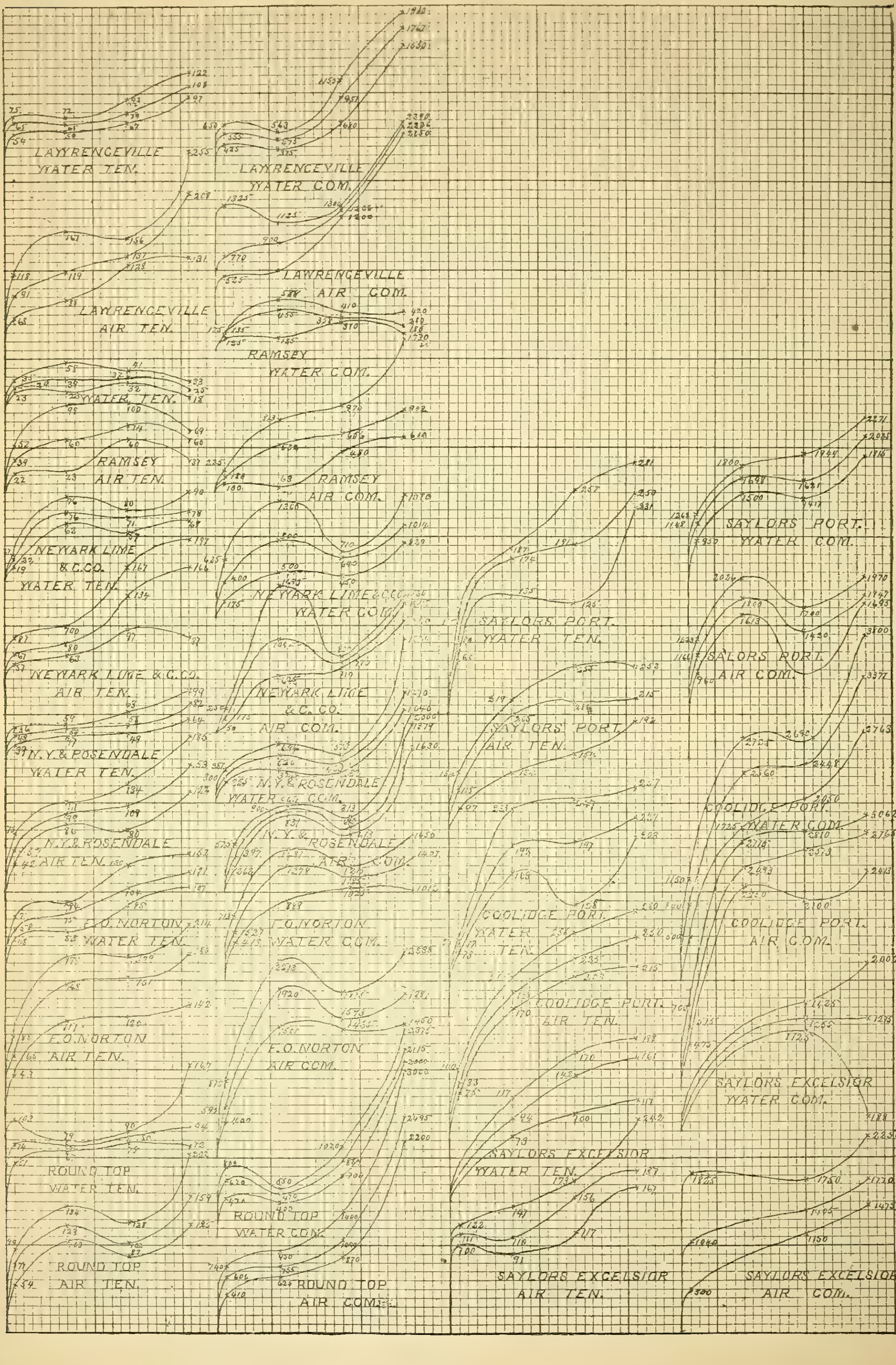
First, by immersing it in water and seeing whether the cement does not lose its form after being subjected to the water for some time and does not show any cracks or other symptons of disintegration.

Second, by allowing a blunt point to press upon the cement by means of a definite weight, the penetration being assumed as a measure of the setting properties of the cement.

At the commencement of the erection of the approaches to the East River Bridge, it was decided to employ cement for laying the masonry work. As a consequence of this, proposals for supplying cement were advertised for by the Bridge Trustees. A number of manufacturers sent samples in answer to this advertisement, and Fig. 31 gives a series of curves that are taken from the results obtained from these tests. All the samples here given were made by mixing 4 parts of cement with one part of water by weight. The mixture was then rammed into molds having a cross section of 3 inches, and a uniform length of 3 inches between the enlarged ends. After the briquettes had been in the molds for 15 minutes they were removed, half the number of briquettes being placed in water, and half being retained in the air until the time for testing. After the tension test was made, the end of the broken briquette was trued up on a grindstone and tested in compression. As a consequence, on the diagram there will be seen 4 curves for each make of cement, one curve giving the tensile strength in air, one in water, one of compressive strength in air, and one of compressive strength in water. The experiments were continued till the briquette had attained an age of one month, and tests were made at intervals of 24 hours, 7 days, 14 days, 21 days and 28 days. An inspection of these curves will reveal the fact that while they vary considerably in actual strength per square inch, all the curves bear a general resemblance to each other. Each curve rising with a slight inclination to the axis of y, until the seven day test is passed. There then occurs a point of inflection, the curve swinging towards the axis of x, in some cases even taking on a negative value, and returning towards that axis. This dropping of the curve continues until the fourteenth day is passed, when the second point of inflection occurs, the curve returns towards its former position and extends indefinitely. It would seem from these experiments that the hardening of cement is probably due to two different causes, one of which reaches the maximum about the seventh day, while the other does not come into action until after the fourteenth day. In explanation of this fact it is supposed by the author that this hardening is due to two different variations. When the cement is first mixed, the primary hardening is due to the hydration of the salts of lime and alumina by means of the water used in the mixing. This hydration reaches its maximum in intensity at about the seventh day. After that the action of the atmosphere on the cement probably causes the hydrates of calcium and alumina to give up part of the water that is absorbed, and change to silicates and carbonates. This interchange of molecules causes a slight weakening of the cement. This weakening goes on from seven to fourteen days, until after the passage of the fourteenth day the carbonates and silicates have only regained the strength of the hydrates they have replaced, and then probably go on hardening and strengthening for an indefinite period of time.

The Table 17 gives the result of a series of tests made on cement supplied by two of the most noted Rosendale Cement Company's.

These tests were made on briquettes mixed in the same manner as those in the


LAWRENCEVILLE WATER TEN.
LAWRENCEVILLE WATER COM.
LAWRENCEVILLE AIR TEN.
LAWRENCEVILLE AIR COM.
RAMSEY WATER COM.
WATER TEN.
RAMSEY AIR TEN.
RAMSEY AIR COM.
NEWARK LIME & C.CO. WATER TEN.
NEWARK LIME & C.CO. WATER COM.
NEWARK LIME & C.CO. AIR TEN.
NEWARK LIME & C. CO. AIR COM.
N.Y. & ROSENDALE WATER TEN.
N.Y. & ROSENDALE WATER COM.
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N.Y. & ROSENDALE AIR COM.
F.O. NORTON WATER TEN.
F.O. NORTON WATER COM.
F.O. NORTON AIR TEN.
F.O. NORTON AIR COM.
ROUND TOP WATER TEN.
ROUND TOP WATER COM.
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SAYLORS PORT. WATER TEN.
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COOLIDGE PORT. AIR COM.
SAYLORS EXCELSIOR WATER TEN.
SAYLORS EXCELSIOR AIR TEN.
SAYLORS EXCELSIOR WATER COM.
SAYLORS EXCELSIOR AIR COM.

TABLE 17.

Age.	Cement A. Tension in lbs. per sq. in.		Cement B. Tension in lbs. per sq. in.	
	Air.	Water.	Air.	Water.
1 hour...	45	38	49	32
2 " ...	52	44	53	45
3 " ...	60	55	71	61
4 " ...	63	57	75	62
5 " ...	71	64	79	67
6 " ...	77	65	85	71
7 " ...	82	65	87	74
8 " ...	86	78	86	76
9 " ...	87	69	85	75
10 " ...	88	63	89	74
15 " ...	94	74	97	90
20 " ...	94	77	98	96
24 " ...	88	70	95	90
2 days...	81	72	89	86
3 " ...	91	72	91	59
4 " ...	125	69	117	68
5 " ...	155	71	115	72
6 " ...	149	83	149	83
7 " ...	103	71	111	62
14 " ...	117	94	183	86
21 " ...	148	129	149	113
1 month.	192	205	188	185
2 " .	202	199	216	220
3 " .	209	216	196	187
4 " .	201	217	140	148
5 " .	243	242	196	186
6 " .	182	180	232	212
7 " .	186	296	361	210
8 " .	319	236	311	235
9 " .	225	216	209	199
10 " .	264	226	177	209
11 " .	280	245	178	187
12 " .	255	244	190	149
13 " .	263	226	174	142
14 " .	178	249	184	191
15 " .	226	213	144	176
16 " .	248	216	292	240
17 " .			203	190

preceding tests, and treated in the same way in every respect, excepting that they were tested at intervals of from 1 hour to 17 months.

PROOF TESTS.

The testing machine is frequently employed to make proof tests upon parts of structures to ascertain whether there may be any concealed defects either in quality of materials or in character of the workmanship. For example, in making bridge columns, eye-bars, chains and cables and the like, it is customary to test each article after the manufacture is completed by placing it in a testing machine and subjecting it to a stress which is slightly in excess of that which it is calculated to meet in the structure. Nearly all eye-bars of the best bridges are tested in this way, being subjected to a strain of about 15,000 pounds to the square inch. This is largely expected to exceed the stress to which they will be subjected in the structure, but at the same time it is not intended to quite reach the elastic limit of the bar itself, and so no injury can result therefrom. Whereas, should the bar be defective in any way, either from concealed flaws, welds, or poor material, this proof stress will make the defect sufficiently obvious to cause the rejection of the piece. In many cases, however, it is doubtless that these proof tests have been too severe, for if the piece in question be overstrained so as to injure any of the fibers, or cause an incipient crystallization, the test instead of insuring safety insures the commencement of destruction.

In making proof tests the action of the piece subjected to the stress should be very carefully noted, especially to see whether there is the slightest symptom of deformation, causing, after the piece is taken from the testing machine, a permanent set in the material, signs of flaws, cracks, or other imperfections, should be carefully looked after, and any indication of weakness of any kind under proof test should be considered conclusive either for the rejection of the piece absolutely or for the continuance of further experimenting thereon.

TESTS TO DETERMINE THE VALUE OF DIFFERENT METHODS OF CONSTRUCTION.

Engineers are very rapidly ascertaining the value of the testing machine in demonstrating the best forms and the best methods to be employed in construction. To accomplish this it is of course necessary to make tests on the full-sized specimens exactly as they are to be used in the structure in question. While many experiments have been on such forms as eye-bars, columns, small plate girders and trusses, riveted joints and the like, yet the art is in its very earliest stage of infancy, so that it is almost impossible to give any rules for the guidance of experimenters in this direction, excepting the very general ones, that the pieces should be tested in a manner precisely analagous to that which they will be called upon to

resist in the structure itself, and that all circumstances during the progress of the experiment should be noted with the most minute and scrupulous attention, so that an after consideration of all of the facts thus obtained, may lead to the development of the most perfect knowledge.

The sad and disastrous failures that have occurred in various structures throughout the country during the past history of American engineering has demonstrated too completely the lack of correspondence between the physical properties of materials, as given from the data supplied by us: all test specimens and properties which have actually been developed in actual practice.

The importance, therefore, of making experiments on full-sized members is being more fully and completely realized day by day, and cannot be too strongly urged upon architects and engineers, so that it is hoped before long our country may be supplied with the requisite apparatus for testing any structures, be they small or large, in such a manner and on such a scale as to demonstrate to the world at large that America stands foremost among nations because she is foremost in her structures.

RECENT PROGRESS IN DYNAMO-ELECTRIC MACHINES.

By Professor SILVANUS P. THOMPSON, B.A., D.Sc., M.S.T.E., University College, Bristol.

From the "Journal of the Society of Arts."

II.

Another point in which theory has for long been ahead of practice, is in the advantage to be gained by working as nearly as possible with closed magnetic circuits; that is to say, with a nearly continuous circuit of iron to conduct the lines of magnetic force round into themselves in closed curves. The enormous importance of this was pointed out so far back as 1878 by Lord Elphinstone and Mr. C. W. Vincent, whose dynamo embodies their idea. Every electrician knows that if a current of electricity has to pass through a circuit, part of which consists of copper, and part of liquids—such as the acid in a battery, or the solution in an electrolytic cell—the resistance of the liquid is, as a rule, much more serious than the resistance of the copper. Even with dilute sulphuric acid the resistance to the flow of the current by a thin stratum is 200,000 times as great as would be offered by an equally thick stratum of copper. And in the analogous case of using a field-magnet to magnetize the iron core of an armature, the stratum of air,—or, it may be, of copper wire—in between the two pieces of iron, offers what we may term a relatively enormous resistance to the magnetic induction. If we take the magnetic permeability of iron as 1, then the permeability of air is something like $\frac{1}{20000}$, and that of copper is not very different. Or, in other words, a stratum of air or copper offers about 20,000 times as much resistance to the magnetic induction as if the space was filled with soft iron. Obviously, then, it would be a gain to diminish as much as possible the gaps between the portions of iron in the circuit. The values of the magnetic permeability for iron, air, and copper, have been known for years, yet this simple deduction from theory has been set at defiance in the vast majority of cases. We have had, a few minutes ago, an experimental proof that the Pacinotti ring, so far from having been "perfected" or "improved" by Gramme, as some very high authorities say, is vastly inferior to it. It will perhaps be intelligible now why Pacinotti's design was essentially right.

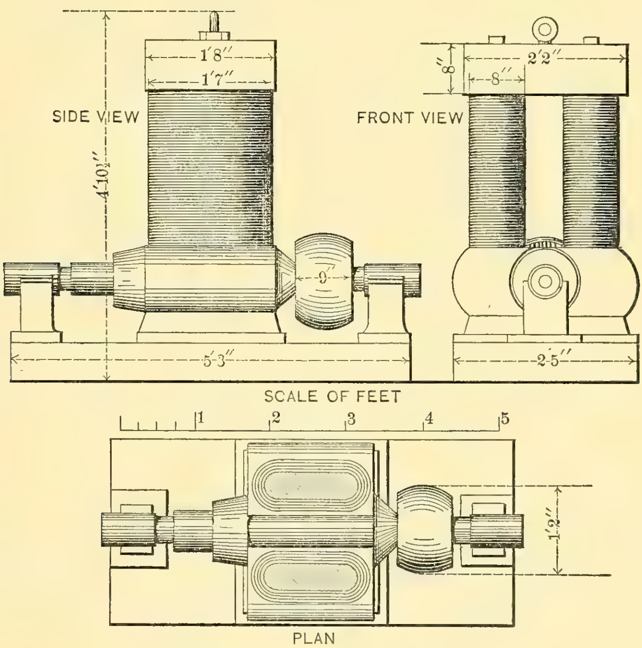
I now pass on to the progress recently made in the practical construction of dynamo-electric machines. Thanks to the kindness of several of those by whom this progress has been achieved, I am able to put before you their very latest results.

The Edison dynamo has, during the past eighteen months, received very material improvements at the hands of Dr. Hopkinson, F. R. S. Some of these im-

provements relate to the field-magnet; others to the armature. Dr. Hopkinson has, in the first place, abolished the use of the multiple field-magnets, which in the Edison "L," "K," and "E," machines were united to common pole-pieces, and instead of using two, three, or more round pillars of iron, each separately wound, he puts an equal mass of iron into one single solid piece of much greater area of cross-section and somewhat shorter length. One such iron mass, usually oval or oblong cross-section, is attached solidly to each pole-piece, and the two are united at the top by a still heavier yoke of iron. The machines have, consequently, a more squat and compact ap-

This wire packs more closely round the iron cores than an ordinary round wire. In the armature the following change has been made. The iron core in the older Edison machines were made of thin iron disks, separated by paper slipped on over a sleeve of *lignum vitæ;* and held together by six longitudinal bolts passing through holes in the core-plates, and secured by nuts to end-plates. These bolts are now removed, and the plates are held together by great washers, running upon screws cut on the axle of the armature. The size of the central hole in the plates has been diminished, thus getting into the interior more iron, and providing a greater cross-section for the magnetic induction.

Fig. 17



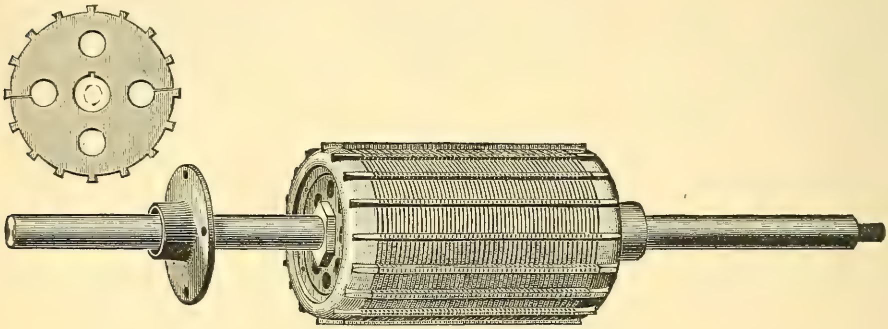
Edison-Hopkinson Dynamo.

pearance than before (Fig. 17). It may be remarked, in passing, that the use of multiple pillars of iron used by Edison in the "L," "K," and "E" machines must have been prejudicial, because the currents in those portions of the coils which passed between two adjacent iron pillars must have been opposing each other's magnetizing effect. Dr. Hopkinson has also introduced the improvement of winding the magnets with a copper wire of square section, wrapped in insulated tape.

By these improvements, a machine occupying the same ground space, and of about the same weight as one of the older "L," 150-light machines, is able to supply 250 lights, the efficiency being at the same time improved. In the new 250-light machine, the diameter of the armature is 10 inches; its resistance, cold, is 0.02 ohm; that of the magnet is 17 ohms. The characteristic curve of the machine shows that even when doing full duty, the field-magnets are far from being sat-

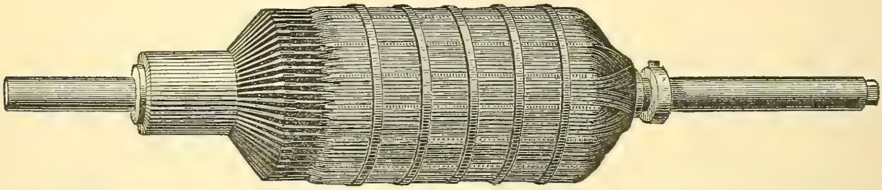
urated. It will be remarked that, in the older construction, the bolts and their attached end-plates furnish a circuit in which idle currents were constantly running wastefully round, with consequent heating and loss. An Edison 60-light "Z" machine of the older pattern, tested by the committee of the Munich Exhibition, was found to give an efficiency which, if measured by the ratio of external electric work to total electric work, exceeded 87 per cent.; but its commercial efficiency—the ratio of external electric work to the mechanical energy imparted at the belt—was only, at the most, 58.7 per cent. In a recent test made by Mr. Sprague, at Manchester, on an improved dynamo (a 200-light machine), the efficiency of electrical conversion exceeded 94 per cent., and the commercial efficiency 85 per cent.

FIG. 18.



CORE OF WESTON ARMATURE.

FIG. 19.



WESTON ARMATURE.

The Edison Company states that "the weight and cost of the machines per lamp are greatly reduced," but they add a table from which it appears that the old 250-light machine cost £250, while the new 250-light machine costs £265, if made as a fast-speed machine, and £425, if constructed as a slow-speed machine.

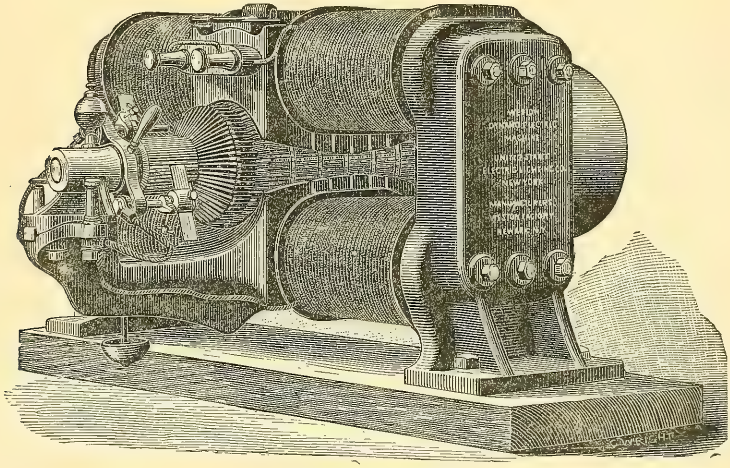
The Siemens' machines have not been much altered during the past year; and it is a little difficult to describe the improvements which have been made, as the firm of Siemens Bros. decline, for commercial reasons, to furnish data for publication. Progress has, however, been made by this firm, especially with their compound-wound machines, of which some account has been given by Herr E. Richter, in the *Electrotechnische Zeitschrift.* It appears that three methods of combination have been tried. The shunt and series coils have been wound on different arms of the magnets; they have been wound on separate short frames, and slipped on to the cores side by side, and they have been also wound over one another. In the latest machines the series coils are wound outside the shunt windings. The regulation, judging by the curves given by Herr Richter, is not perfect. The best regulation was from a "*g* D 17" machine, of which two of the magnet limbs were wound with shunt coils of 29 layers of a 1 millimeter wire, and the other two with two layers of a 3.5 millimeter wire. The potential varied from 64 to 69 volts when the number of lamps was reduced from 20 to 9.

The Weston machine has an armature

more or less resembling those of the Siemens' machines. The core is built up of disks of iron of the form shown in Fig. 18, strung together, and presenting projecting teeth all along the surface of the cylinder. Fig. 19 shows the armature when completed. In this machine, as is also evident from Fig. 20, the pole-pieces are laminated to obviate eddy currents and heating. Recently, Mr. Weston has adopted a method of winding the armature with two circuits, so that an accident to one section shall not completely break down the machine. The latest of the Weston machines show substantial design, and many improvements in detail upon the older forms.

The machines of the Gramme type next come in for consideration. In those of the actual Gramme pattern I cannot learn that any important improvement has been made in this country; but in the States the Fuller Electrical Company, which holds the Gramme and Wood patents, has brought out several improved forms of machine, in which mechanical engineering skill of a high order is apparent. The field-magnets, frames and pole-pieces are very substantial, the ring is better built than the European types, and the collector bars are prevented from flying to pieces by the addition of an insulated ring shrunk on over their ends. In France, too, the machine has received important modifications at the hands of M. Marcel Deprez. M. Deprez's dynamo has two Gramme rings upon one axle, which lies between the poles of two opposing field-magnets, each of the two branched, or so-called horse-shoe form. These are laid horizontally, so that the north pole of one is opposite the south pole of the other, and *vice versa*, the poles being provided with curved pole-pieces between which the rings revolve. M. Deprez, who has given much attention to the question how to design a machine which, with the least expenditure of electric energy, gives the greatest actual couple at the axle, is of opinion that the horse-shoe form of electro-magnet is the most advantageous. The iron cores and yokes of the field magnets are very substantial, but the pole-pieces are not very heavy. M. Deprez's machine has a very elaborate system of sectional windings of the field magnets and a switch board, enabling him to couple up the connections in various ways. The circuits of the two rings are quite distinct, and each armature has its own collector and brushes. M. Deprez has also constructed other Gramme machines, with armatures of very fine wire, for his experiments on the electric transmission of power.

Fig. 20.



Weston Dynamo.

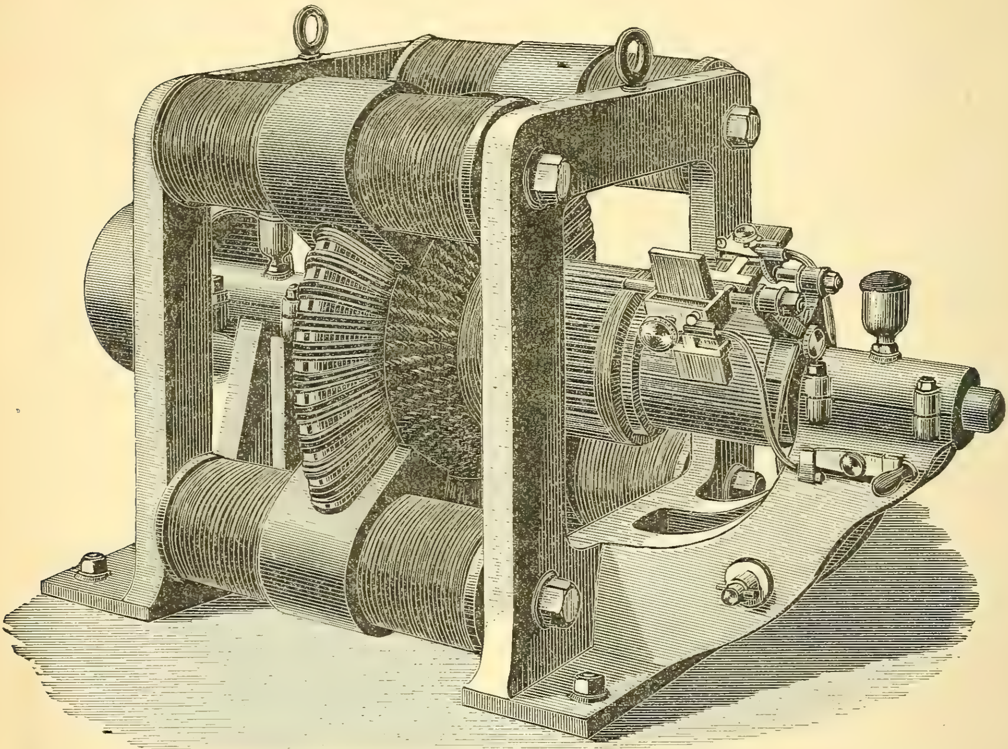
Another machine, having as an armature an elongated ring, somewhat like that of the Maxim dynamo, was shown during last autumn at the Fisheries Exhibition, ander the name of the Hockhausen dynamo. The field-magnets of this machine are very strangely disposed,

the ring being placed between two straight electro-magnets placed vertically over one another, the upper magnet being held in its place by curved flanking pieces of iron, which run down the two sides of the machine, and connect the topmost point of the upper magnet with the lowest part of the lower. This arrangement, which strikes the eye as being both mechanically and magnetically bad, is claimed as one of the virtues of the machine, which, in spite of its magnets, appears to be a very good working machine. Its armature is constructed of four separate curved iron frames, upon which the previously wound coils are slipped, and which are then bolted together and secured to strong end plates. I have not seen any report on the efficiency of this machine.

We next come to the class of machines in which a flattened Gramme ring is used, and of which the machines of Fein, Schuckert and Gülcher are the best known types.

Mr. Gülcher has been steadily at work improving his dynamo in its various mechanical and electrical details. In particular, he has devoted attention to the winding of the field-magnets, so as to secure a constant potential at the terminals. After experimenting with various methods of compounding, he finds that the best results are arrived at in the following way: In his four-pole dynamo there are eight cores to be wound. Each of these receives a shunt coil of fine wire, and outside this a main coil of stout wire. The eight fine wire coils are then joined up in series with one another, and connected as a shunt to the terminals; whilst the eight main circuit coils are joined up in parallel. In proof of the degree of accuracy attained by this method Mr. Gülcher has given me many numerical data from actual tests. All of them show a very fair approximation to a constant potential, and an actually-attained constancy for a considerable range. For example, a 4-pole machine, intended to give

Fig. 21.



Victoria (Schuckert-Mordey) Dynamo (4-Pole).

65 volts, gave that figure exactly, when the external current varied from 30 to 88 amperes; and gave 64 volts at 63.5 amperes, 63.5 volts at 130 amperes. With one ampere only, the potential was 61.5 volts. Mr. Gülcher adds that, in spite of all possible care in manufacture, very large machines do not give results as satisfactory as those given by machines of somewhat smaller dimensions, though the machines are of identical type, and their parts calculated from the same formulæ. He thinks this, to indicate that to obtain the same ratio of out-put and efficiency to weight, there ought to be a corresponding increase made in the electromotive force of the machine. In other words, the means taken in large machines to keep down the electromotive force to equality with that of the smaller machines are detrimental to the action of the machine.

The Anglo-American Electric Light Corporation has been manufacturing during the past year a dynamo of the flattering type, under the patents of Schuckert and Mordey, to which the not very apt name of the "Victoria" dynamo has been given. By the kindness of Mr. F. Wynne, general manager of the Corporation, and of Mr. Mordey, and Mr. P. Sellon, I have been able to learn a great deal about this machine, and to test personally its capabilities. There are two types of the new Schuckert-Mordey dynamo, one having 4, the other 8 poles arranged round the ring. As mentioned earlier in this paper, Mr. Mordey has given great attention to the form of the pole-pieces. These pole-pieces, in the earlier Schuckert machines, consisted of hollow iron shoes or cases which occupied a large angular breadth along the circumference of the ring. Similar hollow polar extensions are still used in the Gülcher machines (see Fig. 26 of my Cantor Lectures). Mr. Mordey has found my opinion, based upon the diagrams of potential at the collector, to be correct, that these wide-embracing pole-pieces were responsible for false inductions, giving rise to opposing electromotive forces and setting up secondary neutral points at the collectors. He has, therefore, by long extended experiments, arrived at a form of pole-piece which completely obviates these effects. As will be seen from Fig. 21, which represents the 4-pole Victoria dynamo, the pole-pieces, though they embrace the ring through its whole depth, from external to internal periphery, are quite narrow, and do not cover more than 30° of angular breadth of the circumference of the armature. They are of cast iron, and are cast upon the cylindrical cores of soft wrought iron which receive the coils. It may be mentioned that, in the 4-pole Gülcher machines, the wide box-like pole-pieces are also cast on wrought iron cores. The armature of the Victoria dynamo resembles in its structure the Pacinotti rather than the Gramme type. Its core is built up of rings cut from sheet charcoal iron, and Mr. Mordey has taken special pains to ensure that there are no electric circuits made in the bolting together of these cores, each plate being both electrically and magnetically insulated from the adjacent plates. Eddy currents in the core are thus almost entirely obviated, This was far from being the case with some of the earlier machines, in which, as in the Edison machine until Dr. Hopkinson improved it, the bolts holding together the cores constituted an available path for wasteful inductions. The core rings of the Victoria dynamo are toothed, as in the Pacinotti ring, and the wires are wound in the intervening gaps. There is, moreover, ample ventilation in this armature, a point not to be overlooked. Formerly, in a four-pole machine, four brushes were necessary—as in the Gülcher dynamo and the four-pole Gramme. Mr. Mordey has reduced the number to two, by the device, firstly, of connecting together those segments of the armature coils which occupy similar positions with respect to the poles; and, secondly, of connecting together, by metallic connections, those bars of the collector which are at the same potential. In the four-pole machine opposite bars are thus connected. Two brushes only are then necessary, and these are 90° apart. Fig. 22 gives the actual diagram of the potentials at the collector. There being 60 sections in the ring, there will be 15 segments of the collector from the negative brush to the positive. The potential rises steadily from the negative brush, and becomes a maximum at the positive brush, at 90°, whence it again diminishes to zero at 180°. The bars of the collector being

connected, it will be remembered, to those diametrically opposite to them, it follows that the potential will rise from 180° to 270°, precisely as it rose from 0° to 90°, and will again fall to zero in passing from 270° to 0°. If the curve from 0° to 180° were plotted again horizontally, we should clearly see how nearly regular the rise and fall is. If from this curve we were to construct another one, in which the heights of the ordinates should correspond to the tangent of the angle of slope of this potential curve—in other words, if we were to differentiate the curve—we should obtain a second curve—the curve of induction. It would show a positive maximum at about 30°, and a negative maximum at about 120°, where the slope up and slope down are steepest in the potential curve. These maxima of induction are situated very nearly opposite the edges of the pole-pieces, on the side toward which the armature is rotating. Apparently the lines of force of the field are the thickest here. In this displacement of the maximum induction we have, I think, the explanation of the inferiority of the earlier machines with broad polar expansions. In those machines the maximum position of induction was displaced to the very edge of the broad pole-piece, and, therefore, the induction was sudden and irregular. It is a singular result that while in those machines in which the ring armature is extended cylinderwise, there must be wide-embracing pole-pieces, in those in which the ring is flattened into a disk shape, the pole-pieces must on no account be wide.

The Victoria dynamo is self-regulating, having all the eight field-magnet coils doubly wound, with main circuit coils inside, and shunt coils outside. The characteristic of this machine is wonderfully straight. In a "D²" machine, wound for a potential of 60 volts, the following values were obtained. Open circuit, 58 volts; 10 amperes, 58.5 volts; 20 amperes, 59 volts; 60 amperes, 59.7 volts; 90 amperes, 59.9 volts; 120 amperes, 60 volts. It will be seen that for small loads the potential drops a little; but it is under these circumstances that the engine speed usually rises slightly in practice, so that the constancy of the potential between the mains is somewhat better than the figures would show. In actual practice, the regulation is marvelous. I have myself opened the circuit of a Victoria dynamo which at the time was feeding 101 lamps, 100 being at a distance, 1 lamp attached to the terminals of the machine. On detaching the main wire from the terminal, the 100 lamps were suddenly extinguished. The solitary

Fig. 22

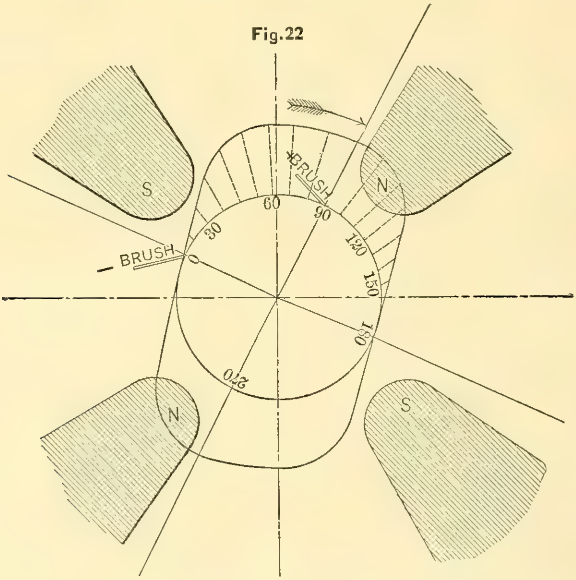
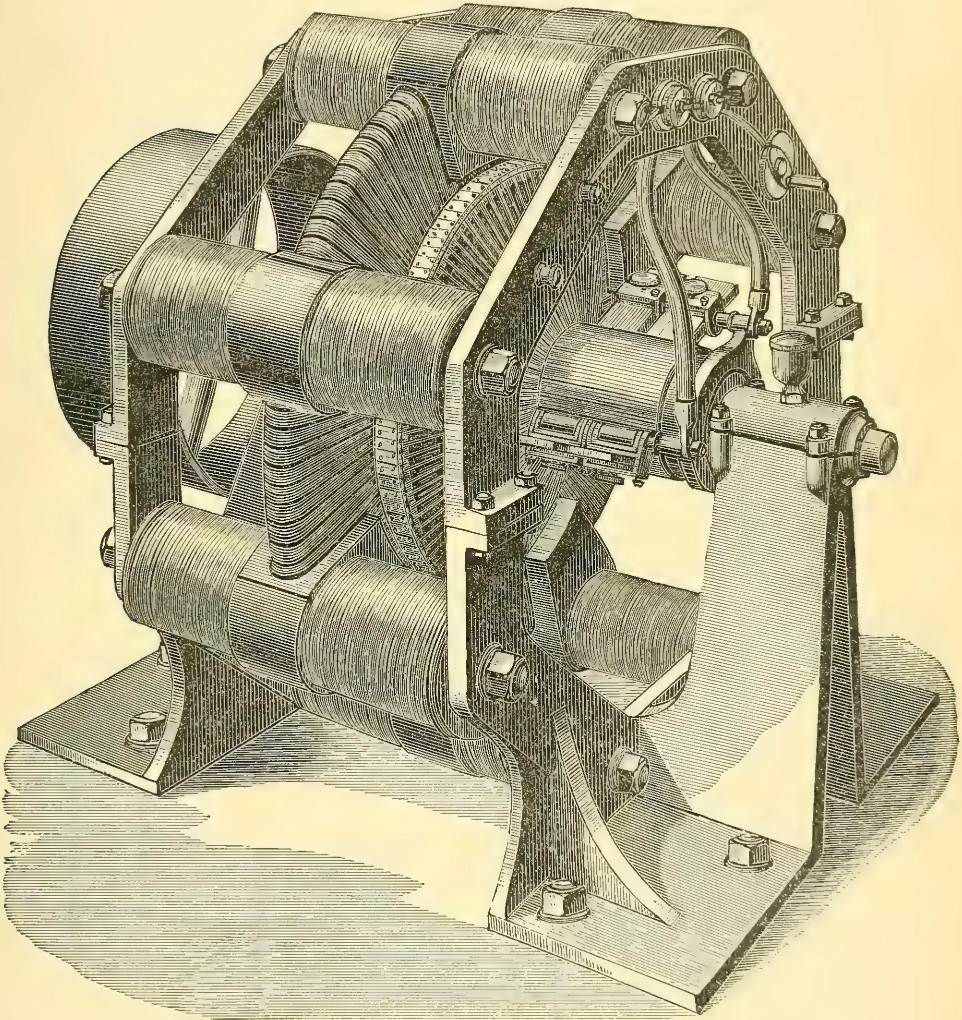


DIAGRAM OF POTENTIALS AT COLLECTOR OF 4-POLE SCHUCKERT-MORDEY DYNAMO.

lamp on the machine did not even wink and there was no flash at the brushes. The sparking was so slight it was impossible to tell whether the machine was an open circuit or whether it was doing full work. The lead was the same under all loads. There are not many dynamos that can show a result of this kind. According to measurements made by the Anglo-American Corporation's electricians, who have published the figures entire, the factor of conversion of this machine is 96.15 per cent., the electrical efficiency 85.68 per cent. These values assume the B. A. unit of resistance as the true ohm, and are, therefore, probably about $1\frac{1}{2}$ per cent. too high. Some of these machines are wound for low speeds for ship lighting. These machines have an electrical activity slightly higher, and an efficiency slightly lower, than the high-speed machines. They also have field-magnet cores slightly heavier, requiring, therefore, the expenditure of rather more electrical energy in maintaining the field. These remarks refer, of course, to a comparison between machines wound to light

FIG. 23.



VICTORIA (SCHUCKERT-MORDEY) DYNAMO (8-POLE).

an equal number of lamps, and to work at an equal electromotive force.

A larger type of Victoria machine, having 8 poles, alternately north and south, set around the ring, has also been constructed by the Anglo-American Corporation. This machine (Fig. 23) illuminates 750 incandescent lamps. The ring has 120 sections, there being 15 sections, therefore, between each pole and the adjacent pole of the surrounding set. As each segment of the collector is connected with those situated at 90°, 180°, and 270° distance around the set, only two brushes are required.

A rather singular commentary upon the real superiority of multipolar dynamos having rings of the flattened type over the more compact ring armatures, to which we have been accustomed in the ordinary Gramme machines, is furnished by the announcement within the past month of a new and improved dynamo designed by M. Gramme himself, in which there is a flat-ring armature rotating within a crown of 12 poles. Elaborate illustrations and a detailed description of this latest of dynamos are given in the *Revue Industrielle*, of January 9th, 1884. From this article it appears that in the opinion of Mr. Gramme the new machine still requires some modifications to make it quite a practical machine. A glance at this drawing is quite sufficient to enable one to hazard a guess at the reasons. The pole-pieces are broad, nearly meeting one another. I should confidently predict from such a design the vice of sparking at the brushes and heating of the collector segments. Moreover, there are no fewer than 24 brushes! Think of the friction of 24 brushes, and the labor of making the complicated holders. It appears that in England we are at least a few steps ahead of France in the matter of designing dynamo machines.

Another 4-pole flat-ring dynamo has been designed by Herr Schuckert, of Nürnberg, and was exhibited at the late Vienna Exhibition. This machine, which had many excellent points in its design, was compound wound, and was calculated to give, at 450 revolutions, a current of 320 amperes at 100 volts.

The present drift toward multipolar dynamos of this type is very significant. There is little difference, save in detail, between the 4-pole machines of Gülcher, Schuckert, and the newer "Schuckert-Mordey" dynamo, albeit these differences are not unimportant. But all these constructors agree in adopting the flat ring. The advantage originally claimed for this construction, namely, that it allows less of the total length of wire to remain "idle" on the inner side of the ring, is rather imaginary than real, for the total resistance of the armature is but a small fraction of the whole resistance of the circuit; and it is possible to spread the field so as to make all parts of the wire active without any gain whatever, if, by this spreading there is no increase on the whole in the total number of lines of force in the field. The real reasons in favor of multipolar flat ring armatures appear to be the following: First, their excellent ventilation; second, their freedom from liability to be injured by the flying out of the coils by the tangential inertia (often miscalled centrifugal force) at high speeds; third, their low resistance, due to the fact that the separate sections are cross-connected either at the brushes or in the ring itself in parallel arc. To these may be added that, with an equal peripheral speed, the armature rotating between four poles undergoes twice as much induction as when rotating between two poles, since it cuts the lines of force twice as many times in the former case as in the latter.

I pass on to the improvements made in the dynamo by Messrs. R. E. Crompton and Co. To describe the course of development which the Bürgin dynamo has undergone in Mr. Crompton's hands would alone occupy a whole evening. The armature of the original machine, as it came from Switzerland, consisted of several rings set side by side on one spindle, these rings being made of iron wire wound upon a square frame, and carrying each four coils. In this form it is described in Professor Adams' Cantor Lectures on "Electric Lighting," in 1881. Mr. Crompton changed the square form to a hexagon having six coils upon it, and increased the number of rings to ten, so that the armature consisted of sixty segments. He then found it advisable to alternate the positions of these instead of placing them in a regular screw order on the spindle, as shown in most of the public drawings of this well-

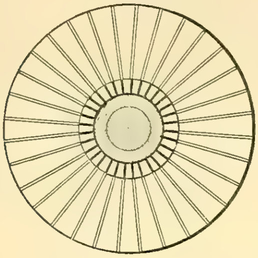
known machine. The next step was to increase the quantity of iron in the hexagonal cores, and to ascertain by experiment what was the best relative proportion of iron and copper to employ. At the same time Mr. Crompton and Mr. Kapp introduced their system of "compounding" the windings of the field-magnets. Another change in the armature followed, the rings being made much broader and fewer in number, four massive hexagonal rings, united to a 24-part collector, replacing the ten slighter rings and their 60-part collector. Quite recently Mr. Crompton and Mr. Kapp have again remodeled the style of armature and have produced a machine which, though it is not yet quite completed, shows what may be done in the way of improvement by careful attention to the best proportions of parts and quality of material. The new dynamo weighs 22 cwts. Its field-magnets are of the very softest Swedish wrought iron, compound wound. The armature is a single ring of the elongated or cylindrical pattern, and its coils are wound upon an iron core made up of toothed discs of very thin, soft iron fixed upon a central spindle, the coil being wound between the teeth as in a Pacinotti ring. In fact, the armature may be described as a kind of cross between those of Weston and Pacinotti, having also something in common in the Bergin armature, at least so I understand, for though I have, by the kindness of Mr. Crompton, been allowed to see the machine, I have had no opportunity as yet of examining the armature. Mr. Crompton's great aim has been to have as complete a magnetic circuit as possible, and that of the best quality. He has sought to increase the intensity of the field by having plenty of iron in the armature, and bringing that iron as close as possible into proximity with the pole-pieces by means of the projecting teeth. The result is an extraordinary increase in the "output," or, as Sir William Thompson puts it, "activity" (*i. e.*, amount of work done per second) of the machine. The machine is only 3 ft. 4 in. long, 12 in. high, and 2 ft. wide. The armature is 17 in. long, and 8 in. diameter. At 1,000 revolutions per minute it gives 110 amperes at 145 volts, or its "activity" is 15,050 watts; but at this speed it heats too much. The power of the field magnets is such that, at all speeds, and under all conditions of the external circuit, the intensity of the field over-masters the magnetizing action of the currents in the armature coils. There is, therefore, hardly any lead at all at the brushes, and what lead there is, is absolutely constant. There is no sparking at the brushes, and it is impossible to tell by looking at the brushes whether the current is off or on. Mr. Crompton is now constructing another machine of the same general design, but larger, to drive 1,000 Swan lamps. This machine, together with its engine, is only about 8 ft. long, 6 ft. high, 2 ft. 4 in. wide, and complete with its bed-plate, will weigh only about 8 tons.

It may be mentioned that in Messrs. Crompton's compound dynamos, as also in those of the Anglo-American Corporation, the series coils are wound direct upon the iron cores, and the shunt coils outside them, thus reversing the practice adopted by Messrs. Siemens and by Mr. Gülcher. It might have been expected that theory would have something to say in determining which practice is preferable. If the shunt coils of thin wire are outside, the prime cost for an equal magnetizing effect will probably be greater. If the series coils are outside the loss by heating in producing an equal magnetic effect will probably be increased. It might have been expected that, as with galvanometer coils, so with the coils of field-magnets it would be advantageous to get as many of the turns as close as possible to the core, and, therefore, that the thinner wire should be wound on before the thicker. But, on the other hand, it is advisable to keep down the resistance of the series coils, as they will form part of the main circuit, whilst the additional resistance, necessitated by winding in coils of larger diameter, is not altogether a disadvantage in a shunt coil. If this proves to be the right way of regarding the problem, we shall wind the shunt coils outside those that are in series with the main circuit.

Before leaving the subject of ring armatures I should like to refer to a form recently devised by Signor B. Cabella, which I think might be recommended to amateur constructors of dynamos as being easily made. The Figs. 24 and 25 show its general arrangements. The

armature resembles that of the Edison dynamo in being built up of copper strips. These are separately cut out,

Fig.24

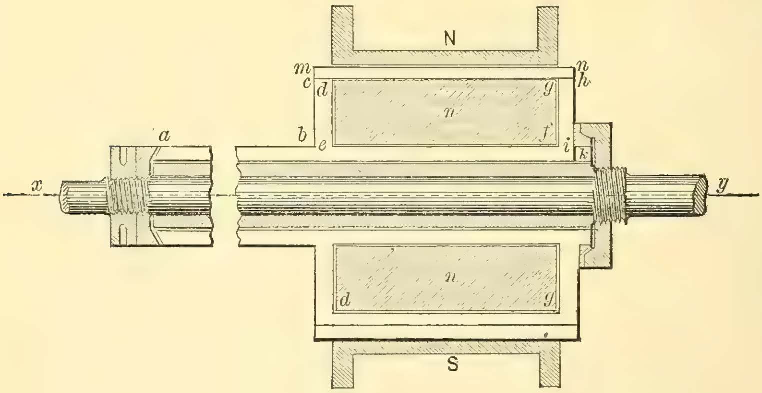


Cabella Armature.

and consist each of a straight piece having two arms, and projecting at right angles. A sleeve of insulating material is placed over the axle, and around this these copper pieces are arranged to the number of some 240 or so, having their arms projecting symmetrically around in two radial sets, one near one end and the other near the other. The channel formed thus between the two sets is lined with insulating material, and then entirely filled up with soft iron wire wound around. Then straight strips of copper, eight millimeters broad and two millimeters thick, are screwed across the outside (like the bars of the Edison armature) from the ends of one set of radial projections to the end of the others, forming the parallelogram section. But, in order to connect the ring all around in a continuous circuit, these external strips of copper are connected at their two ends to pieces which project not from the same internal copper strip but from adjacent strips. Thus an external bar will connect the anterior end of the first strip with the posterior end of the second; and so on. Every third strip is carried along the axle and connected to a segment of the collector. This construction is certainly simpler than that of the Edison armature, and might be adapted to many different types of machines. According to professor Ferrini, one of Cabella's armatures placed between the poles of a 60-light Edison ("Z," old pattern) instead of its ordinary armature, increased its power so that it could be used over 100 lamps.

Passing from ring-armatures, I come to another type of machine having disc armatures. The earliest machine of this type was due to the indefatigable Mr. Edison, who built up his disc of radial bars connected at the outer ends by concentric hoops, and at the inner by plates or washers. Each radial bar communicates with the one opposite to it; and the disc thus built up is rotated between the cheeks or pole-pieces of very powerful field-magnets, which very nearly meet, and which therefore yield an enormously powerful field. I cannot hear of any of these disc dynamos having yet come into practical use.

Fig.25

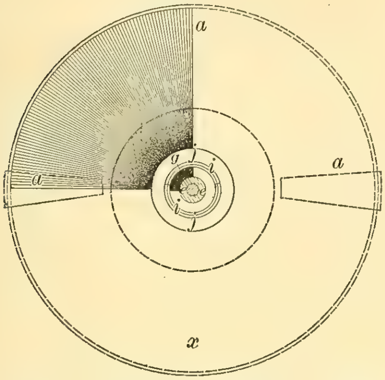


Cabella Armature. Section.

Another type of disc-dynamo has been invented by Sir. W. Thomson. In this case, the armature is a flat wheel, very like a flattened bicycle wheel. It is shown at a in Figs. 26, 27 and 28. The

radial arms or spokes of the wheel, in which the currents are induced, are all connected at their external ends to the

Fig.26



Armature of Sir W. Thomson's Wheel Dynamo.

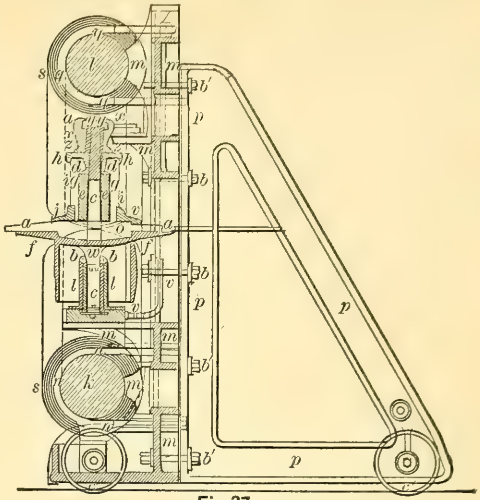
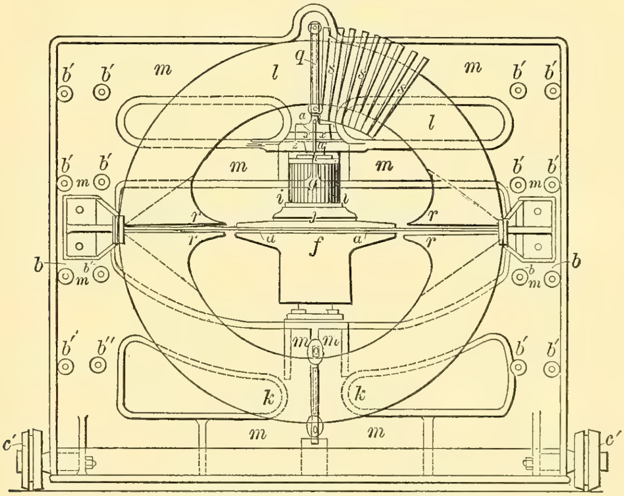


Fig.27

Sir W. Thomson's Wheel Dynamo. Vertical Section.

Fig.28

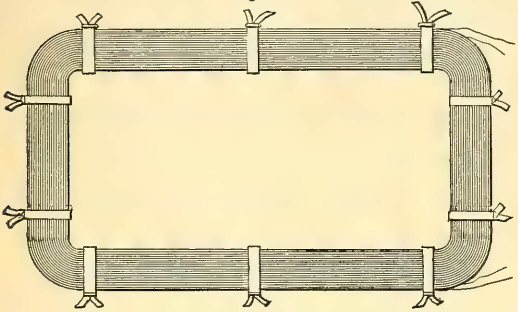


Sir W. Thomson's Wheel Dynamo. Front Elevation.

copper rim, but at their internal ends are carefully insulated and connected each to a segment of a collector or commutator, *g*. As in Edison's disc machine, so also in this, the thin disc rotates between the poles of very powerful field-magnets, which, in the case of Sir W. Thomson's machine, are semicircular in form. Sir. W. Thomson also pivots his armature with its axis vertical, and spins it like one of his gyrostats. Unfortunately, the machine has not shown itself o be in practice a success. Its construcion necessitates a very high speed, else the electromotive force would be small- If the radial bars, instead of being all joined to one rim, were united by overlapping insulated rings, each one to the one next to that diametrically opposite, and a connection brought round again at the hub to the next but one from that at which the outer rim started, then, applying similar connections all round, the radii would all be connected in circuit, and a much higher electromotive force might be obtained. I am not aware that any disc so connected has yet been tried.

Some improvements have also been made in the Elphinstone-Vincent machine. The sections of the armature of this machine are wound separately in parallelogram forms like that shown in Fig. 29, and the separate sections are then fixed upon the periphery of a papier-maché cylinder which is mounted so as to rotate between powerful field-magnets and internal field-magnets whose poles reinforce the field. In the improved ma-

Fig. 29



Parallelogram Coil from the Elphinston-Vincent Armature.

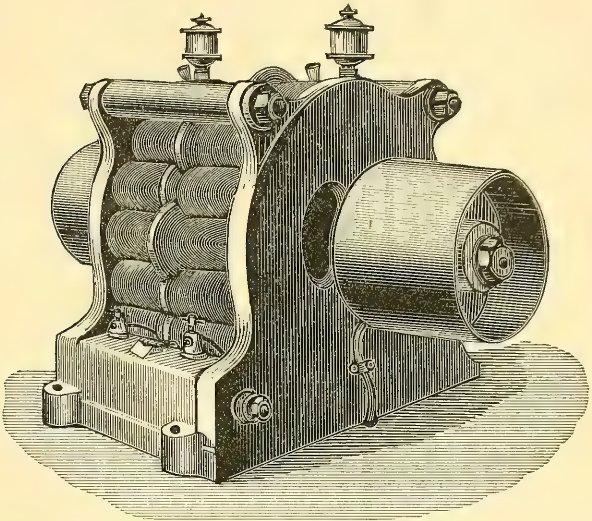
chines the parallelograms of wire are so arranged that the overlapping ends lie outside the ends of the polar surfaces of the field-magnets, which, therefore, can be brought very close to the surface of the rotating cylinder. Amongst other improvements, also, segments of the collector are internally cross-connected, so that only two brushes are needed instead of six as formerly. Several improvements in mechanical details have also been made.

In alternate current machines something has also been done. The Ferranti-Thomson machine, which, at the date of my Cantor Lectures, had just made its appearance, has been considerably perfected. By the courtesy of Mr. Hammond, I am enabled to show Figs. 30 to 33, illustrative of the "1,000-light" dynamo, and of its working parts. Externally, the machine is scarcely charged at all; the driving pulley being a little larger in proportion. Internally, considerable changes have been made, and in these the hand of the experienced mechanical engineer is apparent. The framework of the machine is now cast in two halves, which are afterwards bolted together. Fig. 31 shows one-half of the carcase of the machine with its projecting circle of magnet cores, C, which receive the field-magnet coils. The armature, originally a single zig-zag piece of copper, has assumed the form shown in Fig. 32, in which it may be seen that the convolutions are multiplied, and are held in their places by bolts through a star-shaped piece of brass which also serves to carry to one of the two collectors the connexion with one of the zig-zag copper strip. There are in fact three complete circuits of copper strips in the armature connected in parallel arc. They begin at three of the alternate four bolts of the star-shaded piece, and folding around one another, they all eventually unite with a second and inner star-shaped piece, which communicates with the second collector. Each strip makes ten turns round the zig-zags, so that there are thirty layers, all well insulated from one another by strips of vulcanized fiber. This armature is 30 in. in diameter, and a little more than $\frac{1}{2}$ in. thick in the upper convolutions, so that the opposite poles of the field-magnets can be brought very close together, and a very powerful field produced. The entire armature weighs only 96 pounds. The most extraordinary part of the machine is, however, the arrangement adopted for conveying the currents to the external circuit. The axle carries on either side of the armature an insulated collector ring of bronze, to which the afore-mentioned star-shaped pieces are respectively connected. Instead of brushes, solid pieces of metal, shown at C′ C′, Fig. 33, are employed to collect the current. These collectors hook on over the collecting rings, and bear against about 180° of the periphery of each ring. They are fixed on universal joints, and held by springs from rotating. Copper strips connect them to the terminals of the machine. The arrangements for lubricating the bearings are extremely perfect. The machine requires a speed of 1,400, and weighs $1\frac{1}{2}$ tons. Mr. Ferranti has also designed a slow-speed dynamo to run at 300 revolutions per minute, and feed 500 lamps.

A very large alternate current machine was shown at the late Electrical Exhibition at Vienna, by Messrs. Ganz, of Buda-Pesth. It was capable of furnishing light for 1,200 Swan lamps (20 candle power

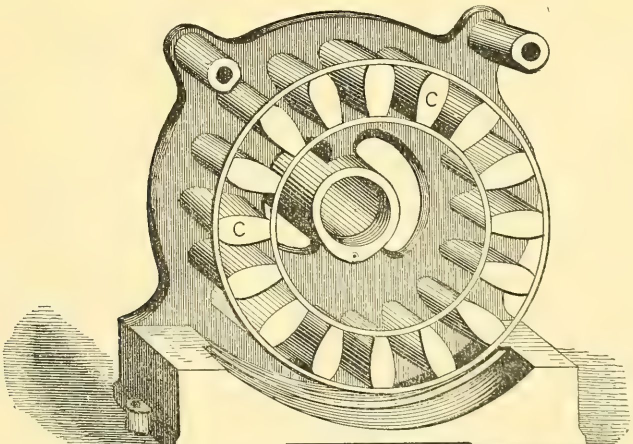
each). This dynamo, which in some points resembled Gordon's well-known machine, was constructed according to the Mechwart-Zippernowsky system. The thirty-six bobbins of the field-magnet were set concentrically on an iron frame, and rotated within an outer circle of thirty-six armature bobbins. The field-magnet coils were, in fact, the fly-wheel of the high-pressure compound engine which drive the machine and its exciter. The diameter of the rotating part was $2\frac{1}{2}$ meters. A salient feature of this machine is the fact that any one of the coils, either of armature or field-magnets, can be removed from the side of the machine, in case such are needed. The whole fly-wheel can, in this way, be taken down by one man in a few minutes. An electrical efficiency of 85 per cent. is claimed for this machine.

Fig. 30.



The Ferranti Thousand-Light Dynamo.

Fig. 31

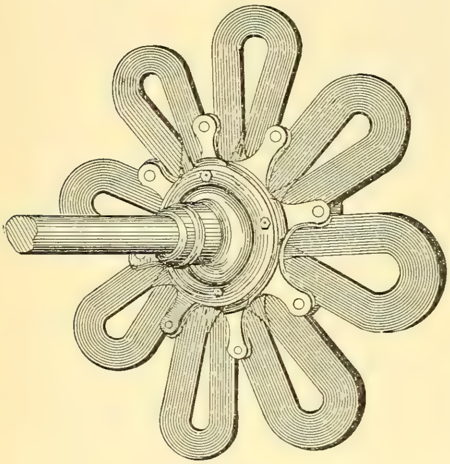


Half-Carcase of Ferranti Dynamo.

Of one other class of machines—the

unipolar dynamo—I had intended to say something. It is a remarkable thing that, though to my knowledge a grea deal of attention has been paid lately to machines of this type, no one has yet succeeded in designing a practical unipolar dynamo. There seems to be some hiatus in the theory of this class of machines, for the very singular fact remains that those which are designed in defiance of precautions to avoid wasteful internal eddy currents will work, though badly, and those designed with such precautions will hardly work at all.

Fig. 32



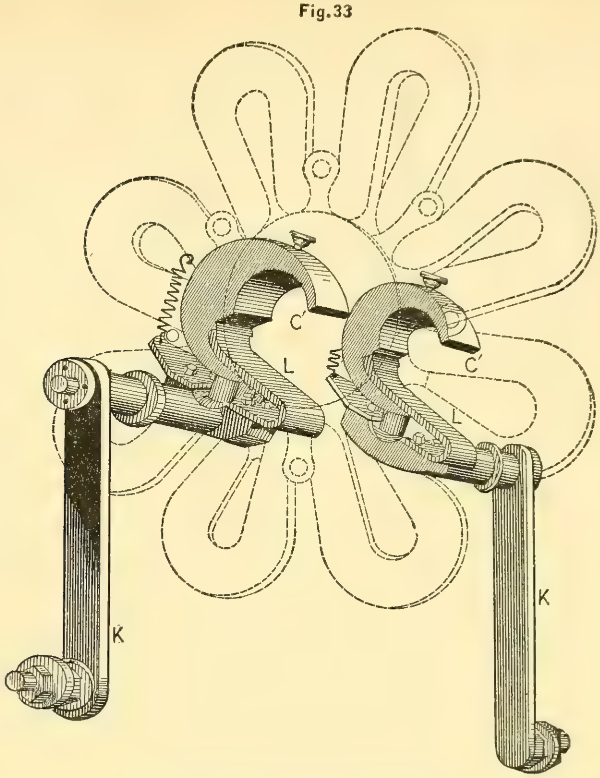
FERRANTI ARMATURE.

There are two or three other new designs for machines which, at present, can hardly be called anything but curiosities. There is, for example, a design for a dynamo (a drawing of which is given in Fig. 34), by Sir Charles Bright, in which the field-magnet coils and armature stand still, but in which the iron cores and the brushes rotate. There is another design by Professor G. Forbes, in which part of the field-magnets rotate. Mr. C. Lever has designed a machine on somewhat similar principles to the foregoing. I have myself essayed an alternate-current machine, in which both armature and field-magnets stand still, while laminated pole-pieces alone revolve. I hear also of a dynamo designed in the States in which there are no field-magnets, only two revolving armatures.

And now I have left myself no time to deal with the third branch of my subject, namely, the dynamo in its functions as a mechanical motor. In this branch also much progress has been made. If time permitted I would speak of the motors designed by Professors Ayrton and Perry, which are successful to a very remarkable degree in yielding a great mechanical power in proportion to their weight. I might have been inclined to say something of the attempts made by the same able electricians to produce a self-governing motor by various devices of centrifugal and periodic governors, and also by using an ingenious differential winding. I might tell you how I have myself worked at the question from a different point of view, and have sought to govern motors so that they shall run at a uniform speed, by devices which will not wait until the speed changes before the act, but by devices depending upon the variations in the load of the machine, in short, upon dynamometric governors instead of centrifugal ones. These things must, however, wait until some more convenient season. Time will only admit of my showing you here, in conclusion, a model designed by Mr. C. Dorman to illustrate the graphic law of efficiency of motors, which I put forward in my Cantor lectures, and which I am happy to learn has since been largely used in many different countries.

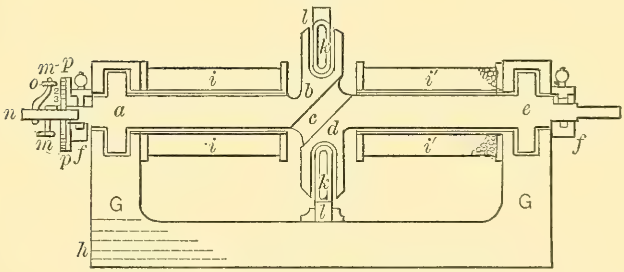
To sum up, then, it may be observed that in every department under review, the story of the past fifteen months is one of solid progress. It has been, it is true, progress of a quiet and perhaps of a commercial rather than a scientific order, yet, as I have shown you, one in which practice and theory have gone hand in hand. It is true that in some few points theory is ahead of practice, but in a still larger number practice is ahead of theory. It would be a great boon to us if our theoreticians could bring up theory to the level of practice in some of the simplest facts. We do not even know the exact law of the saturation of iron in electro-magnets, and content ourselves with formulæ, which we *know* to be incorrect. Of the laws of induction of magnetism in circuits partly consisting of iron, partly of strata of air, or of copper wire, we know very little. We want some new philosopher to do for the magnetic circuit what

Fig. 33



Collectors of Ferranti Dynamo.

Fig. 34



Sir C. Bright's Suggested Dynamo.

Dr. Ohm did for the voltaic circuit fifty years ago. There is ample room for progress yet in theory as well as in practice; and the perfection of theory means the deliverance of practice from arbitrary rules of thumb, and from the blunders of inexperience which have so retarded progress in the past. The history of the past fifteen months, however, gives great encouragement for the future, because it shows how much may be done, even in the face of great commercial depression, by those whose knowledge and experience give them a deliberate faith in the future, and whose efforts are directed toward no uncertain end. A steady development toward the yet far distant goal of perfection is going on unceasingly. The progress of which I am permitted to be the chronicler to-night is progress of the good and substantial kind, that owes nothing to the excited rush of Stock Exchange speculations, and which, not having been nurtured at an unhealthy

fever heat, is destined to be of permanent value.

DISCUSSION.

The Chairman said they were much indebted to Professor Thompson for the extremely clear and able manner in which he had brought forward the progress of dynamo-electric history, and he was very much pleased to find that in that progress he had given the first place to practice, and the second place to theory. He had always found theoretical men rather inclined to look with a certain amount of disdain upon practical men, and to think that practical men knew nothing about their subject unless they followed the dictates of theorists. But a change seemed to be coming over the spirit of the dreams of philosophers, and they were now paying much more attention to the teachings of experience than they used to. Professor Thompson had shown how, during the past 15 months' progress, the lead had been taken by practical men; and he had been rather amused at this, because not much more than a week ago he heard him make similar remarks upon a paper brought before the Royal Society by Professor Hughes, one of the most wonderful experimenters and discoverers of the present day. Professor Hughes succeeded, with pins, bodkins, bonnet wire, match boxes, tin kettles, and similar pieces of apparatus, in extracting from nature some wonderful secrets, and he had brought out some very beautiful instruments, which had been the result of incessant toil and constant experiment in every shape and form. Professor Thompson, at the Royal Society, gave the theory after Professor Hughes had shown his instruments, and at the end of the paper expressed surprise that Professor Hughes' instrument accorded with his theory. This was much what he had told them now, though not quite in the same words, that, after all, experience was the only safe guide in teaching one how to make instruments for real practical work; and theory was a very good servant indeed when it came afterwards, and gave a plain, simple, unvarnished tale, which all could understand, to enable them to understand the peculiar action which had taken place. Professor Thompson had alluded to the theory of the dynamo machine, and divided his theories into three—the physical, the algebraical, and the geometrical; and he was bound to say that his own diagramatical representation of the action of dynamo machines, and of the performance of magnetic fields, were perhaps the most interesting part of his paper. The way in which he had worked out the magnetic field had taught everybody more about this subject than they knew before. But he must say that he objected *in toto* to this application of the term theory to explanations which Professor Thompson had given of the action of the dynamo machines. Theory was neither algebraical, nor geometrical. What Professor Thompson called theory was an explanation—a mode of representing what took place. But a theory was more than that; it was an explanation—a mental picture—of what took place in nature which placed it beyond mere hypothesis. For instance, the theory of light was an explanation of a certain physical action which took place in the universe, which enabled the vibrations of the sun to be represented in our eyes. The theory of heat was something which enabled us to conceive of matter itself vibrating and the space around it also vibrating in unison; but he objected to applying the term theory to either an algebraical or geometrical description of the actions of the dynamo or any other machine. He, and all other electricians, wanted to have some idea conveyed to their minds of what took place in a wire, in iron, and in the space occupied by the iron and the wire, which produced those marvelous results which they saw in the electric light and in the transmission of force, and to this sense he would confine the word theory.

Mr. Liggins said he was not qualified to speak on the scientific aspect of the question, but he was much interested in hearing of the progress which had been made during the last sixteen months. He was glad to find that some of the bubble companies were going to the wall, and when some of the systems which had very little merit were out of the way, there would be a chance for some simple and practical method to come into use. One day last week, at the Edgwareroad Station, he noticed that the electric arc light was wonderfully steady, and the superintendent informed him that an improvement had just been introduced, which reduced the cost from £200 to £8.

That was all he heard about it, for he had not time to inquire further particulars, but he hoped the time was not far distant when the public would derive a benefit from some of these improvements.

Mr. Percy Sellon then gave some particulars of the Victoria machine. He said that taking an original Schuckert machine, capable of giving 60 incandescence lamps of a certain power, by substituting four poles for two, the mass of which, both in iron and copper, was very little in excess of that in the original two, and using identically the same ring, they obtained 100 lights and a much better result in the commutator. From the old Schuckert machine they got a very similar diagram to that which had been shown, among these integrated from Isenbeck's as a bad one with false inductions (Fig. 6), the output of the machine being crowded into two bobbins, and on the other side there was a back electromotive force which caused considerable sparking. This was almost entirely removed by the use of four poles, and at the same time the internal resistance was brought down to about one-fourth what it was before. In constructing large dynamos for 500 lights and upwards, they were increasing the number of poles to 6 and 8, and eventually, probably, they would go to twelve for very large machines. The great advantage of this was that when you brought the poles together you had a given number of lines of force crowded into a very much smaller space; the distances of the centers between north and south polarity were much less, and whilst the number of poles increased, the intensity of the field remained the same, and, therefore, a given length of wire and iron, revolving at a certain speed, would give a decidedly higher result. With regard to the system of compound winding, he should say that a great deal of the credit was due to an *employé* of the firm, Mr. C. Watson; and it was applied without any formula or mathematics, and the very first machine constructed without any experiments gave as good a curve as the one shown on the diagram. It gave a difference of potential of only about 3 per cent. when the whole number of lamps was at work, and when the armature was simply working on the shunt. That showed theory was not always necessary.

Professor Thompson, in reply, said he did not agree with the Chairman's criticism on his use of the word theory. He thought that he had indicated that these three methods were really three aspects of the theory. The number of lines of force which he had been dealing with might be expressed by certain length of line, geometrically, or by the symbol n alphabetically, or when viewed optically, by a mere pictorial demonstration. What some people wrote n for, other people indicated by drawing a line in a certain direction. It was only another way of arguing about the things themselves. He wanted them not to ignore those essential considerations which constituted the true theory, and which underlay the mere facts of observation. They were approximating to that theory by one process or another, sometimes by algebra, sometimes by geometry, sometimes by diagrams, and it made all the difference between working intelligently and unintelligently to have a principle to guide one. He had been trying to discover, both from *a priori* considerations, and from the teachings of experience, what the principles were which ought to guide practice; and he thought he might claim to have vindicated the claims of theory. It was quite true that theoretical men had been sometimes loth to allow any consideration to practical men; but, on the other hand, practical men would often refuse to have anything to do with theory. The vindication he wanted to put forward was this: in his Cantor lectures, and since, he had been working diligently at the curves of potential around the commutator, and to-night he had connected them with the curves of Isenbeck. Part of the improvement he had noticed in the Schückert machine was directly due to the advice he had given Mr. Mordey, and to the world at large, on shaping of the pole-pieces to that form which would give the best result on the commutator in respect to the rise of the curve of potentials. Originally the Schückert machine had great shoes of iron as pole-pieces around it, now they were narrowed and tapered. It was found that where you had pole-pieces on the two sides of an ordinary cylindrical Gramme ring, it was better to have them curved around through a large arc, because it gave a flatter field, and the cutting of the

lines of force was more regular. Now they were finding that practice was leading in an exactly opposite directions for flat-ring machines with the poles at the sides; the shoes were being cut down into little narrow pole-pieces. Theory had led practice to that, and theory, too, had pointed out the relation between the curve of the potential and the induction due to the pole-pieces. After theory had indicated the source of error and a possible means of remedy, they had gone on experimentally cutting down the pole-pieces until they got the proper curve on the commutator. He might further claim, as a vindication of theory, that M. Gramme, who had had fourteen years' experience in constructing Gramme machines, and to whom they owed an enormous debt of gratitude, had not yet arrived at the real point which had been reached in England. M. Gramme confessed that this 12-pole machine of his was not yet perfect, and he (Professor Thompson) had little doubt that the reason was that he had great flat pole-pieces nearly meeting each other. He would stake his opinion on the fact that if he had only made the poles narrower he would have had a better curve of potential, a machine that did not spark so much. That was the deduction he should draw at once from the potential curve. He claimed that it was entirely admissable to call a generalization from which deductions of that kind might be drawn, theory; it was theory in the truest sense of the term, because it pointed the way for practice to follow. He did not require to know what electricity was, or what magnetism was in a piece of iron, or what was the ultimate form of the transfer of force across the space between them, before he used the word theory. He regretted that cold water should have been thrown on his attempt to raise the construction and design of dynamo machines above mere rules of thumb, which were the essence of practice when unenlightened by theory. Practice without theory went on blundering in the dark, and though it was possible to blunder into success, it was much more easy to blunder into failure. With regard to the invention which had been mentioned which would reduce the cost from £200 to £8, he need hardly say he should be much interested in further details; he believed it was the first instance on record in which an inventor dared to claim to have reduced the cost more than 50 per cent., that being the usual reduction claimed in the stock phrase of company promoters.

The Chairman, in proposing a vote of thanks to Professor Thompson, said he did not apply the word "theory" in the sense he objected to. He thought that certain geometrical and algebraical investigations were essential in carrying out any practical question, but he objected to the use of the word "theory" to explain what was really better expressed by the word "principle." Prof. Thompson himself used the word "principle," and had detailed the principles that governed the action of the dynamo machines, and it had been in following these principles that such vast improvements had been made. He should like to have heard some description from Professor Thompson of a class of dynamo machine which was now coming into use daily very largely for the production of copper by the deposit of the metal from its salts. This was being done both at Swansea and at Birmingham. Mr. William Elmore, a hard worker in this field, who had works at Blackfriars, was setting up very large works at Swansea for the extraction of copper from impure ores by this method, and when next Professor Thompson brought the subject before this Society, as he was sure the council would insist upon his doing next year, he hoped that he would pay a little attention to those dynamo machines that were met with in other fields besides the production of electric light and the transmission of power. Again, he was rather disappointed to hear him pass by very lightly the performance of the Hockhausen machine, which was certainly the most distinguished feature of the Fisheries Exhibition. He did not think that that machine had received the attention it deserved, or been studied so carefully by Professor Thompson as others. Its performance was simply wonderful, and though he had not yet seen the Victoria machine, his own impression was that of all those he had seen the Hockhousen was decidedly the best. Perhaps Mr. Crompton's was an exception, for he had this advantage over Prof. Thompson that he had seen the performance of the Cromp-

ton machine. It was certainly a wonderful little thing. You saw before you apparently a lump of iron which was motionless and silent; it appeared motionless because it went so quickly, and it was silent because it was so beautifully made, but with that machine currents of electricity were produced that had increased the output of the form known as the Crompton-Burgin machine exactly 100 per cent. The great lesson to be learnt was this: advance was being constantly made in all these machines, an advance whose future they could scarcely see an end to, and every day they could perceive how, by the combination of theory and practice, they were developing the power of producing electricity to an extent which no one had dreamed of. What with the improvements such as had been mentioned that evening, with improvements in electric lamps and various other appliances, he did not think there was any chance of that end being reached which gas shareholders were so anxious to see.

INTERNAL CORROSION AND SCALE IN STEAM-BOILERS.

By G. SWINBURN KING.

From the "Journal of the Society of Arts."

In presuming to offer any remarks on corrosion and scale in steam boilers, I have no thought of instructing eminent engineers and chemists in a matter concerning which it would better become me to listen to instruction from them. But, profiting from that which they have already taught me, my desire is to call renewed attention to an important subject, and to endeavor, in popular language, to interest the philanthropist and the economist, to whom it especially appeals, and to lay before the owners of steam power, and their superintendent engineers, the results of my researches; pointing out what I believe to be the surest methods of overcoming evils which are known to cause a considerable loss of life and an enormous annual waste of property.

Further discussion and inquiry may result in the discovery of better methods, but, in the meantime, those that have proved effectual cannot be too widely made known.

A former connection with the Admiralty led me to reflect on the importance of this question and to take a great interest in it. Since that period I have made personal investigation into the subject in the great shipping ports of London, Liverpool, Cardiff, Hull, and other places; and in the manufacturing centers, such as Warrington and the neighboring towns.

From an economic point of view, the subject affects the widest interests in this country, the birth place and home of steam power. Besides its relation to the navy and the shipping world generally, it is of vital importance to every owner of factory, mill, or mine; and, as it must ultimately affect the cheapness of production, it should concern every class and every consumer.

Since the days of James Watt, now a century ago, when the giant steam was finally subjugated to the will of man, there has been a constant cry for more power.

In the earliest days of steam as a motive power, boilers were sometimes made of wood, and afterwards of stone; and these primitive materials were followed by the adoption of cast iron, generally in a spherical or hemispherical form, as being best calculated to hold in bonds the expansive force. Copper was found to be inferior in tensile strength, and too expensive for the purpose. Cast iron has now been discarded for wrought iron and steel, and the spherical form of boiler has given way to the tubular or cylindrical, with tubular furnaces and flues running through and within it. The Cornish, the Lancashire, the Galloway, and marine boilers are all constructed after this fashion; and the greatest efforts of our ablest engineers have been directed, not in vain, to construct a boiler strong to

resist, strong to drive, and strong to meet the cry for more power.

But, from the moment one of these structures is mounted into its position, insidious foes are working, which, sooner or later, will compass its destruction; and among the foremost of these must be placed corrosion and incrustation, or scale. These, while constantly reducing the strength of the boiler, appear to be the most common cause of explosion.

The Boiler Explosions Act came into force on the 12th July, 1882, and from that time to November 1883, fifty-seven explosions had been reported to the Board of Trade. This Act, however, has only a limited scope, and does not apply to steamships having certificates from the Board of Trade, nor to H. M. ships, &c., nor to boiler explosions on railways, nor to those investigated under the Mines Regulation Act of 1872. The total number of deaths resulting from these particular catastrophes was thirty-nine, and in addition, forty-six persons were injured. Such fatal explosions—happily rare—as that on board the Royal mail steamer Severn, last November, by which nine persons lost their lives, are excluded from this return.

Mr. Gray, in his report to the President on the explosions occurring in the twelve months ending July, 1883, states that,

"The prevailing cause of explosion is the unsafe condition of the boilers through age, corrosion, wasting, &c.; and [he adds] a noticeable feature in many cases is the absence of any effort on the part of the steam user to ascertain the condition of the boiler, and, consequently, of any attempt to repair defective plates or fittings."

The report of the Registrar-General for 1881, for England and Wales alone, shows that the total number of deaths in that year, directly attributed to boiler explosions, was 51; but, besides these, no fewer than 652 persons were killed by scalding in various and unstated ways; and though it cannot be stated as a fact, it is fair to assume that at least a proportion of these deaths by scalding, from ungiven causes, arose from boiler explosions. These, it must be remembered, are the bare official returns of the killed. Add to them only an equal number of wounded, and it needs no vivid imagination—no affectation of word-painting on my part, to picture to your minds the greater suffering, unrecorded, of mutilated men and women, of bereaved families, and homes made desolate.

If some of these terrible disasters can be traced to their cause, and a remedy found—if life can be saved—as I am fully convinced it can be, this paper will not have been read in vain. Such is the humanitarian side of the question; but the loss of property, arising from decay, is not indicated by the number of explosions, and is far in excess of that which is thus suddenly occasioned. Every steam user knows the heavy annual outlay necessitated by rapid deterioration of boiler plates and tubes, the amount of fuel wasted by reason of scale encrusting the interior, the injury it inflicts on the boiler, and the labor and expense of chipping it out with the scaling hammer.

Mr. Robert Wilson, in his valuable "Treatise on Steam Boilers," says:

"As a rule, steam boilers explode from one cause alone—over pressure of steam." "It often happens," he says, "that boilers are too weak for the pressure they are worked at, and no accumulation of pressure beyond this is requisite to bring about their destruction."

A boiler may be unfit to bear its working pressure from four causes, which he enumerates: (1.) Its original design and strength not being understood by those who fix the pressure; (2), the strength although originally sufficient, having been gradually reduced by wear and tear; (3), by a sudden overtaxing as by unequal contraction; (4), by bad workmanship or material.

I propose to confine my remarks to the second of these causes, and to inquire whether the aggregate of casualties which are attributable to wear and tear cannot be reduced, and whether a remedy cannot be found for the weakening of the various parts of the boiler by corrosion and "pitting," and by the over heating of furnace plates and tubes, consequent upon the formation of lime scale upon them.

Internal corrosion is a trouble from which few boilers entirely escape; but marine boilers are the greater sufferers. Land boilers are subject also to very serious and often rapid decay from external corrosion, but I do not propose to make

more than a passing remark on external decay. The principal causes arise from undue exposure to the weather, unscientific mounting on possibly damp brickwork, leakage consequent upon faults of construction, or negligent management on the part of the engineer in charge. These sources of corrosion are commonly known, and the measures necessary to prevent them are now well understood; although it must be admitted they have been often culpably neglected.

Internal corrosion may be divided into ordinary corroding (or rusting) and pitting.

Ordinary corrosion is sometimes uniform through a large part of the boiler, but it is often found in isolated patches, which have been difficult to account for.

Pitting, which is still more capricious in the location of its attack, may be described as a series of small holes often running into each other, in lines and patches, eaten into the surface of the iron to a depth sometimes reaching a ¼ of an inch. Pitting is the more dangerous form of corrosion, and the peril is increased when its ravages are hidden beneath a coating of scale or fur which may have gathered over it. For without great watchfulness this insidious canker may go on unsuspected until a catastrophe reveals it.

Ordinary corrosion has been commonly accounted for by the presence of acids in the water, but the mysterious ways of pitting have been an enigma to engineers; and although a variety of theories have been advanced to explain its capricious and peculiar methods, none were conclusive until recent scientific investigation discovered the true agency.

It was long suspected that galvanic action, or electricity in some form, had to do with both corrosion and pitting. One theory was that voltaic action was set up between the iron shell and brass tubes, and another that differences in the quality of the iron plates produced the same result. Experiments were made, from time to time, to test these hypotheses, but they seem to have ended, for the most part, in the conclusion that electricity was inoperative either as a cause or a cure. Considered as a cure, it was set aside by some eminent engineers as mere empiricism; but most thoughtful men admitted that the action of electricity, if it really existed, was not understood. New light has since been thrown upon the subject, and altered views now prevail; but I will not anticipate.

There is another form of decay in boilers, known as grooving. This also comes under the head of wear and tear. It may be popularly described as a kind of surface cracking of the iron, caused by its expansion and contraction under the influence of differing temperatures. It is attributable generally to the too great rigidity of the part of the boiler affected, and it may be looked upon as resulting from faults of construction. It is, therefore, outside the scope of this paper, except in so far as it may be, and frequently is, aggravated by internal corrosion, which fastens upon the cracks and eats them more deeply into the iron.

The hard calcareous scale which is deposited by the water on the internal surfaces of the boiler may be taken roughly as identical with the fur which forms on the inside of a tea-kettle. It is composed chiefly of salts of lime, and is known by many names in different districts. I have collected a few specimens for inspection, recently taken from both land and marine boilers, which will give a more definite idea to what it really is. (Specimens produced.) On the whole, it is perhaps a greater enemy than internal corrosion, especially in land boilers, as it brings in its train so many destructive agencies, and involves so many expenses. As of fire it may be said that it is a good servant but a bad master—for a thin covering of about the substance of a coat of paint is found to protect the iron from rust, and is therefore favored by all engineers.

Beyond this point, however, it is an unmitigated evil. In the first place, it necessitates a great waste of fuel, varying according to the thickness and character of the incrustation; but the waste of coal may be fairly put down at an average proportion of not less than 100 per cent. The reason is this, that scale is a very efficient non-conductor of heat, and when it is interposed between the furnace and the water the latter is unable to take up the heat, which, by consequence, goes away unused up the flues. Then again the iron, or other metal, of

the furnace and tubes, being no longer protected by contact with the water, becomes red hot, and is burnt and twisted, to the imminent danger of the boiler, while a heavy expense is incurred for renewing the plates and tubes so affected.

When the scale is thick and hard, the proper examination of the parts beneath it is impossible until it has been entirely removed. Indeed, if it were not removed, the boiler would become unworkable. Scale, therefore, being so great a foe, has to be periodically chipped with hammer and chisel; and the process of chipping is so severe that it tends very greatly to wear out the boiler. The cost of chipping is in itself a heavy item; and it should be borne in mind that a factory boiler must, during the process, perhaps every six or eight weeks, be put out of work for several days at a time.

Scale will stop the feed pipe, which supplies water to the boiler; or hardening over the fusible plug in the furnace crown, which is intended to melt and give warning when the water is dangerously low, will nullify this precaution; and it has thus caused both collapse and explosion.

To dwell on the nature and detail of all the various deposits that afflict steam boilers would occupy too much time, and it is not necessary for my purpose; but, concerning carbonate of lime, which is often a source of danger, I must offer here a few remarks.

This is deposited as a pulverent body; and under certain conditions, chiefly of neglect on the part of the engineer in charge, will form a hard scale similar to that we have been considering, but by proper attention a great deal of it may be got rid of by blowing down, or emptying the boiler to the extent of a few inches day by day, by the scum cock, while it floats near the surface, or by the blow-off cock when it has settled at the bottom. If, however, this floury deposit is allowed to accumulate, and thicken the water, it will produce priming, which may be described as "boiling over," the same process which is apt to take place in boiling a saucepan of milk, or of water thickened with flour. The water is driven with the steam into the machinery, and may knock off the cover of a cylinder, or blow out the bottom of it. The second great danger which it involves is, that lying in a mass upon the furnace plates, it may prevent the steam from rising, and thus the water being lifted on the top of the deposit by the steam held beneath it, the furnace is left without protection, and is liable to be over-heated, and to collapse by the pressure of the steam.

However, these particular dangers may be averted, as already indicated, by due care, without which no scientific appliance is of any avail; and touching this point, I may quote Mr. Michael Reynolds, the author of several excellent engineering works. After inculcating care in various ways, he says, in his practical and significant style: "Any boiler can be made sensitive and hard to manage. Fire it on no system, feed it with water just as the lead plug is in danger, and fill it to the whistle; and your boiler will one day give a big kick." Many a big kick, it may be added, has been occasioned by want of ordinary attention to well-known rules.

The difficult problems that corrosion and scale have presented to engineers and chemists are evinced in the number of patents that have been taken out for chemical compounds to solve them. Hundreds of these compositions have been put into the market, and the number is still increasing; a proof, perhaps, that no panacea has been discovered; although many preparations are still in use by different engineers. These compounds have in truth become so numerous, that every new one is looked upon as another nostrum, and perhaps by the majority of interested persons it is not credited even with the virtues it may really possess.

The chemical laboratory has been ransacked in vain for an absorbent of oxygen that will stop corrosion, or an alkali that can be applied without the risk of causing priming.

With regard to the inutility of boiler compositions, I cannot do better than quote Mr. Hannay of Glasgow, of whose invention I shall have to speak later on:

"Boiler composition [he says] can be classified under two distant heads. First, there are compounds of the nature of precipitants, which are intended to render the incrusting material pulverent instead of coherent. But these do not at all prevent corrosion, and the best compounds only partially prevent incrusta-

tion. The second class, called solvents, are chiefly ammonia salts, which form soluble double salts with lime, and so prevent incrustation. They are, however, very dangerous, as they dissociate under high-pressure steam, and act rapidly on iron, thus increasing the corrosion. Besides, in marine boilers, under great pressure, the presence of even a minute quantity of ammonia salt causes violent 'priming,' that is, sudden ebullition, driving the water of the boiler over into the cylinders of the engine, and sometimes causing the fracture of the cylinder or piston rod. Experiments were made with nearly every possible form of chemical boiler composition, and they were all found wanting."

If further condemnation is required, it will be found in a report of Mr. Lavington Fletcher, of the Manchester Steam Users Association, an engineer of acknowledged and leading authority in such matters. He is reported as saying:

"The number of anti-incrustation compositions was very numerous. Their component parts were veiled in mystery. Many of them proved injurious to the boilers on actual trial. Some lined the plates with a glutinous coating which, while it had the desired effect of keeping off the scale, unfortunately at the same time kept off the water, in consequence of which the furnace crowns became over-heated, strained, and bulged out of shape. The members, therefore, were warned not to adopt any of these boiler compositions without the greatest caution. As the incrustation compositions were costly, blowing out was too often given up when they were used. The practice of neglecting blowing out was strongly objected to, and an explosion that occurred at Bury from that cause was referred to as an illustration."

Mr. J. A. Rowe, an able engineer and officer of the Board of Trade, observes with regard to compositions, that some of them may be useful to prevent the formation of hard deposit, but the "objection to the very best of them is that their acids tend to injure the boiler. . . The majority of them have passed into oblivion, and those that survive seem doomed."

The superintendent engineer of a Bristol company told me of an instance in which, after careful trial, he had adopted a compound which was strongly recommended. It was used in the boiler of a ship on a voyage to America, and it answered its purpose admirably; but, on the return voyage, the engineer in charge put too much of the fluid into the boiler, and the feed-pipe was consequently stopped. The openings into the water gauges being also stopped, they continued to indicate a sufficiency of water until it had fallen to the level of the furnace crowns. The consequence was, the furnaces burned and collapsed, and the ship was seriously disabled on the high seas.

Some compounds cure one part of the evil, and do not touch others, while some again are extremely dangerous to use in a boiler under steam pressure.

Among the many inquiries directed upon the general subject, some of the most exhaustive and minute have been those instituted by the Admiralty, extending from 1874 to 1880. They were carried out by committees appointed to inquire into the causes of decay in the boilers of H. M. ships. The committees were invested with very abundant powers, and were directed to propose measures tending to increase the durability of boilers.

The results of their labors are contained in very able reports, full of valuable information and practical suggestion; but for the purpose of the present inquiry, they may be briefly summarized as follows:

1. With regard to a prevailing belief that the presence of particles of copper in a boiler was a source of injury, they state, first, that the quantity carried into the boiler is extremely small; and, second, that no injurious effect of importance can be produced by it.

2. That fatty acids resulting from the use of vegetable and animal oils for lubrication were a source of injury, and they recommend the use of mineral oils.

3. Moist air, or water containing air, are powerful corrosive agents. They recommend increased density in the water, especially in boilers fed from surface condensers, and that the boiler should be emptied as seldom as possible.

The main conclusion, however, at which the committee arrived—the great prin-

ciple that they asserted and demonstrated—was that galvanic action, induced by the contact of zinc with the iron of the boiler, was the best and only trustworthy remedy for corrosion; and that, so long as the metallic contact was maintained, little or no corrosion would go on.

They adopted a plan of hanging slabs or plates of zinc by iron straps from the stays or rods within the boiler, the zinc being held in a clip in which it was tightly bolted. The theory was perfect, but the weak point in practice was found to be in keeping up electric contact between the two metals. The zinc and the iron not being metallically connected, but only mechanically pressed together, were liable to be so far separated—by the corroding of the surface of the zinc—that the galvanic current was soon weakened and destroyed.

The committee endeavored to circumvent this difficulty, first, by fixing in each boiler an excessive number of plates, so that (apparently) if electric contact should cease even in many plates, it might chance to be maintained in some; and, secondly, they directed a frequent examination with a view of renewing the contact, and putting in fresh plates in lieu of those destroyed by corrosion.

This system was the best they were able to arrive at, but it could be maintained only at such a cost that, to use the words of the report: "The expense of the zinc necessary for efficient protection is undoubtedly an important element in determining how far it should be adopted." For besides the expense of fitting, examining, and renewing the excessive number of plates already referred to, the committee go on to say that "the actual waste of zinc is much greater than that due to the protection of the boiler; and it becomes important to ascertain whether that waste cannot be avoided."

With great care, however, this system was found to prevail against the inroads of corrosion; and herein was a distinct advance, although it still left the question of incrustation by lime-scale comparatively untouched. Indeed, it is stated that the scale which formed in some of the boilers in which slabs or plates were used became "harder and more adherent," and it was therefore suggested that the zinc should be periodically removed altogether, for a limited time, in order that a slight corrosion forming under the scale might enable it to be separated more readily from the iron.

Under the Admiralty system, then, it must be presumed that the bad effects arising from scale, such as the burning of the iron, the waste of coal, and the injury caused by severe chipping, are still a source of trouble and expense; and such I believe is the case. I have before me a specimen of the scale which formed in two months in one of H. M. steam vessels. It was recently taken from a boiler treated on the Admiralty method, and considered to be in very good order.

The committee's report attributed this hard scale to the action of the excessive quantity of zinc which was found necessary for protection when used in the form of slabs or plates. This, however, is now shown to have been an erroneous conclusion, the real cause being that the galvanic current set up under the Admiralty system has not sufficient intensity. I hope to explain this point a little more fully later on.

Now, having pointed out where the Admiralty method fails, I must give credit to the inventor, *per contra*, for the large amount of success it has, nevertheless, achieved. Experience teaches us that few inventions cannot be improved upon, and Mr. Weston himself, of whom I am about to speak, would be the last to claim perfection for his particular plan of protecting boilers by means of zinc.

Mr. William Weston is the Admiralty chemist at Portsmouth, and was a member of the Boiler Committee, whose collective labors are worthy of all praise. It is due to him, however, to state that the system of protection finally adopted in the British navy was initiated and worked out by him, with indefatigable pains and assiduity; and that his method, whatever may be its shortcomings when viewed in the light of later discovery, has worked so well that near half-a-million of money must have been saved to the navy and the British taxpayer since its introduction.

Before the application of the galvanic principle, it was not an uncommon occurrence for the boilers of H. M. ships to be worn out in one commission, or, at least, to become so unsafe to render their renewal necessary; and, as a case in

point, I may mention the *Bellerophon*, whose boilers had to be renewed, after serving only one commission of three or four years, at a cost of some £30,000.

A man who serves the Queen is content to do his duty, and must often do so without being singled out for praise or reward even for special services; but in this paper I am at liberty to mention the name of Mr. Weston with honor, and to claim for him the personal credit which is his just due.

The mercantile marine has in a great measure followed in the wake of the navy; and zinc is now very largely employed in the fleets of all the large companies. Every possible method of fixing it in the boilers has been adopted, with more or less success. Engineers who understood the principle of its action in forming with the iron a galvanic battery, have sought to secure metallic connection by many mechanical devices, while others, convinced of its efficacy, but not understanding its methods, have even thrown it loose into the boilers, to waste and crumble away to no purpose.

Zinc, indeed, had long been used with the object of depositing any minute particles of copper that might find their way into the boiler. It was useless for this purpose, as the Admiralty inquiry fully proved; but wherever it happened to be connected with the iron, it became protective, by setting up electric action, which would continue for a short time, until oxidization of the zinc had broken the electric contact.

A superintendent engineer who was using zinc in his boilers, fixed to the iron, told me he did not believe in the galvanic theory, while in practice he was profiting by it; and when I pointed out this fact, and asked him to explain on what other principle the zinc could be protective, he was unable to answer me.

Another engineer to an important company told me he had a theory of his own, that what was called corrosion and pitting was nothing of the kind, but was solely the result of friction from the circulation of water in the boiler. On the other hand, the majority of superintendent engineers who have some knowledge of electricity and chemistry, are quick to appreciate the truth of the now ascertained cause, and the scientific remedy.

Among the numerous methods adopted for fixing the plates in boilers, the plan patented by Mr. Phillips (formerly a member of the Admiralty Committee), was considered one of the best. It consists in attaching a plate of zinc to a stud or peg, 3 or 4 inches long, projecting from the shell of the boiler, the plate being screwed on tightly by a nut, to ensure close contact with the iron. This system, however, is liable to failure, like all other modes of mere mechanical attachment, as I shall be able to show; and the only means whereby it can be made at all successful is by introducing an excessive number of plates, at great expense, and by constantly examining, cleaning, and renewing them. I have known as many as thirty-eight plates fixed in each of six boilers in one ship on this system, and as many as fifty-six in one boiler of a ship of war on the Admiralty system.

Now, it often happens that when one mind is moved to investigate in a particlar direction, another, at a distance, is working to the same end; and men unknown to each other are working out the same problem, and arriving at similar conclusions. So it was in the application of zinc to steam boilers. While Mr. Weston was working up, step by step, to his present system, Mr. Hannay, of Glasgow, an electrician and chemist, alone in his laboratory, or watching experiments in steam factory or sea-going ships, was building up fact upon fact, and coming about the same time to the same conclusion in principle. In the application of that principle he made an important advance on other methods.

I will endeavor, as briefly as possible, to narrate his proceedings, and describe his invention. The investigation was commenced for the Allan Line of steamers, from Glasgow, at the request of Messrs. Allan Brothers, who, like all owners of steam power, were deeply interested in the boiler question.

The common theory held at that time was that free oxygen and carbonic acid in the water were the active causes of corrosion, and Mr. Hannay's first experiments were directed to absorb the oxygen by the ordinary methods known to chemists, with the result, however, that corrosion continued. When an alkali was added to absorb the carbonic acid, priming was caused to such an extent as to be danger-

ous to the safety of the machinery. He may be said to have exhausted chemistry in his endeavor to find a means of stopping the decay; but, although he succeeded in removing every trace of free oxygen and carbonic acid, the corrosion still continued after six months of patient trial. He concluded, therefore, that, while free oxygen and carbonic acid might help to corrode the boilers, they were certainly not the chief causes.

It next occurred to him that certain parts of the boiler more highly heated might, for reasons familiar to science, have their surfaces so altered as to cause them to become electro-negative to the colder parts. This view was particularly impressed upon him from the fact that corrosion so often took place along certain well defined lines, as, for instance, along the sides of the fire tube. Sometimes the corrosion was so deep that there was reason to apprehend collapse of the furnace. The cold blast going to feed the fire kept the part where the corrosion was quite cool, while the flames kept the top very hot.

Starting, then, from these facts, he deduced the theory that thermo-electric currents were set up between the colder and hotter parts of the boiler, and that the colder part, forming the positive pole, corroded by the natural law of galvanic action.

To test this theory, experiments were made with a boiler specially constructed to allow it to be heated in sections, and to stand a pressure of 200 lbs. to the square inch. Two iron plates were fixed in the boiler, one near the top, and the other near the bottom, and both were connected with a galvanometer, so that a current of electricity passing from one plate to the other could be detected and measured. The boiler was heated alternately more strongly at the top or the bottom, with the constant result that whenever the temperature rose above the boiling point, as in a steam-boiler, the cooler plate became positive, and wasted away. Thus the theory was lifted into the region of ascertained fact.

Attempts were made to keep the boilers in actual use more uniformly heated, but if corrosion were stopped in some places, it was sure to break out in others.

It appeared, therefore, that the only way to prevent this corrosion was by making the iron all negative by a current stronger than that set up in the iron itself by differences in temperature. The current was estimated and found to be very small; a weak battery was fitted up, and the positive electrode, or wire, passed into the water of the boiler, the negative electrode being soldered to the outside of the boiler.

After six months' trial with this arrangement, it was found that corrosion had entirely ceased.

Two important facts, therefore, were now made clear; natural electric currents, so to speak, caused corosion, and a stronger artificial current could be made to cure it.

The experimental boiler was then again tried with the same arrangement as before, but it was first filled with dilute acid. The current was kept going for three months, when it was found that the interior of the boiler was still quite free from corrosion, the acid having been powerless to injure it.

The actual natural current between the metals being so very small, it was thought that, instead of a battery a simple galvanic couple, formed by a mass of zinc within the boiler, properly connected with the iron, might be sufficient to overcome it.

Now, the theory of zinc in contact with iron preventing corrosion may be illustrated thus:

Take two pieces of metal, one of zinc and one of iron, and immerse them in a solution of water diluted with acid, both will suffer from corrosion; but connect them with a wire, and you make them at once into a galvanic couple. A current of electricity is set up between them—the corrosion is directed entirely upon the zinc, which crumbles away, while the iron is no longer injured. The zinc is the positive, and the iron the negative pole. Now you have only to continue the plate of iron till it extends all round the zinc and encloses it, and you have a perfect illustration of the manner in which an iron boiler, enclosing a block or mass of zinc, is made as a whole into the negative pole of a galvanic couple, and is thenceforward absolutely protected from corrosion. It will also become evident that if the connecting wire be broken, or the contact between the zinc and the iron

made imperfect by the intervention of any foreign matter, the galvanic current will cease, and the iron of the boiler will corrode as well as the zinc—just as the two pieces of metal were seen to corrode before they were joined by a wire.

The theory, then, of the protection of iron by contact with a more electro-positive metal being unassailable, and experiment corroborating it, inquiry was next instituted to discover the cause of failure in zinc as ordinarily employed.

It was found that, as zinc had been previously used, no proper arrangements were made for ensuring a true and lasting metallic contact. To show how this fact was demonstrated, and at what pains it was ascertained, a narration of one of the numerous experiments made will be interesting: A boiler was fitted with rolled zinc plates attached to studs, on Mr. Phillip's principle, as previously described. Every precaution was used to give perfect metallic contact. The stud was filed bright and made slightly conical, the hole in the zinc plate made to fit tightly, and the nut on the stud screwed home so as to drive the plate into thorough contact with the clean iron. An insulated wire was fixed to the plate, and led, through a stuffing box packed with india-rubber. Another wire was soldered to the outside of the stud. When a small battery and a galvanometer were put in circuit, the current passed from the zinc to the stud, and so round again to the battery, proving that the contact was perfect. There were six plates put in on trial altogether, and the result was that, after three days' boiling, two of the plates had become metallically disconnected from the boiler—that is to say, no current could pass through the circuit. After five days, another plate became useless, owing to the same cause. At the end of twelve days, only one plate was in electrical contact with the boiler. The boiler was opened after thirteen days' steaming, and it was found that none of the plates were really used up, but that a layer of oxide of zinc had formed between the plate and the stud, and the zinc was thus rendered useless. It must be remembered that these six plates were all specially fitted, and ought to have acted thoroughly well if anything could. The sixth plate was sent away with the ship again, but only remained active two days. It was eaten through when the boiler was again opened.

By this and other experiments it was proved that no mere mechanical attachment of the zinc will suffice to insure continued maintenance of the galvanic current, because, no matter how closely the zinc is fitted to the stud, or bolted to the iron, the water creeps in between and soon destroys the metallic contact.

It was also found that the use of plates was faulty. If they are cast, they split up and fall to pieces in a few days, and if rolled, they are only about a quarter of an inch in thickness, and they soon dissolve away.

To meet the various defects in the use of zinc plates, Mr. Hannay designed a ball of zinc, with a copper conductor cast through the center of it, the copper being so combined and amalgamated with the zinc at the junction of the two metals as to form brass, and thus no corrosion could form between them to stop the galvanic current. The zinc is well hammered at a certain temperature, insuring long existence in an efficient condition.

This ball of zinc is called an "electrogen"; it is fitted in any convenient part of the boiler by a simple device, and a wire from each end of the copper conductor is soldered firmly to the iron. From this moment the electrogen keeps up an uninterrupted galvanic current, and the whole of the interior of the boiler is absolutely protected from corrosion so long as any of the zinc remains.

It was ascertained, by further experiments, that a very small surface of zinc was sufficient to afford protection for a radius of twenty-five feet from the point of contact, and the spherical form of the zinc was adopted because it would maintain perfect protection with a minimum of waste, the large surface exposed by plates, in proportion to their bulk, being quite unnecessary. Herein, therefore, was the means of avoiding that waste which the Admiralty Committee stated was "much greater than that due to the protection of the boiler," and for which they sought a remedy.

Two electrogens are found in practice sufficient to protect an ordinary "single-ended" marine boiler, in which, by some engineers, forty or fifty plates would have been considered necessary. The electro-

gens will last for about six months, while the plates would probably corrode away in as many weeks.

The advantages that Mr. Hannay claims for his system, as compared with any employment of zinc plates, are that it is less expensive and more effectual, and that the protection it affords does not depend upon a chance contact that may be destroyed at any moment. But a further gain, perhaps even greater than these, is that it does not allow scale to form in a boiler at any time to a much greater thickness than that of an egg shell, or a coat of paint.

The zinc ball, with its perfect contact, generates a current of greater intensity than zinc plates mechanically fitted, and the consequence is that a portion of the water is slowly decomposed, and the hydrogen that is evolved at the negative pole, all over the surface of the iron and underneath the scale, forces off the scale in thin flakes by mechanical action, as soon as it becomes thick enough to be impervious to the hydrogen. In this way the scale is kept forming and reforming, hanging in loose flakes, or falling off as it becomes detached from the iron.

Thus, all the evils attending incrustation, which have been before enumerated, are avoided. Fuel is saved, burning of the iron is prevented, and chipping becomes no longer necessary.

The reason why scale becomes more hard and coherent under the zinc-plate method as used in the navy, is that while the galvanic current sets up acts in retarding corrosion, it has not sufficient intensity to decompose the water and deposit a layer of hydrogen on the iron; so the scale grows on a firm surface, and is not pushed off by gas evolved beneath it.

When zinc is merely pressed against iron, the two metals really touch each other at minute points only, and thus great resistance is introduced. Resistance in this case means that the current is destroyed to a certain extent as electricity, and converted into heat; just as the resistance of the break destroys the motion of a train and converts it into heat. Then the water creeping in between the two metals, and forming a non-conducting oxide between the two surfaces, increases the resistance, and ultimately prevents the passage of the current altogether.

When resistance is prevented, and the full intensity of the current is allowed to pass from the zinc to the iron, and back through the water, hydrogen is slowly accumulated at the iron surface, yielding protection from corrosion, and, at the same time, loosening and throwing off the scale.

The value of any discovery that will prevent the formation of hard scale in land boilers can scarcely be over-rated. These boilers in which fresh water is used do not suffer so much from internal corrosion; but the calcareous scale which forms in them has been always a great source of trouble. Compositions have failed, and zinc plates are ineffectual to remove it.

The electrogen, however, seems to have solved the problem; and, to make it sufficiently active in fresh water, the homeopathic principle is applied of *similia similibus curantur.* A small quantity of salt, which is the active corrosive agent in sea water, is made, not only to cure the disease of corrosion which it actuates, but to stimulate an electric current which entirely disposes of incrustation.

Sea water contains on an average 32 to 38 parts of salt in 1,000. Mr. Hannay's homeopathic dose is a-half an ounce to a gallon, or 4 parts to 1,000; and, as no proportion less than 8 times this amount has any effect on iron, no harm can be done to the boiler, even if it were not protected by the zinc. Into brewers' "tanks" and other boilers, the water from which is used for manufacturing purposes, salt, of course, cannot be admitted; but this difficulty is overcome by a simple device, by which the salt is kept separate from the body of the water.

Land boilers, in many districts, would become quite unworkable through the accumulation of scale, if it were not chipped off every five, six, or eight weeks—of course at considerable expense —the boiler lying idle during the process. With electrogens it has been proved that boilers will work more than twice the usual time without any necessity for opening them, and that then the loose flakes of scale may be cleared out in a short time with a hose and a broom. Meantime, no thick scale being allowed to form, it becomes perfectly harmless;

the coal consumed does its full work, and steam is made more freely.

Engineers who have witnessed the results in several recent trials, have stated their opinion that the discovery will revolutionize the treatment of land boilers.

In conclusion, a further and valuable addition has been made to the marvelous applications of electricity, which have pre-eminently distinguished the last decade of scientific discovery.

But there is no finality in human invention. More light will dawn, and with it new marvels will arise. It may be that, before another decade has run its course, electricity or atmospheric power will have superseded steam, and the huge iron boiler of to-day will be looked upon as the clumsy expedient of an ignorant generation.

Till then, while the evils we have been considering exist, and are potent for the destruction of life and property, inquiry into their nature and origin is both desirable and necessary, and time will not have been wasted in seeking to discover the most effectual remedies.

REPORTS OF ENGINEERING SOCIETIES.

American Society of Civil Engineers, May 7, 1884.—Vice-President Wm. H. Paine in the chair—John Bogart, Secretary.

Ballots were canvassed, and the following candidates were elected:

As members—Willis D. Chapman, Akron, Ohio; John E. Cheney, Boston, Mass.; Arthur DeW. Foote, New York City; George S, Gatchell, Buffalo, N. Y.; Henry L. Marindin, Boston, Mass.; Evelyn P. Roberts, Fort Hamilton, New York Harbor, N. Y.; Jesse W. Walker, Pittsburgh, Pa.

As juniors—Henry Goldmark, New York City; Samuel C. Weiskopf, Milwaukee, Wis.; Herbert A. Young, Toledo, Ohio.

As honorary member—Gen. John Newton, Chief of Engineers U. S. A., Washington, D. C.

A paper by James Christie, M. Am. Society, C. E., on "The Strength and Elasticity of Structural Steel, and its Efficiency in the form of Beams and Struts," was read by the author. He said that the various grades of steel possess such a range of physical properties, that it is impossible to consider the metal as one might treat of iron.

It is customary to denominate the grades of steel by the percentage of carbon they contain. The higher the carbon, the higher the tenacity of the steel and the lower its ductility. Steel whose carbon is below fifteen hundredths per cent. is conventionally known as mild or soft steel. The steels subjected to the tests described in this paper were of two distinct grades—mild and hard; both being products of the Bessemer convertor, the hard steel having thirty-six hundredths per cent. of carbon, and the mild steel twelve hundredths per cent. The tensile tests were made on strips about 24 inches long to which were clamped plates exactly 12 inches apart. The compression tests were made on specimens 12 inches long inserted in a tube and the space between the specimens and the tube filled with fine sand.

The tests on transverse resistance were made on bars of three or four inches diameter, and on solid flanged beams from three to twelve inches deep, all being supported at the ends and loaded in the middle.

Extended tables were then presented of these various tests, and it was stated that the results showed that the elasticity of steel and iron is practically uniform; the steel may stretch less than the iron in tension, but the steel shortens most under compression.

Transversely if there is any practical difference the advantage of stiffness probably belongs to steel, but the elasticity of both metals is so close and uncertain that further experiments may modify the average results here found. The specimens show that the elastic limits for tensile and compressive stress for the different grades of steel are practically equal per unit of section, and the transverse resistance is approximately proportionate to the longitudinal resistance, and that the strength of the material indicated on tensile stress will serve as a comparative measure of the absolute strength of iron, or of either grade of steel; but as the transverse elasticity is practically alike beams of iron or of either grade of steel of the same length and section will deflect alike under equal loads below the elastic limit of iron.

Tables were presented of experiments on flat-ended struts of both mild and hard steel. It was stated that the experiments on direct tension and compression prove that the elastic limits of steel of any particular grade are practically equal per unit of section for either direction of stress. A similar equality is known to obtain with iron. Therefore, for the short struts in which failure results from the effects of direct compression, the tensile resistance of the material will serve as a comparative measure of strut resistance. As struts increase in length the lateral stiffness becomes a factor of increasing importance. The transverse elasticity of steel and iron does not vary much. The tendency will be for struts of steel and iron to approach equality of resistance as the lengths are increased. Mild steel will fall to equality with iron when the ratio of length to least radius of gyration is about 200 to 1. Hard steel would fall to practical equality at the point beyond the bounds of practice.

This paper, and the paper previously presented by Mr. Christie giving experiments on the strength of wrought-iron struts, were then discussed.

Mr. A. P. Boller expressed the opinion that the variations in the compressive resistance of iron shown by these very careful experiments were so great that it was impracticable from them or from any other experiments, so far as had yet been made, to prepare a formula which

would ever give satisfactory results, and that dependence must be placed upon experimental charts which will express extreme values for all sections progressively determined.

Mr. Onward Bates considered that the experiments developed the great importance of placing the center line of pressure coincident with the center of the struts. If this could be done perfectly, a round-ended strut would be as good as a flat-ended one. In actual practice in the construction of bridges the methods of securing the ends of such struts are so various that it is impracticable to make from such experiments a table of safe loads. The only safe practice is that of low unit strains corresponding to the lowest results of recorded tests.

Prof. E. A. Fuerkes considered that the areas of cross sections should be obtained by direct measurement instead of deriving them from the weight and length of the bars, particularly when the specific gravity of the material is not determined. The reason why an accurately-centered straight bar behaved as a flat-ended strut when hinged, is due to the friction developed by pressure on the bearing of the hinge, and the early failure of flat-ended struts was probably due to the want of parallelism between the planes at the extremities, or the one or both of these planes being warped surfaces. Since a bar, very long in proportion to its radius of gyration, fails with a comparatively light load without permanent injury, it would seem proper that such load should be gived a name other than ultimate load, the latter being restricted to its bearing on the elastic limit.

Mr. Theodore Cooper considered the experiments of Mr. Christie most valuable, particularly in carrying out a complete series with different end connections upon the same class of materials. The paper shows that slight changes in the direction of the lines of applied forces produce great changes in the results. By interchanging different sizes of ball and socket joints it shows the influence of the size upon compressive resistance of the struts. It gives a more complete knowledge of the action of struts of high ratios of length to transverse dimensions than before existed. The method of using the least radius of gyration, instead of the least dimension, gives a fair comparison between the various forms. Attention was called to the relation of the ball and sockets to the transverse dimensions of the struts, and diagrams were presented by Mr. Cooper, showing the influence of the size of pins relative to the width of the struts. From the great effects of non-centering the line of applied force upon columns and of initial, though minute, bends in the materials, and the increased influence of possible side blows, it is very important not only to keep the working strains within proper limits, but also to specify a limit to the number of diameters to be used in all columns. In recent specifications this limit has been about at 45 diameters, corresponding approximately to about 120 radii of gyration for the usual forms of bridge columns. With this proviso a practical formula may be reduced to very simple forms.

Mr. E. B. Dorsey presented some comparative tests of iron and steel.

The subject was further discussed by Messrs. Bouscarew, Chas. E. Emery, Pegram, P. Roberts, Jr., Towns and Christie.

ENGINEERS' CLUB OF PHILADELPHIA.—Regular Meeting, May 3.—Mr. S. N. Stewart, Visitor, exhibited a model of his River or Current Motor. Paddles are placed upon cranks and maintained in a vertical position by long floating vanes or tails. The cranks are placed upon posts, rafts or boats in the stream and journaled at the water-line, thus keeping one-half of the paddle surface in action, while the common floating-wheel or current-wheel only keeps one-tenth of its surface in action. In Mr. Stewart's motor a 10 ft. arm carries a paddle 10 ft. high. For particulars concerning this engine, he refers to "Commercial Relations of the U. S.," 1881, p. 682, or New York *Daily Tribune*, June 21, 1881. Mr. Stewart said "the only way to utilize the power of large rivers is by current-motors, for dams are not permitted and are too expensive; that a current of five miles an hour has one hundred times the pressure of wind at ten miles an hour, and that, as the current soon regains its normal velocity, it can be used over and over."

Mr. Thomas M. Cleemann read a paper on an Economical Form of Bridge Truss. In outline it resembles Whipple's arch truss, inverted so as to bring the curved portion in tension, but differing from it in having the chord made to resist tension and being anchored to the abutments. In this way the only parts of the bridge in compression are the vertical posts, and the extra material, required to stiffen the upper chord in an ordinary bridge, is saved. To illustrate the correctness of his conclusions, Mr. Cleemann had a small model made of pink wrapping twine that broke with five and a half pounds, and with posts made of wooden knitting needles. This model, by calculation, ought to have borne about eleven pounds. After loading it with a pound weight at each of the seven panel points and letting it remain for a little time for the inspection of the members, he added first two more pounds at the centre, and afterwards two pounds more. With this eleven pounds the model hesitated a moment and then broke at each abutment, nicely illustrating the author's conclusions. He likewise gave the saving of material in such a bridge over a Pratt truss of five hundred and sixteen feet span, and pointed out its advantages for military purposes, where facility of transportation is a prime object, and a bridge almost entirely of rope is especially valuable on this account.

Mr. Henry C. Roney exhibited a section of four-inch Wooden Water Pipe and Joint, found about two feet below the surface while excavating a trench for the conduit of the Philadelphia Sectional Electric Underground Co., on Chestnut Street, between Fourth and Fifth, and described those which had been found on Chestnut and Market Streets during the progress of the work. These are interesting as showing the durability of such pipes and connections, under the conditions to which they had been subjected.

Mr. W. G. Neilson read some notes on the

recently published Report of Gun Foundry Board. The recommendations embodied in this report, in regard to the establishment of a gun factory at Watervliet Arsenal, West Troy, New York, for the manufacture of guns for the Army, and at Washington Navy yard for the Navy, and that the Government should depend upon the steel manufacturers of the country for its forged steel and should simply finish the guns in its factories, were referred to, as also were the interesting facts colleted by the Board, during their visit to Europe, in regard to the character of steel used for guns.

The secretary exhibited, for Mr. J. H. Harden, a neat Topographical Model of the Jones Iron Ore Mine in Berks Co., Pa., and briefly explained the method by which such models are constructed.

ENGINEERING NOTES.

The Inspection and Testing of Iron Road-Bridges in Germany.—According to a decree recently published by the Minister for Public Works, all iron bridges on public roads are annually to be subjected to rigorous inspection and tests.

The parts to which special attention is to be directed in the inspection are:

1. The girder-beds and the brickwork of the piers and abutments.
2. The bed-plates with regard to normal position, freeing them from rust, &c., and eventually seeing that they are in perfect working order.
3. The riveting at the junctions of bracings, &c., with the booms, especially the existence of loose rivets at points where the greatest strain is borne.
4. The separate parts of the bridge; whether any fractures have occurred at the rivet holes, and whether bending, rust, or want of paint are manifest.

After the inspection of the bridge it will be evident whether any measurements are required to be taken; if so they are to be made with a view of ascertaining—

(*a*) The normal height of the bearing-plates.

(*b*) The height of the center of the bridge, and its camber when unloaded.

(*c*) The amount of oscillation produced by vehicles passing over the bridge.

If, after the above inspection and measurements have been made, the state of the bridge should be doubtful, load-tests are to be resorted to, in order to bring out more prominently any defects which may exist in the structure.

For measuring the deflection a level is generally to be employed, but the use of other means and apparatus in suitable cases is allowed.—*Proceedings Inst. C. E.*

On River Embankments.—The embankments of the Po between Cremona and Casalmagiore were originally placed from 2½ to 3 miles apart, leaving long strips of valuable land between them and the ordinary channel of the river. In many places large tracts of this intervening land have been enclosed by banks, which might be thought to act as outworks to the embankments, and to afford them additional protection. Instead of this, however, they have frequently proved to be sources of danger, as they are more liable to be damaged by floods, and if the river bursts through them the rupture of the embankments frequently follows, the reason being that as these latter always stand dry, except when the banks have failed, they do not get the benefit of the action of the water in filling up small cavities, nor is there any opportunity of discovering defects till a flood comes and it is too late. It is believed that in many instances in which disastrous breaches have been made in the embankments these have been due entirely to the effect of the banks. This danger was illustrated and happily averted in one instance. The embankment of the Po between the Olona and the Lambro protects an area of 14,000 acres, with a population of nine thousand seven hundred. Between this and the river several tracts of land have been surrounded by banks. In October, 1882, one of these banks, enclosing 355 acres, was threatened by a flood, when the engineer in charge ordered two cuts, of about 70 feet each, to be made in it, so as to allow the water to flow through gradually and to reach the embankment slowly. It was then found that defects existed in this embankment, which began to be undermined by water; but as there was then time to remedy them, the country was thus saved from an inundation, which would certainly have taken place had the river forced its way uncontrolled through the bank, as it would then have poured with such force against the principal embankment that it would have been impossible to repair it.

Mr. Pestalozza considers that these banks should either be removed altogether, or else made entirely separate from the embankments, or in some cases, when villages have been built under their protection, should be raised and strengthened so as to act as the principal embankments of the river.

Mr. Rossi, on the other hand, while admitting that these banks are often improperly constructed and become a source of danger, thinks that in most cases they afford real protection, and are, in fact, embankments. He recommends that they should be kept at a lower level than the embankments, the exact height varying in different localities, but being somewhat below the level of ordinary floods, which would then flow over them and give the embankments the benefit of the wash of water which is required to keep them in good order; the damage which the enclosed lands would sustain by occasional flooding would be compensated by the deposit of rich mud left by the water.—*Abstract of Inst. of Civil Engineers.*

IRON AND STEEL NOTES.

On the Burning of Iron and Steel.—Iron that has been raised too near its temperature of fusion and slowly cooled, is designated as "burned" or overheated metal. It is both red-short and cold-short, and exhibits a coarse, crystalline structure, and a bright, glistening fracture. Such iron contains oxygen. But this oxygen is not, as is commonly believed, de-

rived from without during the heating, but it was previously contained in the iron itself through the medium of the scoria or slag-impurities mixed with it. When the iron is raised to the fusing heat, or near it, a chemical reaction takes place; the metallic iron reduces the sesquioxide to protoxide, which, by being dissolved in the iron, alters the properties of the latter. The coarsely crystalline quality of iron so treated is not due to the presence of the oxygen. The metal usually contains a notable quantity of phosphorus, which is well known to give a coarse grain accompanied by the quality described as cold-short. The crystallization takes place during the slow cooling while at rest. The greater the proportion of phosphorus present the lower is the temperature to which the iron may be raised without being burned. Pure iron should not take up more than 0.25 per cent. of oxygen in solution. Though this substance does not greatly affect the ductility of the metal when cold, it acts like sulphur on its malleability.

The qualities of steel also undergo change when heated to a high temperature, or when subjected to a lower temperature for too long a time. The richer the steel is in carbon, the lower is the temperature at which the change take place. Therefore, the harder the steel the more carefully is it to be dealt with in the fire. Such overheated steel becomes coarse grained and brittle; that is, cold-short. If the temperature be increased, showers of sparks are thrown off, and the steel is said to be "burned." The alteration brought about in this way has generally been attributed to a diminution in the proportion of the carbon constituent, though this assumption is not warranted by the results of analysis. The presence of manganese and silicon is of more weighty consequence. When steel containing these is heated it is not the carbon, but the manganese and silicon that first become oxydized, and there results an important change in the properties of the steel. Later the carbon is oxydized; and while the oxide of carbon escapes those of the manganese and silicon remain behind, and the whole molecular structure of the metal is altered. If the heating be carried still farther the iron will next be oxydized. A cast-iron furnace door, exposed for several years to the flame of a coal fire, was found to contain 27.8 per cent. of oxygen, in combination with iron, sulphur, nickel, copper, phosphorus and arsenic. The cause of the sparks is not the combustion of the carbon, and the consequent generation of carbonic oxide gas, but the escape of gases imprisoned in the steel. Similar results may be brought about by exposing the steel to a lower temperature for a longer time; the oxidation of the constituents will, in this case, be effected in the order mentioned above, the only difference being in the slower action. Steel altered in this way is well described as "dead." A regeneration of the metal by mechanical treatment is hardly possible, since the original chemical composition cannot be restored by such means.

RAILWAY NOTES.

The Progress of Canadian Railways.—The official statistics show that 1,275 miles of railway were built in Canada last year, making a total of 8,805 miles under traffic, and when the lines at present under construction are completed, which will be within two years, the railway system of Canada will comprise over 11,400 miles. The paid-up capital was increased to $494,271,264, or 19 per cent. The gross amount of freight carried during the year was 13,266,255 tons, the gross receipts for which were $21,320,208. The gross receipts, together with those for mails and sundries, were $11,924,377, making the gross revenue $33,244,585—an increase in receipts over those of the preceding year of $4,216,796. The net earnings for the year were $8,552,928—an increase of nearly $2,000,000.

Steam Tramways in Burmah.—Reports have reached here of the starting of the steam tramways of Rangoon, owned by Messrs. Darwood & McGregor, of that city. The engines are by Messrs. Merryweather & Sons, and are of the Stockton type, which have successfully run them for upwards of two years. Mr. Henry Bateman, from the works of the builders of these engines, is locomotive superintendent. There are excellent workshops and first-class tools for carrying out the necessary repairs. The report just to hand states that the engines are running continuously, each drawing three large cars crowded with passengers, and commercially it is expected that this will be one of the greatest successes in Rangoon. Some more engines will be built by Messrs. Merryweather for the extensions, also some special double bogie cars of teak with iron frames. We shall shortly illustrate the cars and workshops of this tramway.

Indian Railway Statistics.—The second part of the administration report on the railways of India for 1882–83, recently published in Simla, contains elaborate statistics regarding the lines serving India, and furnishes information which is of great interest now that the railway policy of the Indian government is under discussion. There are four classes of railways in India, viz., nine state railways under the control of the government, thirteen provincial state railways, six native state railways (the most important of which is in the Nizam's territory), four lines belonging to assisted companies, besides six lines controlled by the government of Bombay and two by that of Madras. As regards the Indian government railways, the East Indian and some others pay well, while a few are worked at a dead loss, but, taken all round, they now yield a surplus in aid of the Indian exchequer. Speaking of Indian railways generally—that is, of the whole 10,000 miles of system—they earn about £1,525 gross per mile, about one-third of the earnings of English railways. The working expenses, too, are less than what we might expect, considering the breaks of traffic, and the fact that most of the coal used has to be imported from England. They amount but to 49.94 per cent. of the receipts, against 52.34 per cent. for England and Wales, 49.51 per cent. for Scotland, and 54.88 per cent. for Ireland. The net receipts are sufficient to yield 5.37 per cent. on the capital expended, that is, a shade over the

5 per cent. guarantee. The East Indian yields over 8 per cent., the guaranteed lines 4.94 per cent., and the state lines a little over three per cent. The gauge controversy is opening up a burning topic. There are no less than five railway gauges in India—the 5-foot 6-inch, or broad gauge, the 3-feet 3-inch, or meter gauge, the 4 foot used on the Azimganj Railway, the 2-foot 6-inch gauge of the Gaekwar of Baroda's line, and the 2-foot or military gauge of the Himalayan Railway. Practically the contest now lies between the broad and meter gauges, and as the most important lines have been laid on the former principle, opinion inclines to the view that the broad gauge is the proper one to universally survive. At any rate, what is now desired is uniformity, as the present breaks of gauge and consequent shifting of goods add considerably to the cost of transport.

ORDNANCE AND NAVAL.

The collapse of Shipbuilding.—After attaining last year to the highest point that it has reached in the history of the industry, shipbuilding at the North-Eastern ports has in the first three months of 1884 collapsed. It has been definitely stated that there are now about 7,000 shipbuilders unemployed on the rivers Wear and Tyne. If this statement be even exaggerated, it must be confessed that there is an enormous falling off in the number and the tonnage of vessels in course of construction. On the Tyne and the Wear the number of the vessels on the stocks is only about one-half of those on the stocks at the same date last year; and at the other shipbuilding ports there is also a falling off, though not quite so marked. Again, out of the vessels on the stocks there are some, the progress of which is stopped; and as others are launched their places are not taken up. It is thus clear that there will be an enormous falling off in the tonnage of the vessels built at the North-Eastern ports during the current year, and it is probable that the completeness of the collapse will be one of the reasons that lead to the conclusion that the recovery, if not very rapid, wtll be not so long deferred. The loss of vessels still goes on, and as the work of the steamers had been restricted by the enormous stocks that had accumulated, and as these stocks are now falling off, it may be fairly concluded that the demand for tonnage will recover with more speed than had been thought likely. So complete a collapse as has been witnessed, and is being witnessed, may lead to a revival in the trade at no very distant date.

The Mann Gun.—Mr. H. F. Mann, of Pittsburgh, has been advised by Sir Joseph Whitworth & Co. that the steel tube for his breech-loading rifle has been shipped, and that the steel jacket and other parts that are being made by that company will be forwarded shortly. The tube and other parts are made of the Whitworth fluid-compressed steel of the best quality. The tube is 17 feet long, 6½ inch bore, and weighs about 4,500 lbs. All the parts furnished by the Whitworth Company will weigh some 14,000 lbs., and will be finished complete, ready to be put together by the South Boston Ironworks, of Boston, Mass., which has been the contract with Mr. Mann for completing the gun. The total weight of the weapon will be about 20,000 lbs. Mr. Mann has also a contract with the Ordnance Department for 250 projectiles to weigh 110 lbs. each, to be used in the test of his gun. These projectiles are now being made in Pittsburgh, and will be forwarded to the Government proving grounds at Sandy Hook, New York Harbor, as soon as completed. The charge of powder to be used in testing this gun will be from 30 to 40 lbs.

The Armorplate Trials in Denmark.—The *Army and Navy Gazette* gives full particulars of the experiments carried out on the Island of Amager on March 20 and 21. The four plates were bent on an inside radius of 10 feet 9 inches, each formed a separate target, the English plates being fixed by means of twelve bolts to the backing, that of Marrel Freres by eleven, the Schneider solid steel plate by sixteen bolts. A 15-centimeter Krupp gun, 35 calibres long, and an 18-ton Armstrong muzzle-loading gun were used. The projectiles consisted of 5⅞-inch and 10-inch steel shell of the latest pattern manufactured at Essen, also 5⅞-inch chilled-iron shell and solid 10-inch chilled shot of Swedish manufacture. The latter was only used against the Cammell compound plate. The range was 100 meters. On March 20 the experiments commenced with a round of steel shell at each plate. All the four shells struck the plates full in the center, penetrating the Marrel plate considerably, two large cracks starting from point of impact to the edges of the upper and right side of the plate. The Cammel plate was slightly penetrated, and showed a few surface hair cracks. The Brown plate showed deep cracks running from the point of impact to the left side of the plate. The shell penetrated the Schneider plate to a considerable depth, and cracked it in half from top to bottom through the point of impact. The next experiment was made on the left-hand lower corner of each plate, the 10-inch gun being fired with steel shell. The first shot knocked the whole left side of the Schneider plate off its target. The lower part of the Marrel plate was completely wrecked; the shell, passing through the target, was picked uninjured a long distance in the rear. The English plates broke up the shell as they passed through. The Cammell plate showed a few additional surface cracks; the shell, striking the Browne plate somewhat low down, broke off the corner. On the 21st experiments were resumed; a solid 10-inch chilled-iron shot was fired, with a charge of 63 lbs. of powder, at the Cammell plate. Although the shot did not penetrate the compound plate, the target was flung bodily to the rear. A 5⅞-inch chilled-iron shell was next fired at the Brown plate. This opened up a horizontal crack in the plate through its entire thickness, breaking it into three pieces, a similar shell completely destroying all that remained of the Marrel plate. In these trials 467 lbs. weight of metal were fired at the Sehneider plate, 569 lbs at the Brown and Marrel plates, and 832 lbs. weight at the Cammell plate.

BOOK NOTICES.

SHAVINGS AND SAWDUST. By "OBSERVER." Buffalo: C. A. Wenborne.

The care, operation and designing of wood-working machinery form the material for the work bearing the above quaint title.

The author evidently knows what is most desirable to be learned by the inexperienced in this branch of industry, and he certainly has a direct way of imparting the facts and principles which he desires to communicate.

THE ELEMENTS OF THE HELIOGRAPH. By FRED'K. K. WARD. Washington: Signal Office.

The increasing use of heliotrope signaling in extended surveys will serve to create a demand for this little essay among surveyors.

It requires but little mechanical skill to construct a serviceable instrument, and the suggestions of this writer explain sufficiently the necessary dimensions for all practicable limits of surveying.

THE SPECIAL CHARACTERISTICS OF TORNADOES. By JOHN P. FINLAY. Washington: Signal Office.

This is issued in the form of a report of a Signal Service officer to Gen'l Hazen.

The phenomena that precede or follow the tornado, the changes of pressure, temperature and direction of air currents, the electrical manifestations, are all carefully grouped and described.

All students of meteorology will read with interest this essay, prepared, as it is, by an expert, who has been fitted by professional training for the work of collating such data as shall best aid in the solution of one of the chief meteorological mysteries.

THE STUDENT'S HAND-BOOK OF PHYSICAL GEOLOGY. By A. J. JUKES-BROWNE, F. G. S. London: George Bell & Sons.

The author of this compact little treatise explains apologetically that another book on physical geology might seem uncalled for while such works as those of Geike and Green are accessible to students. But he adds, "there does appear to be room for a book of more modest pretensions, arranged on a different plan, and issued at a more moderate price."

The work is divided into three parts: Dynamical Geology, Structural Geology, and Physiographical Geology. The second part seems to be of more immediate value to American students, as it gives a concise classification of igneous rocks.

An excellent book for students.

LEGAL CHEMISTRY. By A. NAQUET. Translated by J. P. Battershall, F. C. S. Second edition. New York: D. Van Nostrand.

This new edition exhibits an addition to the well-known first book in a chapter on Tea and its adulteration. The interest lately exhibited in this particular form of sophistication seemed to the editor to call for a simple and concise method of examination which would include the requisite tests without entering upon an exhaustive treatment of the subject.

The experience of Dr. Battershall in this line of testing in the United States Laboratory renders this supplement valuable.

The Bibliographical Appendix has been considerably extended.

MISCELLANEOUS.

THE following method of coloring soft solder, so that when used for soldering brass, the colors may be about the same, is given by the *Metallarbeiter:* First prepare a saturated solution of sulphate of copper—bluestone—in water, and apply some of this on the end of a stick to the solder. On touching it with a steel or iron wire it becomes coppered, and by repeating the experiment the deposit of copper may be made thicker and darker. To give the solder a yellow color, mix one part of a saturated solution of sulphate of zinc with two of sulphate of copper, apply this to the coppered spot, and rub it with a zinc rod. The color can be still further improved by applying gilt powder and polishing. On gold jewelry or colored gold, the solder is first coppered as above, then a thin coat of gum or isinglass solution is applied and bronze powder dusted over it. which can be polished after the gum is dry and made very smooth and brilliant; or the article may be electro-plated with gold, and then it will all have the same color. Quoting from a German source, the *Scientific American* says; On silverware the coppered spots of solder are rubbed with silvering powder, or polished with the brush and then carefully scratched with the scratch brush, then finally polished.

IN illustration of the tendency of dust to move from hot and to deposit itself on cold surfaces, the following experiments were recently described before the Royal Society of Edinburgh by Mr. J. Aitken:—Two mirrors, one hot and the other cold, fixed face to face and close to each other, were placed in a vessel filled with a dense cloud of magnesia, made by burning magnesium wire. After a short time the mirrors were taken out and examined. The hot one was quite clean, while the cold one was white with magnesia dust. In another experiment a cold metal rod was dipped into some hot magnesia powder; when taken out it had a club-shaped mass of magnesia adhering to its end, while a hot rod attracted none. This tendency of dust to leave hot surfaces and attach itself to cold ones explains a number of familiar things; among others it tells us why the walls and furniture of a stove-heated room are always dirtier than those of a fire-warmed one. In the one case the air is warmer than the surfaces, and in the other the surfaces are warmer than the air. This effect of temperature is even necessary to explain why so much soot collects in a chimney. It explains something of the peculiar liquid-like movements of hot powders, and perhaps something of the spheroidal condition. For practical application, it is suggested that this effect of temperature might be made available in many chemical works for the condensation of fumes, and that it might also be used for trapping soot in chimneys.